Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-382-AC1, 2021

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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Received and published: 5 March 2021

General comments:

This paper presents a procedure of refining large ensembles (LE's) of geophysical model simulations, iteratively narrowing parameter ranges based on fits to observations. The core of the paper is the statistically based method of refining each parameter range, Eqs. 10-12. The procedure is tested using modern simulations of the Antarctic Ice Sheet with the PISM model, run to equilibrium with

modern climate, and testing on continental average fits to observed ice extents and thicknesses.

The procedure is put into perspective and is well motivated in a particularly broad-based introduction. It is well considered and should be a useful tool in many large-ensemble studies. It is explained succinctly and clearly, and the results presented for the PISM application illustrate the potential and limitations of the method.

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

Specific comments:

As shown in Table 2, after 5 iterations the reductions in parameter ranges from those at the start do not seem very impressive - considerably less than 50% in all cases. The same is true for the reductions in error metrics Ecrit_A and Ecrit_V. I think the procedure could still be useful for the community, but perhaps with some caveats along these lines.

We acknowledge this point. Greater reductions in the parameter ranges might be possible if the ensemble sizes were larger. When applying the technique that we describe, there is a trade-off between expense (ensemble size) and precision (reduction in parameter uncertainty). We will add text to the revised manuscript to emphasise this point.

As an alternative to Eqs. (10) and (11), these could be combined into a single step by considering: p = ((Xmax - Xmin)/(XB - XA))^N. Could there be any advantage to this, compared to the separate consideration of the max's and min's in the paper? There is none that I can see, but it might be interesting to mention briefly (or to rule out). The basic concept of refining parameter ranges in an iterative series of LE's is analogous to Lee et al. (2020), who use a new method of "re-

sampling" parameter values at given steps within MCMC (Markov Chain Monte Carlo) sequences. However, the procedure here is considerably less complex and will be more accessible to the cryospheric modeling community.

This is theoretically possible. However, in the case where the null hypothesis can be rejected, this combined approach would not allow us to determine whether it is XA, XB or both that should be changed: it would merely allow us to determine that the parameter range should be reduced. As such, we consider that Equations 10 and 11 should remain separate.

However, we will add a reference to Lee at el. (2020) to the manuscript.

The large number of parameters (10) and the relatively small number of simulations (100) in the Latin HyperCube (LHC) ensembles here is a concern. Chang et al. (2014) found that coarsely spaced LHC sampling inadequately resolves parameter ranges and interactions in LE's for high-dimensional parameter spaces (their Fig. 4). One alternative in future work could be to greatly reduce the number of parameters, still with LHC's or possibly performing runs with all possible combinations of parameter values.

Thank you for bringing this reference to our attention. As acknowledged above, the application of the technique that we describe involves a trade-off between expense (ensemble size) and precision (reduction in parameter uncertainty). We gratefully acknowledge the reviewer's suggestion and will add a discussion of this point to the manuscript, as well as bearing it in mind for future work.

The sentence of line 368-370 may be questioned: "We have also shown that regions of parameter space exist that cannot be meaningfully reduced in volume any further, given constraints arising from the availability of data and limitations on our understanding of the underlying physical system." "Meaningfully" here relies on the assumption in line 299 that the top tercile of ensemble members identifies "good" vs. "bad" simulations. That is reasonable, but the chosen frac-

tion (1/3, vs. 1/4 or 1/5 say) is fairly arbitrary and possibly model dependent. Also considerably more observational modern data than ice extent and thickness are available for validation, e.g., surface ice velocities and ice temperature profiles in deep cores (and also paleo data as in some other studies referenced here).

We agree. We will revise the text of the manuscript to make a more nuanced statement and to expand upon this point.

Some of the sentences in the concluding sections sound negative concerning the ability of LE's to address probabilistic ranges of parameter values and future-projection results (lines 379-380), or whether it has been addressed before (lines 399-400). That ability may indeed be limited due to other sources of uncertainty (lines 382-383), but is prominent in some LE studies, including MCMC applications (Chang et al. 2014; Edwards et al., 2017; Gilford et al., 2020; Lee et al., 2020). Perhaps the extent of these points could be clarified.

We agree. These statements are intended to refer to the application described in the manuscript, rather than to refer to all possible techniques for parameter optimisation. We will revise the text accordingly, including adding a discussion of the references that the reviewer has brought to our attention.

Detailed points:

Lines 7-8: In the abstract, the sentence "We find that co-dependencies between parameters preclude the identification of a single optimal set of parameter values" might be misunderstood. Does it mean "the identification...is impossible with any technique", or "it cannot be done simply, for instance by varying just one parameter at a time, and so needs LEs" ? This is addressed but not quite clearly answered for me on lines 389-393.

As per our response to the previous comment, this statement is intended to be specific to the technique described in the manuscript. We will revise the text of the manuscript

accordingly.

Lines 284-288: Does Eq.(8) distinguish between floating ice and grounded ice? i.e., if a model grid point has grounded ice and observed has floating ice, or vice versa, do the masks M agree or disagree? I would think they should disagree, as suggested by line 287, but perhaps not from line 288(?).

Equation 8 does not distinguish between grounded and floating ice, although we acknowledge that there would be potential benefits to this. We will revise the text at lines 286–291 to clarify this issue.

Line 325: "Convergence is achieved..." may be questioned. Only one parameter minimum or maximum changes in the last iteration (Table 2), but only one has changed in most of the previous iterations as well. If one more iteration was performed, would no parameter ranges change?

At the final iteration, the statistical tests described by Equations 10 and 11 do not result in a rejection of the null hypothesis for any of the ten parameters. As such, if one more iteration was to be performed, it would repeat the previous iteration exactly. We would therefore expect to obtain the same results as the previous iteration, with no further parameter changes.

One minor proviso relates to the issue of model reproducibility. PISM simulations are bitwise reproducible, if run on the same number of processors, on the same machine and using the same executable. However, if these conditions were not satisfied, then we cannot rule out the possibility that a further iteration might result in further parameter changes.

We will add a discussion of these points to the revised manuscript.

References:

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