

Interactive comment on “A pseudoproxy assessment of data assimilation for reconstructing the atmosphere-ocean dynamics of hydroclimate extremes” by Nathan Steiger and Jason Smerdon

Anonymous Referee #2

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The paper reports on a pseudoproxy study designed to assess the possibility to reconstruct hydroclimatic variability using a data assimilation approach. The approach as well as several other aspects are described elsewhere (they use CESM simulations as background and proxies as observations - in the pseudoproxy study they use forward models to generate pseudoproxies), but here the focus is specifically on hydroclimatic variables.

The results are scientifically sound and the paper is well written. It is interesting for the community. However, the paper clearly gives a very optimistic upper bound. While

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this is certainly important, it might be more useful to the reader to give a more realistic view. Where does the method break down? What are the limitations? It is hard to judge for the reader, particularly since several optimistic assumptions are only implicit and a discussion of the limitations is missing. While I don't think that the authors should add further analyses, I definitely think they should (1) be more clear about the assumptions and (2) add a discussion on the potential limitations when applying it to the real world.

Specifically, the methods assumes that there is no model bias, that the model B matrix represents the true covariance structure. It is not clear how the residuals for the pseudo-proxy are calculated and whether H is assumed to be known perfectly (or are the parameters of H degenerated, or is the VS-lite recalibrated somehow with the pseudo-proxies?). Is R taken as diagonal? What noise model is used? And is R assumed to be known perfectly?

These assumptions are very optimistic (even more optimistic is the low assumed error), and it is hard to judge how important it is. Teleconnections are assumed to be perfectly represented and linear and they are assumed to be stationary (constant in time, e.g., independent of forcings).

Another worry I have, in the examples given, is that the NINO index is specified with the same tree ring width series as the drought indices. Hence, the NINO index and drought are expected to be related as they are specified from the same tree ring width. Actually, this is seen in Fig. 9, which shows a stronger ENSO signal in the reconstructions using only tree ring width than in the "true" simulation. There is an element of circular reasoning here. In this context, localisation should be discussed. The author speak of localisation as an "ad hoc" method, but controlling this sort of circular reasoning would be one advantage that the authors should consider.

Minor

P. 3, l. 10: Give a reference for the Ensemble Square Root Filter implementation.

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P. 3, l. 29: I think the boundary conditions require some further explanations. Is it sensible not to consider boundary conditions? Or would physical consistency be violated (e.g. by using a non-volcanic background during a volcanic year?)

P. 6, l. 11: The amount of noise is really small; I am surprised by that.

P. 6-7: From the statement that a monthly NINO index was reconstructed I take that x_b and x_a contain both monthly and seasonal variables. How about the annual ones? Are annually and seasonal variables in the same state vector? Or are these two different experiments?

P. 7, l. 23: If possible within reasonable length, give equation and references for CRPSS.

P. 9, l. 4: It is not fully clear how the limitations were derived. As I understand the approach, the actual limitations are dependent on the climate conditions (couldn't the same VS-lite parameters make a tree moisture sensitive in one year but temperature sensitive in another one?).

Fig. 2 is interesting. In the upper row (which is not really discussed in the text), the high correlations over Antarctica are striking (this could be relevant for other reconstructions). Also in the second row I find the very high skill in the tropics remarkable (the authors note it, but I think it requires more explanation). Also, it is interesting that the skill in the annual mean is smaller over the proxy sites than over the adjacent oceans. This is due to winter, where there is no skill over the proxy locations but (due to thermal inertia or other memory effects) some limited skill over the ocean. Also, in the annual mean there seem to be prominent patterns (dipoles?) in the N- and S-Pacific, pointing to very stable teleconnections within the model world.

P. 12, l. 27: I presume that the assimilation is the same as above - or not?

P. 14, l. 3: In addition to El Niño, a look at the Atlantic Ocean might be interesting.

P. 14, l. 12-14: I find this conclusion a rather dangerous one to make in a perfect-model

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set-up.

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