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Climate of the Past Discussions

Interactive comment on "A novel approach to climate reconstructions using Ensemble Kalman Filtering" *by* J. Bhend et al.

Anonymous Referee #3

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review of 'A novel approach to climate reconstructions using Ensemble Kalman Filtering' by Bhend, Franke, Folini, Wild and Brönnimann.

The study introduces an existing data assimilation technique to the paleo climatic sciences and shows that this method, based on the Ensemble Square Root Filtering, does produce a simulation that reflects (part of) the information contained in the pseudo-proxies and is dynamically consistent with the assimilated data.

The language in the study is fine and the subject is suitable for *Climate of the Past* and has all that it takes to make a fine contribution to an exciting newly developing part of the field. However, there is one big problem with this study (and a few smaller ones):

the authors seem to assume that the audience of this journal are specialists at EnKF techniques. The explanations of the method are too terse to be comprehensible for a non-specialist.

Next to this issue, there are some points related to the method that the authors do not discuss but are relevant for the paleoclimatic community in order to be able the weight the different assimilation methodologies which are now used in the field.

I think that rewriting the ms. can be done by this able team of researchers but it will not be a small task. My advice to the editor is the accept this ms. with *MAJOR* revisions.

Major points

- Readers not familiar with the basics of EnKF have stopped reading after the first few paragraphs of section 2.2, and that is a pity. Please be a little more expansive on the concept of EnKF. This allows a more gentle introduction of the covariance matrices P^a and P^b and the gain matrix K. Preceding the mathematical explanation of EnSRF, a conceptual explanation would be nice and would guide the reader through the mathematics. An small example that the authors are not kind to the reader is on line 14 of page 2842. Why not explain that 1342 is related to 2*694+4 (temp. and precip. at 694 locations plus 4 indices).
- In section 3 we read on the first line about the 'covariance localization' which presumably relates to equation 5. Unfortunately, the terminology is not introduced at eq. 5. Given the importance of localization to the results of this study, a less terse and more informative introduction seems to be in place.

Other points to look into

- The pseudo-proxies used are of inter-annual resolution. The timestep of the model is much smaller than this timescale as well as the typical timescale of synoptic systems. At every timestep of the simulation, the EnSRF method takes the

simulation to the pseudo proxies. The authors show that this leads to a reproduction of the pseudo-proxies in the simulations, but what does it do to the variability of the model on the timescales of the synoptic systems? The authors rightly claim that EnSRF has been successfully applied to a reanalysis study, where 6-hourly data are assimilated - no problem there in any suppression of internal variability. The authors should discuss the effects of their method on the level of synoptic scale variability in their data-assimilated simulations (like the activity/position of the storm tracks).

- page 2838/2839: I don't really understand the reasoning why the corrected states need not be fed back into the system to give the next simulation. I see that the timescale of the pseudo proxy is much larger than the typical timescale of atmospheric processed, but the AGCM surely has a land surface model, with soil moisture and snow cover, and a swamp ocean as boundary conditions that introduce long timescales in the model. This also relates to the discussion on page 2849, lines 5 and 6.

Interactive comment on Clim. Past Discuss., 7, 2835, 2011.