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**Geoscientific
Model Development
Discussions**

Interactive comment on "An optimally tuned ensemble of the "eb_go_gs" configuration of GENIE: parameter sensitivity and bifurcations in the Atlantic overturning circulation" *by* R. Marsh et al.

Anonymous Referee #2

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This manuscript presents the results of a tuning exercise in which 13 parameters are varied and 5 pareto-optimal sets of parameters are presented and recommended for future use. The manuscript is generally well-written and I consider it appropriate for publication in GMD. I have a number of rather minor suggestions for the authors to consider.

I realise this paper is not claiming to make any particular methodological breakthrough, but it seems a little misleading that the advantages of the method used are described only in comparison to the Latin Hypercube approach of Edwards and Marsh: the cited

Price et al 2009 has already described it as having comparable performance to two other efficient methods that were previously used (Proximal ACCPM and Ensemble Kalman Filter). Additionally, part of the methodological description seems misplaced at the start of the results section.

The wording on p934 l10- is hard to understand. Please make clear on l10 that you are actually talking about the cost functions of Tocn and Socn rather than the variables themselves (as you clarify later). The claimed correlation does not seem at all clear to me, and the same applies to Tatm vs Tocn (but Qdry vs Socn is evident). Also, I don't understand the distinction between "correlation" and "competition" made in this paragraph. Is competition here a negative correlation - and then "correlation" refers to a positive correlation? That seems even less plausible for Tocn vs Socn. I conclude that I don't know what you mean.

It would be useful to mention how the GMD11 set performs against the cost functions used here. Also, surely it was optimised somehow - its performance seems rather good to have been selected arbitrarily.

The fine resolution sampling in the neighbourhood of one point is probably the highlight for me in this work. I suspect the underlying explanation is that the model has two stable solutions in the region of the transition, with the selection of on or off being a quasi-random response to the initial shock. Is this also the authors' interpretation? This would be easily testable with a different set of initial conditions, which I would expect to give a different pattern of switching (with a similar overall appearance).

It would be nice to see XML files for the 5 parameter sets uploaded as part of the paper (SI) rather than relying on the stability of a personal web page.

Interactive comment on Geosci. Model Dev. Discuss., 6, 925, 2013.