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Geoscientific Model Development Discussions

Interactive comment on "An iterative process for efficient optimisation of parameters in geoscientific models: a demonstration using the Parallel Ice Sheet Model (PISM) version 0.7.3" *by* Steven J. Phipps et al.

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The following is a review of a Geoscientific Model Development manuscript on "An iterative process for efficient optimisation of parameters in geoscientific models: a demonstration using the Parallel Ice Sheet Model (PISM) version 0.7.3" By S. J. Phipps et al.

This manuscript describes a systematic brute-force statistical approach to determining the most realistic combination of values for multiple parameters of a

geoscientific model. The authors adopt an iterative sampling procedure in which they continuously down-select from a collection of ensemble members until they reach their criterion for convergence. This procedure allows them to identify the areas of the multi-dimensional parameter space that will result in the most realistic model outcome. For this paper, the authors use the Parallel Ice Sheet Model (PISM) to illustrate the utility of their approach, and they find 14 different configurations that best match with observational data. In conclusion, however, they are not able to identify a truly optimal set of parameter values due to computational limitations and the fact that a number of the variables being explored co-vary. They suggest that ice sheet models may not be able to be tuned to a truly optimal state, and that model complexity and non-linearity demand the use of ensemble modeling in order to adequately quantify model uncertainty due to variable tuning.

Overall, this manuscript is well-written and comprehensive. The theme is appropriate for GMD, especially since the outlined approach can be used for exploration of other types of geoscientific models. The methods are sound, and the authors do a good job describing the ice sheet model experiments. The figures illustrate the findings adequately, and the conclusions are well-founded, especially given the described caveats and challenges associated with using a state-of-the-art ice sheet model. As a result, I recommend acceptance for publication to GMD, after minor edits.

We thank the reviewer for their positive feedback and for their constructive comments on the manuscript.

Below, I outline a few comments/suggestions for the manuscript:

Line 77: Please rephrase. Evaluate is used twice and the wording is confusing as I am not sure what "the ability of a model to evaluate multiple different states" means.

Our intention is to refer to multiple different states of the physical system being modelled. In the case of a model of the Antarctic Ice Sheet, such as in the current study, this might refer to distinct states such as the Last Glacial Maximum and the present day.

We will revise the text of the manuscript to clarify this point.

Line 197: Please define F here. In the PISM User's Manual I think they define it to mean a function, but please specify here.

F is a function that describes the flow law. We will add this to the text.

Line 255: Can the variable distribution referred to at this point be described as uniform (as opposed to normal for example)? Please specify in the text.

We consider that the parameters studied in the manuscript are insufficiently understood to enable any robust assumptions to be made in regard to the distribution of prior probabilities. As such, we consider that the simplest possible assumption (i.e. uniform) is the most appropriate.

We will revise the text of the manuscript to expand upon this point.

Line 256: I think this means that for each of the 100 ensemble members, all 10 variables are perturbed independently, and then one ensemble member is run. Please rephrase this part of the procedure so that it is clear to the reader.

The reviewer is correct. We will revise the text of the manuscript to clarify this.

Line 256: Based on comments later in the manuscript, you are aware that 100 ensemble members for variation of 10 variables statistically not enough to fully characterize the parameter space. Of course, a lot of work went into just running the 100 members presented here, so I am not suggesting that you run more. However, while this caveat is not completely ignored by the authors in the manuscript, I would like to see it specifically addressed either here or in the

discussion (i.e. where the need for more systematic, larger ensembles is discussed).

As in the response to Referee #1 we acknowledge that, when applying the technique described in the manuscript, there is a trade-off between expense (ensemble size) and precision (reduction in parameter uncertainty). We also acknowledge that an ensemble size of 100 is not sufficient for us to fully characterise the properties of a ten-dimensional parameter space.

We will add text to the revised manuscript to emphasise these points.

Line 343: Awkward use of "from" twice. Please rephrase.

Thank you for spotting this typographical error. We will remove the first instance of "from" in the revised manuscript.

Line 359: Please specify what smaller means in this context. I think you refer to the area extend of the ice sheet, but it is not clear.

The reviewer is correct. We will revise the text of the manuscript to clarify this.

Line 370: I am curious as to why you did not also choose to use surface velocities as an observational constraint. I realize that since the ice sheet is in balance, the thickness profile encompasses velocities in a way, however, I would be interested to see results showing that the use of thickness and mask is adequate on its own. The choice of which of these constraints is chosen to be uses actually could, in itself, contribute to uncertainty. I think is worth adding some sentences addressing this in this discussion section.

We agree. We will add a description of this point to the revised manuscript.

Lines 375-377: Agreed. This is especially the case because model response will become even more convolved when model forcing is not held constant (i.e., transient forcing through time). This is a potent point. Could you expand upon it

with 1-2 sentences that give a concrete example of why this is the case (so that a reader can clearly discern how you make the jump from your results to this claim)?

We agree and we will expand upon the important points raised in this paragraph. We do not provide future simulations in the current manuscript. Therefore, to provide a concrete example, we will cite and discuss the results of DeConto and Pollard (2016) and Edwards et al. (2019), which demonstrate that parameter uncertainty and ensemble design influence the probability distributions for projections of future sea level rise.

DeConto and Pollard (2016): https://doi.org/10.1038/nature17145

Edwards et al. (2019): https://doi.org/10.1038/s41586-019-0901-4

Lines 378-380: This is a very strong statement. While this claim may be accurate, I do not see how it is shown in this manuscript. That is, the results shown in this manuscript do not directly illustrate how the approach shown here explicitly quantifies parameter uncertainty and that this derived uncertainty is propagated into projections. The experiments only derive uncertainty in model steady-state spin-up due to chosen, appropriate spreads chosen for key model variables. I think this is what you are trying to say in this paragraph, but I think it can be said in a more direct, clear way to the audience that your approach can be expanded to projections using ice sheet models, but will require expanded ensembles and further investigation into the model system. Expanding on why this is true with a couple of sentences would strengthen your final discussion point.

We agree and we will revise the paragraph in accordance with the reviewer's suggestion. In particular, we will emphasise that further work, involving large ensemble modelling of future changes, would be required in order to explore the role of parameter uncertainty in future projections.

Lines 399-400: With respect to my last comment, this statement does a good job

of summarizing what I think you are trying to say at the end of the discussion. Leading the reader to how and why you make this conclusion is exactly what I am hoping you can accomplish in the last paragraph of your discussion.

Thank you. We will follow this suggestion when revising the final paragraph of the discussion.

Figures 1 and 2: In addition to these, plots of the mean and maybe standard deviation (or uncertainty) spatially, derived from the surviving ensemble members could also be a helpful way to summarize your results.

We agree that this would be valuable information. The standard deviation would be the same for both Figures 1 and 2, as the only difference between the two figures is that the Bedmap2 topography has been subtracted from the simulated topography in Figure 2.

We will, therefore, remove the bottom-right panel from Figure 2 (which simply replicates the same panel in Figure 1). We will instead add two new panels showing the mean and standard deviation of the model error.

Supplemental Material: I am not sure myself, but is it appropriate to include the PISM user's guide as supplemental material? It already has a copyright and its own set of authors.

We consider that it is helpful to the reader for us to provide the version of the manual that documents the precise version of the model described in the manuscript.

The PISM manual is distributed as part of the model source code and is updated with each new release. It must also be compiled. As such, the only way that the reader could otherwise obtain the correct version of the manual would be for them to download the precise version of the model source code and to compile the documentation themselves. Under the circumstances, we think it is helpful for us to provide them with a PDF.

However, we acknowledge the copyright issue. We will contact the PISM Authors and the Handling Topical Editor of Geoscientific Model Development to confirm that it is acceptable for us to distribute a copy of the manual in this fashion.

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