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Interactive comment on "A reduced-order Kalman smoother for (paleo-)ocean state estimation: assessment and application to the LGM" *by* Charlotte Breitkreuz et al.

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For a number of reasons I've found it difficult to obtain two reviews for this manuscript. To avoid further delays I am taking the slightly unusual step of acting as reviewer despite also being Editor.

The manuscript presents an interesting method for parameter estimation in an ocean model, with application to paleoclimate. The paper is in scope for GMD and with some revision it could potentially be published.

My main concern is with the details of the finite difference approximation, which seems

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a bit arbitrary (at least based on the presentation). Why were three perturbations used, and selected randomly? The size of sigma/100 seems very small. There is clearly a compromise here due to model drift/disequilibrium/internal variability which will vary with application. Larger perturbations would provide a more robust estimate and if the response is nonlinear then wouldn't it be better to use a more realistic estimate of the average response over a larger increment anyway, since you are in fact applying substantial increments in your optimisation? If you can really get away with a small finite difference approximation then even a single perturbation would have sufficed, but the information from smaller and large perturbations could contain useful information regarding the nonlinearity (and/or the accuracy of the scheme in estimating the true response). For example, with 3 samples, you could have chosen perturbations of +-sigma/2 and +sigma/100 which would have given both the local tangent and also some indication as to the range of its validity. For a journal such as GMD I'd like to see more analysis of how the method works, ie how collinear the sample points are, and also how well the predicted optimum performs in comparison to expectation at the update time.

The comparison to an adjoint is a little harsh in places. There are various ways of ensuring smooth forcing fields with that method, including the state space reduction you have applied here, or also some regularisation as an additional constraint on the forcing fields. An additional limitation of the method you have presented here is that in the case of strong covariances between your control variables, any optimisation may be very slow and inefficient as you are only evaluating the orthogonal directions (interactions would require O(pˆ2) simulations). Therefore, the choice of control variables is probably very critical and the method will have limited applicability.

The state space reduction is a good idea, I'm not sure to what extent it may be novel in this area. It would be useful to have a little more in the way of references and/or discussion here (eg explaining the choice of Legendre polynomials).

In Figure 1 (left) I'd like to also see the optimised results which could be presented as

deviations from target in the case they are invisibly small.

I think the results in Table 2 might be more readable if presented graphically, perhaps some sort of scatter plot after nondimensionalisation. Eg deviations from the true value. You could use colours, symbols, line types etc to distinguish them.

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