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Geoscientific Model Development Discussions
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# *Interactive comment on* "Calibrating Climate Models Using Inverse Methods: Case studies with HadAM3, HadAM3P and HadCM3" *by* Simon F. B. Tett et al.

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This paper studies technical aspects of the calibration of parameters in a climate model using a range of observations. It extends previous work by including more parameters and more classes of observations. Its main concern is whether the process is technically feasible, that is whether the minimisation algorithms employed to find the maximum likelihood estimate of the parameters can converge and whether the converged values are reasonable. The paper is certainly in scope since it studies an important problem in climate science and investigates technical aspects of that problem.

I believe the paper needs substantial work before it can be published but it is possible that I am misunderstanding something quite simple about it and hence my concerns



might be irrelevant. At a practical level my concern is the temporal frequency of the observations being fitted. I didn't see this quoted in the text, presumably it is noted somewhere. In two extreme cases this will pose different kinds of problem for the paper.

1) High-frequency observations (e.g. daily) are used. In this case the sensitivity of the simulation and hence the cost function to the parameter is nearly arbitrary. A given simulation is one representation of the deterministic chaos of the model. The same perturbation in the parameters with a perturbed initial condition (correctly not included in the parameter estimation) might produce quite different sensitivities. The perturbation in the parameter presumably shifts the mean state of the simulation somewhat but the projection of this mean onto the time series might be very hard to see. In this case the gradient suggested by the derivative of the cost function might be a poor predictor of what happens when one actually searches in this direction. This looks like it might be happening but not for any technical reason but rather that the cost function is dominated by variations unpredictable by small parameter variations. This is a fascinating problem: What parts of the manifold in a chaotic system are legitimate targets for assimilation.

2) The other extreme case is that only long-term and large-scale observations are used, perhaps one observation per class. This would circumvent problem (1) but yield a quite different problem where the parameter estimation is under-determined. In this case we are back in the realm of conventional data assimilation where the use of prior information acts as regularisation as well as providing proper scaling etc for the parameters. Note that the authors are implicitly using some prior information by limiting the search space, it would be better to include this information within the probabilistic description of the problem (e.g. Tarantola 2005).

So, I'm not sure which or even whether these problems apply and clearly the authors need to describe their observational dataset more clearly but either way I believe some more work is needed.

There is also, I believe, one serious misunderstanding of the parameter estimation problem which has caused the authors to skip a step they actually can't avoid. On page 5 the authors state that it's not their problem to compute observational uncertainties which must come from those who generate the observations. I don't think this is correct. The observational uncertainty in a conventional estimation problem like this actually combines the error in the observation (difference between measured value and true value) and the difference between what the model should simulate for a given value of its inputs and what it actually does simulate. Here the inputs are parameters so the error likely concerns structural errors uncorrectable by any parameter setting. This is a task for the modeller and, unfortunately, not an easy one. In many problems like atmospheric inversion these model errors dominate the observational component. The authors should dsicuss and, possible, quantify this.

I also believe the authors need to talk some more about uncertainties in their parameters. Information on this is available from error propagation via the Jacobian from the observational covariance. This might be a simple explanation for the apparent equifinality.

Given these rather general concerns about the paper I will await a response before more detailed comments on the text. One concern that may affect any recalculations the authors may choose to do is the comment on page 8 about making sure the covariance is invertible. I agree this must be done, covariance matrices should be positive definite but wonder how singular matrices can appear in a correctly specified problem. Some covariance structures can yield near zero eigen-values but that should not be the case here.

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