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Climate of the Past
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Interactive Comment

Interactive comment on "Multi-time scale data assimilation for atmosphere–ocean state estimates" *by* N. Steiger and G. Hakim

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This is an interesting manuscript that explores the utility of proxy data which represents averages over different time scales in estimating the climatological state. I'd certainly like to see the results published, and have some suggestions that the authors may like to consider. However, there is also one possible problem that may be major and must be addressed.

The major issue is the following: if the "truth" simulation used as the source of the pseudoproxy observations was also included in the prior ensemble (as appears to have been the case according to the description), then the algorithm might be doing little more than homing in that single transient run (which will be easily identified given sufficient observations). This is useful perhaps as a test of coding correctness but not

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for generating scientifically useful results. If the authors did include the interval from which pseudoproxy data were observed, in the prior ensemble of chunked outputs for that given year, then these experiments will need to be repeated, preferably excluding all overlap between the true N-year interval from which the observation is drawn, and the N-year prior ensemble members.

Additionally, I'm not sure that the method is quite as novel as the authors seem to claim. State space augmentation has long been known as a way of handling arbitrary observation operators. Tarantola (1987) seems to be the canonical reference to the theory here, but I admit I have not read it. Anderson (2001) also discusses it more specifically in the context of ensemble Kalman filtering. While most applications of this approach have probably been with contemporaneous observations, my early work in this area focussed heavily on the use of time-averaged observations in order to estimate not only parameters but also the states of models of various complexities, with Annan et al (2005) being perhaps the first application (albeit in a rather different context to that of the authors) that combined state and parameter estimation in a GCM using climatological observations. I believe that the authors' approach in calculating N-year averages of each trajectory and then updating using observations is formally equivalent to augmenting the ensemble members with their respective N-year averages. However, the application and results are still interesting in their own right. Another issue with the off-line ensemble method is that forcing is no longer relevant or correct. Or perhaps more precisely, some of the ensemble members will have appropriate forcing but the vast majority will not. It would be technically straightforward to identify the (mean) forcing corresponding to the posterior estimate - does this bear any relationship to reality, e.g. are there volcanoes at the right time?

I also have some suggestions for minor additional analyses that may help in the understanding and interpretation of their results. The main results would be enhanced by showing the uncertainty on the analysis posterior - this would show if the analysis is consistent in this respect, and would also illustrate if and when filter collapse might

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be occurring. A further useful point would be to explore the spatial performance of the algorithm. Annan and Hargreaves (2012) found that the global mean temperature could be well represented by the posterior even when the spatial pattern was not. Since these sort of data assimilation methods are often used to diagnose and interpret regional climate variability it is important to understand how well they perform at this task. It is possible that the ensemble Kalman filter performs better here than the particle-based methods that have been more widely used, so I'd like to see some more analysis of this. Furthermore, the limited success in recovering the AMOC could either indicate that the assimilation is failing to recover spatial patterns of surface temperature well, or else that the AMOC is not tightly constrained by spatial patterns of surface temperature - which if true would be a stronger conclusion than that currently given in the manuscript.

Refs:

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