

Interactive comment on “Estimation of ECHAM5 climate model closure parameters with adaptive MCMC” by H. Järvinen et al.

Anonymous Referee #2

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This paper offers an example of parameter calibration in climate models. Specifically the calibration exercise considers four parameters that relate to clouds and precipitation which are sampled via adaptive Metropolis methods for the ECHAM5 model. The authors consider alternative objective functions that are 'tested'. The paper is clearly written and could make an interesting contributions for this journal if some revisions are made.

1) The authors claim that this is the first time that has been demonstrated that MCMC is viable for calibration of GCMs. Clearly, the paper of Jackson et al. (2008) in Journal of Climate makes a first consideration to this issue with the CAM 3.1. and via a multiple/optimization type of sampling. For this context, Villagran et al. (2008) makes the point that versions of Adaptive Metropolis could provide a more appealing sampling

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strategy that reduces biases. There is no discussion in detail of these other references in the context of what the authors are trying to achieve for the ECHAM5 model.

2) One of the main contribution of this paper seems to be the formulation of several objective functions and to establish a comparison between them. However, the paper only provides narrow evidence about the question of how to choose an objective function. In general, the discussion around this (section 4.2) is rather unclear and vague compared to other sections of the paper.

3) It is somewhat deceiving that the paper does not provide posterior distributions of parameters since there is an 'apparent drift' on parameter values in Figure 2. If the MCMC needs to be run longer and some convergence issue is apparent here, what is it really gained by the current analysis? Or should we think of this study as rather preliminary? For example, Villagran et al. (2008) consider the effect of sampling assuming only 500 algorithmic iterations were available. This should be discussed in the paper. I think it is really worth showing posterior distributions at least to understand better the limitations of the current results and to gain perspective on how much longer does the MCMC needs to be run to achieve trustworthy results. In regards to Table 2, why not show the corresponding posterior estimates and standard deviations for each of the objective functions that were considered along with what is currently presented there? This could allow readers to understand better some of the different biases and eventually recognize the best objective functions, which is one of the main goals of this work.

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