

Interactive comment on “Spatial probabilistic calibration of a high-resolution Amundsen Sea Embayment ice-sheet model with satellite altimeter data” by Andreas Wernecke et al.

Anonymous Referee #1

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This paper presents a new approach to probabilistic forecasting of future ice flow. The authors use a novel technique, statistical emulation, to reduce the effective dimensionality of otherwise prohibitively expensive ice sheet model runs. Using this emulation technique, the authors apply a calibration procedure to estimate unobserved model parameters that they then incorporate into probabilistic forecasts. This paper addresses a real need in glaciology for more statistically sound approaches to parameter estimation and forecasting, especially given the substantial uncertainty in centurial-scale predictions of mass loss from the West Antarctic Ice Sheet.

However I have serious concerns about the conclusions that the authors made from the

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application of their methods and cannot recommend the paper for publication. These methods have not yet been benchmarked on representative synthetic problems and this step is a necessary prerequisite for the publication of results using new methods.

General comments:

The statistical methods that the authors use are comparatively new in glaciology. The authors cite several precedents from other fields and a paper by Chang and others from 2016 that used a similar combination of emulation and calibration. Chang et al 2016 and the current paper apply these methods to different datasets, however, and the success of the method at making certain inferences from one data set is no guarantee that the inferences from a different one are accurate.

To establish the correctness and capability of a new method on real data, it is common practice to first test it on a synthetic problem where the ground truth values of all fields and the signal-to-noise ratio of the synthetic observations are both known exactly. Without going through this preliminary testing step, you cannot be sure if the method improves on existing approaches, if the posterior density assigns non-zero probability to ground truth values, or even if the code to implement it is correct.

My most serious concern is with the authors' finding that a linear sliding relation gave the best fit to observational data using their calibration procedure. This result disagrees with recent published work using model-data comparison. Gillet-Chaulet et al. 2016 found that $m = 1/5$ or smaller gave the best fit to several years of velocity measurements for Pine Island Glacier. Joughin et al. 2019 tested the linear viscous, Weertman, and Schoof sliding laws against several years of velocity and thickness change measurements at Pine Island Glacier and found that the Schoof sliding law, which is asymptotic to $m = 0$ in the limit of high sliding speed, gave the best fit to observations. Other studies through the years have found evidence for nonlinear sliding using methods ranging from laboratory studies to seismic sensing. The authors state that their calibration procedure gave the best fit with $m = 1$ with little further discussion. Is this an assertion that

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glacier sliding really is linear viscous, despite numerous studies showing nonlinear and even near-plastic sliding? Or is it an artifact of the calibration? If it's the latter then the calibration procedure should be fixed, as other published methods do not come to this same conclusion.

Moreover, the finding that $m = 1$ gave the best fit to observations compared to other parameter choices that were tried does not imply that it gives a good fit to observations in any absolute sense. If the errors in the thickness change measurements are, for example, normally distributed with known variance, then the normalized sum of squared errors should come out to around 1/2. The Konrad et al 2017 paper only offers some range of possible measurement errors but this could be handled in a hierarchical Bayesian framework and the idea is the same. The question is not just what parameter combination gave the best fit to observations, but also whether that fit is good enough in an absolute sense given what we know about the error statistics. Otherwise we are merely choosing the best among bad options. This issue is discussed in MacAyeal et al. 1995 and Habermann et al. 2012.

Part of the problem might stem from the choice of which parameters to calibrate. The only means by which the viscosity and basal traction can be adjusted is by scaling the amplitude of the optimal results from an inversion computed in Nias et al. 2016. The emulation method can capture the sensitivity of model outputs to variations in this amplitude scaling, but amplitude scaling as such is not necessarily a good way to capture additional modes of spatial variability. Several papers (Isaac et al. 2015, Petra et al. 2014) have successfully applied a dimensionality reduction approach in inverse problems by using the largest several eigenvalues of the Gauss-Newton approximation to the Hessian of the log-posterior. The unusual results from the calibration procedure might be ameliorated by a different choice of basis.

Finally, the authors state that the prediction uncertainty is greatly reduced by using their method. However, they apply a constant climate forcing, which is difficult to justify given recent trends of CO2 release that more follow the RCP8.5 scenario. The authors

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also state that future ocean warming is uncertain, but recent results from ocean GCMs suggest that the warming trend around the Amundsen Sea is likely to continue into the future, see Holland et al. 2019.

Specific comments:

Page 2: 10-11: Worth mentioning some of the paleoglaciology literature, see Hein et al. 2016.

Page 3: 9-11: How nearby and how correlated? A standard approach in geostatistics would be to assume that the correlations between the error made in measurements at point x and point y is proportional to $\exp(-|x - y|/L)$ for some correlation length L . What is the correlation length for the observational data you're using? You assert that model-to-observation comparisons on a cell-by-cell basis are not statistically independent, but that depends on whether the model resolution is large or small compared to the correlation length.

Page 4: 15-16: Why should scaling the viscosity and friction coefficients up and down be a good way to capture variability in these fields that was not captured in the original study by Nias et al.? The true misfit might instead have a completely different spatial pattern.

Page 10: 3: The fact that the most likely fields match the inversion from Nias only tells us that the fit can't be improved within the much lower-dimensional parameter space that you've chosen, not that it can't be improved through the addition of a completely different mode of spatial variability.

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