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Discussions

Interactive comment on "The influence of non-stationary ENSO teleconnections on reconstructions of paleoclimate using a pseudoproxy framework" *by* R. Batehup et al.

Anonymous Referee #2

Summary: This study employs a GFDL CM2.1 control simulation and a pseudoproxy framework to assess the impact of teleconnection non-stationarity on the reconstruction of ENSO variance. Teleconnection non-stationarity is found to degrade reconstruction skill when using extra-tropical psuedoproxies, particularly when using pseudoproxies suffering from "organized" non-stationarity. Nevertheless, employing large "random" (comprising non-stationary and stationary psuedoproxies) networks was found to significantly limit the impact of teleconnection non-stationarity on reconstruction skill.

General Remarks: This represents an interesting contribution to our understanding of the impact of non-stationarity on reconstruction skill. The manuscript is, in general, thorough and well thought out. There are, however, sections that are potentially con-

fusing and that need clarification. These comments are largely minor.

Comments:

Introduction: The last paragraph leaves the reader thinking that teleconnection non-stationarity does not impact reconstruction skill. This is not the case and I would start this paragraph by noting that non-stationarity does degrade reconstruction skill. You can then follow that by noting that the impact of non-stationarity can be minimized by employing a large global network.

Page 3854, Line 15: add variance after ENSO. More generally, this is a problem throughout the manuscript. Make sure to be clear that these are reconstructions of ENSO variance. I will note places where this should be clarified but have likely missed some.

Page 3854, Line 27: This sentence seems tangential to the overall results and perhaps overly specific.

Page 3855, Line 11: Suggest removing: "Thus, these proxies are the essential tool for creating paleoclimate reconstructions."

Page 3857, Line 28: Suggest removing: "This places increasing stress on the assumption that teleconnections are stationarity. Further to this, it."

Page 3858, Line 4: I would replace variability with variance to be absolutely clear that that is what is being reconstructed.

Section 2: You are only using a single model and it will be important to note that PPE results have been shown to be model dependent, at least in the case of CFRs (e.g. Smerdon et al. GRL 2011 and Smerdon et al. Clim. Dyn. 2015). This is particularly important given that not all models have non-stationary teleconnections to the tropical Pacific (e.g. Coats et al. GRL 2013). Using a different model may provide different results, so making absolutely clear that the results are specific to the characteristics of this GFDL model and not necessarily applicable to the real world will be important

(after all, a model with either stationary teleconnections or much more non-stationary teleconnections is arguably an equally plausible representation of the real world).

Page 3860, Line 10: Does June–July imply a two month average? Based on the rest of the sentence I assume you mean a 13 month average. Please clarify.

Page 3860, Line 11: The sentence on computational cost seems unnecessary.

Page 3861, Line 4: The reference to Lee et al. (2008) seems out of place because adding non-climatic noise at different levels is a relatively standard choice in PPEs. Perhaps restructuring to put the reference at the end of the sentence with a parenthetical note that Lee et al. is an example of this. The reference is also not listed in the references.

Page 3861, Line 11: Suggest removing "to some extent, making them at least partly relevant for reconstructing the ENSO signal."

Page 2861, Second paragraph: Are 1000 random networks of each size from three to 70 used?

Page 3861, Last paragraph: It is important to note that a real reconstruction will only be able to calibrate on the observational record. The first sentence, however, seems to distract from this important point–perhaps try rewording.

Page 3863, Line 5: The last sentence is unclear and doesn't seem necessary.

Page 3863, Line 8: But you do show results from these experiments in the other figures. Perhaps remove this sentence.

Page 3865, Line 8: Do you mean segments or windows and not years? A year can't be non-stationary but the 31-year window, for instance, can. Or are you counting up the number of years within all the non-stationary windows? That would be much less intuitive. If it is the former I would suggest changing years to windows or segments and doing the same in the corresponding figure and caption.

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Page 3868, Line 21: I think that you mean the running variance of the Niño3.4 index.

Page 3869, Line 13: Perhaps provide a value in parentheses here (after larger network sizes).

Page 3869, Line 17: Again change variability to variance.

Page 3870, Line 11-14: How are the psuedoproxies non-stationary if they display little variability in correlation, that is not intuitive.

Page 3870, Line 19: I understood what you were saying after reading further but was confused by this sentence initially. Perhaps try to make the statement more clear.

Page 3871, Second paragraph: I found this confusing. I think what you are saying is that if the network consists of a large proportion of grid-points chosen due to spurious correlations in that calibration window, the reconstruction skill is very low. The statement: "non-stationarities at the same time" might be part of the problem. A grid point is either non-stationary or not, but if it is non-stationary and weakly correlated it won't always be eligible to be picked and that appears to be what you are getting after.

Page 3873, Line 10: The result that increasing the length of the calibration window is less important for reconstruction skill as compared to the choice of method or the amount of non-stationarity is important and gets lost a bit in the manuscript.

Page 3874, Line 7: "correlations to ENSO variance." It might be worth changing everything to ENSO variance or everything to Niño3.4 variance throughout the manuscript for clarity.

Page 3874, Second paragraph: The discussion of RMSE versus correlation for MRV seems unnecessarily drawn out (and slightly confusing). The takeway appears to be that the scaling of the variance for he MRV method is too low but the timing of variance changes are correct.

Page 3877, Line 2: Coats et al. GRL 2013 showed model dependence of non-

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stationarity to ENSO so this statement isn't strictly correct.

Page 3877, Line 3: Suggest removing virtual.

Page 3877, Line 26: To this reader it was not obvious why the filtering produced these different interpretations.

Page 3878, Line 1: The point here is not that multi-proxy networks will produce more informative reconstructions, it is that larger and more global networks will. Maybe flip the sentence structure so that in the back part you can explain that multi-proxy networks tend to be larger and more global.

Figure 1, Caption: On Line 7 remove correlation coefficients.

Figure 2: As noted above, years or segments. Segments would be much more intuitive. Colorbar has no label.

Figure 10, Caption: put tilda in Nino3.4.

Figure 10: I find this figure hard to interpret in the context of the results. The blue line is the standard deviation of the correlations for a 30 year window, where the correlations are between a pseudoproxy and the actual Niño3.4 index. I don't see the relevance but perhaps I am misunderstanding what is shown. The red circles are just a very small subset of the plot, is this relationship consistent? In any case, this figure and the discussion would benefit from further clarification.

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