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The Cryosphere Discussions

Interactive comment on "A Bayesian Hierarchical Model for Glacial Dynamics Based on the Shallow Ice Approximation and its Evaluation Using Analytical Solutions" *by* Giri Gopalan et al.

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I. General Comments

The paper proposes to apply the Bayesian hierarchical model (BHM) framework to glacier modeling under the shallow ice approximation (SIA), using analytical solutions to the partial linear equation system. A unique aspect of the work is a sophisticated numerical error correction scheme through two dimensions of space and time, based on a statistical model. The authors conclude that their method is able to infer meaningful probability distributions for glacial parameters and predictions of the ice thickness, adequately accounting for the error originating from the numerical solver as well as

uncertainty in the parameters.

The problem is well-framed and the authors introduce it clearly through general explanations and context in sections 1 and 2, which greatly participates to the accessibility of the paper to non-specialists. The figures and tables are straightforward to interpret and informative, although in some cases expanded captions would be more useful if more information was reminded to the user given the length of the paper (see technical comments below).

The contribution is original and significant to advancing uncertainty quantification methods in the field of cryosphere science and seems promising in generating more applied follow-up works. On the other hand, however, I think the manuscript could be further improved, particularly by expanding the discussion section on the potential and limitations of the BHM for the broader community. I support the publication of the paper after minor revisions related to the following points.

II. Specific comments

1. The authors briefly mention in the summary and discussion that the method is applicable to broader problems in cryosphere science. Without going into further calculation, I believe expanding on that topic in the discussion would both provide better contextualization of the problem tackled here and increase the impact of the paper. What challenges do you expect for the cryosphere science community to apply BHM approach to the non-SIA regime, e.g. for fast-discharge ice streams, or to SIA problems without analytical solution (e.g. more realistic geometry)?

2. The authors manage well to point out the limitations introduced by simplifications in the physical problem and choices for the statistical distribution of errors. However, even after a few readings, it remains a bit difficult for me to tell what are the limits or downsides of the BHM approach itself, particularly on the resolution of parameters and state variables (e.g. ice thickness or velocity field).

- In the context of the SIA equations, can you say something about the relationship between the number of observations and the number of parameters? That is, how does the posterior evolve in the different cases with respect to the number of observations?

- Given the symmetry and choice of displaying only one quadrant in Figure 4, I wonder if the information (or uncertainty quantification) retrieved on the ice viscosity reflects that of 8 observations or that of $\sim$32. If one would compute a similar problem with a non-idealized glacier, how many observations would one need to obtain a similar posterior distribution for ice viscosity?

- Similarly, do the authors expect the spatial distribution of the observations to play a critical role in determining the posterior given the different sensitivity of the dome, margin and interior of the glacier?

3. Although the true value always remains within the confidence interval, there seems to be a tendency to under-predict the ice viscosity (as seen in Table 2) and over-predict the thickness (Figure 5). Is there any reason for that or is this purely the result of randomness?

4. In a non-linear PDE system, it is not guaranteed that the posterior is Gaussian or even symmetric distribution (even when propagating Gaussian errors). While the authors put a certain emphasis on the ice viscosity and basal sliding parameter, with respect to which the problem is linear, this linearity might not hold in general for every parameter or state variable one might want to keep track of. After all, a major appeal of Bayesian methods is that they require no assumption on the physics that are being solved, and are thus well suited to nonlinear problems. With that in mind, I believe using an accurate but more general terminology would be beneficial to future users of this work:

- p10 l18-20: "the .99 posterior credibility interval was computed by taking 3 standard deviations below and above the maximum a posteriori estimate (MAP) of the posterior samples." Even though these indicators are equal for a Gaussian (or any symmetric)

distribution, as a principle I would advise to refer to the mean or median instead of the maximum, as the former remain comparatively more adapted to characterize distributions even when they are not Gaussian. Perhaps the authors should also remind the reader that in a general (non-Gaussian) case, a distribution is best characterized by multiple indicators, e.g. quantiles as in Figure 5, and not just maximum and standard deviation.

- Throughout the manuscript the authors use interchangeably the phrases "3-Sigma" and ".99 Confidence" interval, as pointed out above. In a Gaussian distribution, the 3-sigma interval accounts for $\sim$0.9973 of the integral while the .99 interval represents $\sim$2.58-sigma, and clearly these are not the same. I think the authors should clarify and streamline this. It might otherwise introduce confusions and discrepancies in the exact numbers for readers that try to reproduce the results or compare them with a slightly different model setup (e.g. different geometry), especially if their method is based on numerical integration of the posterior.

- I recommend the authors to display the posterior distribution of μmax, as a supplemental figure. Likewise, Figure 7 suggests non-symmetric probability distributions of the thickness originating from the error propagation, it might be beneficial to highlight the non-linearity by plotting these distributions in a similar way as Figure 6.

III. Technical comments -Figure 5: Outside of the whiskers, small circles are displayed, but the caption doesn't indicate what they are. If they are important, the authors should improve their visibility and add explanations related to them in the caption. If these are not meaningful on the other hand, the authors should remove them.

-Figures 5, 6, 7: when referring to test cases, remind the readers the specificity of these tests, e.g. “test case B (no mass balance or basal sliding)”. This would lessen the need for cross-referencing.

-Table 2: The exponents of units are not displayed in superscript.

-Table 3: Is the dome error not calculated the same way as the margin and the interior? If so, I did not find any explanation in the text. If not, I suggest that the authors streamline the column labels. Also, the authors should expand in the caption what RMSE stands for.

-Table 4: The authors should remind in the caption what the different symbols refer to.

I hope the authors will find this useful.

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