Review of "The future of Upernavik Isstrøm through ISMIP6 framework: Sensitivity analysis and Bayesian calibration of ensemble prediction" by Jager et al.

In the paper 'The future of Upernavik Isstrøm through ISMIP6 framework: Sensitivity analysis and Bayesian calibration of ensemble prediction', Jager and co-authors study several aspects associated with the evolution of the Upernavik Isstrøm Glacier, Greenland. Based on a statistical framework, numerical results obtained with the Elmer/Ice finite-element code, and observational data, they quantify the uncertainties associated with predictions of the future sea-level contribution. They improve the robustness of their analysis by considering a cross-validation step and by studying several weighting methods for assigning a likelihood score to the uncertain parameters.

I think that the paper will make a great addition to the scientific literature as it deals with an important topic, namely the quantification of uncertainties, and, more generally, the study of the methods that are used to produce such analyses. Nonetheless, I have a series of comments that I would like the authors to address prior to the publication of the manuscript. As described hereafter, those are mainly related to the form of the paper, rather than its scientific content.

General comments

My main comment is related to the way the paper is written. I have found the methods and results to be particularly interesting, but the style of the paper makes it quite difficult to grasp them efficiently. My main complaints are that the whole paper is very long (45 pages), that some parts are difficult to follow because of the lack of visual data, and that some subsections are particularly long. I would suggest the following changes:

- Streamlining the manuscript, in particular by focusing on the key points in each paragraph, and removing unnecessary repetitions.
- Focusing on the novel aspects of your study. To my understanding, these are the cross-validation (which I believe has not really been done previously in a glaciological context), and the use of different weighing methods.
- Adding figures/tables/schematics that allow to understand the content of the text in a visual and summarized way. For example, in Section 2, you could create a table with the different scenarios (Hpr, Cpr, Ppr), and for each of these scenarios specify the SMB that is used, as well as the front position and the uncertain parameters. For the observational data, you could create a table with the different types of observations that you have, their type, and where they come from.
- I wonder if the f_{param} weighing makes much sense, giving that this parameter is one of the uncertain parameters that are calibrated in the Bayesian process. Note that a classical way to favor specific values of f_{param}, given your knowledge of its importance, would be to change its prior distribution.

To reiterate, I find the paper to be both useful and significant. But I still think that it is important to improve its style, as it will help the audience to better understand the key points presented in the manuscript.

Specific comments

(1) [Line 24] It is a bit unclear at this stage what distinguishes the 'limited understanding of ice dynamics ' and 'uncertainties in Ice Sheet Models'. Maybe specify what you mean for the latter, e.g., 'uncertainties in the parameters of Ice Sheet Models'.

(2) [Line 34] A paper that is missing for Antarctica is Bulthuis et al. (2019).

(3) [Line 63] ‘initialisation’ → ‘initialization’ as you use American English in your manuscript. Also check Lines 97, 158, 340, 546, 756, 774, 775, 792, and 864.

(4) [Line 65] The use of the active voice in this sentence is a bit weird here, given that the rest of the paragraph is written with the passive voice.

(5) [Line 168] I am guessing that the equation mentioned here should be Eq. 1, not Eq. 4.

(6) [Line 168] It is a bit confusing that the sensitivity indices S_i are called ‘first-order sensitivity indices’ here, and not before. I would suggest discussing why the S_i are called ‘first-order indices’, or directly mentioning Line 161 that the indices that you introduce are of first order. Otherwise, the reader might wonder which indices you are talking about in this paragraph, as it is not clear that you are talking about the S_i indices.

(7) [Line 172] ‘Y’ needs to be written in italics (Y) here.

(8) [Equation 6] The first factor is not a prior distribution for the problem considered in the paper. Going back to Aschwanden and Brinkerhoff (2022), a possible name for this factor would be ‘projection’.

The distinction between prior and posterior distributions (i.e., Bayes’ theorem) appears later, implicitly, through the computation of the term $P(\mathbf{M}|\mathcal{B})$ in equation (6). Specifically, Bayes’ theorem writes

$$P(\mathbf{M}|\mathcal{B}) = \frac{P(\mathcal{B}|\mathbf{M})P(\mathbf{M})}{P(\mathcal{B})} = \frac{P(\mathcal{B}|\mathbf{M})P(\mathbf{M})}{\int P(\mathcal{B}|\mathbf{M})P(\mathbf{M})\,\mathrm{d}\mathbf{M}}, \tag{R1}$$

where:

- $P(\mathbf{M}|\mathcal{B})$ is the posterior distribution;
- $P(\mathcal{B}|\mathbf{M})$ is the likelihood distribution;
- $P(\mathbf{M})$ is the prior distribution.

For all intents and purposes, you will find at the end of this review a few equations that show how, starting from (R1), I arrive at your equation (7).

(9) [Line 214] Ideally, you should define every variable that appear in the equations, so $F_m^j, F_o^j, Q_{m,i}^j, \dots$ should be defined. To save space, it makes sense no to do so, but please at least mention in this paragraph that the subscript i is associated with the i-th member of the ensemble and that the superscript j is associated with the j-th observation.

(10) [Line 216] I am guessing there is an ‘it’ missing before ‘is common’ here.

(11) [Line 237] It really is a detail, but please avoid using fractions within the text. Instead, write the definition of w_i as a full new equation, or write it as $w_i = P(\mathcal{B}|\mathbf{M}_i)/\sum_{k=1}^{n_m} P(\mathcal{B}|\mathbf{M}_k)$. Same comment for the factions that appear later on in the text.

(12) [Equations (8)–(12)] I suggest removing equations (8)–(10), as these equations do not add much to the discussion, and might even appear unnecessarily technical. It seems to me that the reader should be able to deduce from the Gaussian and independence assumptions that $P(\mathcal{B}|\mathbf{M}_i)$ has the form shown in (11), which is quite standard.

(13) [Line 248] Technically, H is the measurement operator, not $H(\mathbf{M}_i)$ (which is the value taken by this operator when $\mathbf{M} = \mathbf{M}_i$).

(14) [Line 266] To be consistent, write $f(RMSE, \sigma)$, not just $f(RMSE)$.

(15) [Line 271] Please read again this paragraph, it seems that you repeat yourself.

(16) [Subsection 2.3.1] Overall, I think that this subsection is not structured in an efficient way: you first present the equations (12) and (13), corresponding to the 'classical' approach. Reading the beginning of this section, it seemed to me that you are going to use those expressions. But then you discuss limitations (which always is a real plus), and consequently modify you formulas. It might make more sense to directly state that while expressions (12) and (13) are the usual approach, you are not going to use them, and instead will use the formula (14) instead. On a related note, it is a bit surprising that you mention Line 249 that σ is the standard deviation of the observation error (while it is common, as you mention later, to include the model error in it). So maybe directly state the difficulty associated with σ, and that your equation (14) is a possible solution for it.

(17) [Line 294] I wonder if the discussion of the assumptions that must be examined should not be included in the 'full-period weighting' item, Line 300.

(18) [Line 382] I do not agree with the contradiction indicated by the 'On the contrary' here: the fact that the sum of the first-order Sobol indices is greater than one does not prevent a substantial impact of specific parameter combinations. Furthermore, the fact that the sum of the first-order Sobol indices is smaller or equal than one does guarantee that the inputs are independent.

(19) [Line 462] $law \to f_{\text{law}}$.

(20) [Line 462] The fact that the priors and posteriors distributions are similar for several parameters is an important result. Maybe you could elaborate on that, both in terms of the interpretation that you give to this observation, and the conclusions that can be drawn for it.

(21) [Line 468] As before, this 'posterior ensemble' is a bit confusing as you are looking at the distribution of ice mass discharge, rather than the distributions of the inferred parameters (which have been analyzed in the previous subsection). Maybe use another name for this subsection, or precise in that name that you are going to talk about SLR predictions.

(22) [Figure 5] This figure is difficult to read. Consider using brighter colors and larger labels.

(23) [Line 553] It's → It is.

(24) [Line 595] dynamics modeling community → ice-sheet dynamics modeling community?

(25) [Line 660] it's → it is.

(26) [Line 746] Would that still be true if you looked beyond 2100? I have in mind the study of Brondex et al. (2017, 2019) which showed that the form of friction laws does have a strong impact (in particular, there is a significant difference between the regularized Coulomb and Budd laws).

(27) [Line 789] Maybe add that 'SSA' also stands for Shallow-Shelf Approximation.

(28) [Lines 811, 843, 876] regularisation → regularization.

(29) [Line 840] Maybe add that this value of u_0 is similar to the one chosen in Joughin et al. (2019).

(30) [Line 884] $law \rightarrow f_{\text{law}}$.

Additional equations – comment (8)

If $P(\mathbf{M})$ is a discrete distribution, i.e., if $\mathbf{M}$ can only take a finite number of values in the set $\{\mathbf{M}_1, \ldots, \mathbf{M}_{n_m}\}$, and if these values are equally probable, then

$$P(\mathbf{M}) = \frac{1}{n_m} \sum_{i=1}^{n_m} \delta(\mathbf{M} - \mathbf{M}_i), \tag{R2}$$

in which the factor $1/n_m$ is the normalization constant, necessary to obtain a distribution. Introducing this expression in (R1) yields

$$P(\mathbf{M}|\mathcal{B}) = \frac{P(\mathcal{B}|\mathbf{M}) \sum_{i=1}^{n_m} \delta(\mathbf{M} - \mathbf{M}_i)}{\int P(\mathcal{B}|\mathbf{M}) \sum_{i=1}^{n_m} \delta(\mathbf{M} - \mathbf{M}_i)\, \mathrm{d}\mathbf{M}}. \tag{R3}$$

This can be simplified: on the one hand, we have

$$P(\mathcal{B}|\mathbf{M}) \sum_{i=1}^{n_m} \delta(\mathbf{M} - \mathbf{M}_i) = \sum_{i=1}^{n_m} P(\mathcal{B}|\mathbf{M}) \delta(\mathbf{M} - \mathbf{M}_i) \tag{R4a}$$

$$= \sum_{i=1}^{n_m} P(\mathcal{B}|\mathbf{M}_i) \delta(\mathbf{M} - \mathbf{M}_i). \tag{R4b}$$

On the other hand,

$$\int P(\mathcal{B}|\mathbf{M}) \sum_{i=1}^{n_m} \delta(\mathbf{M} - \mathbf{M}_i)\, \mathrm{d}\mathbf{M} = \sum_{i=1}^{n_m} P(\mathcal{B}|\mathbf{M}_i). \tag{R5}$$

Combining these results together yields

$$P(\mathbf{M}|\mathcal{B}) = \sum_{i=1}^{n_m} w_i \delta(\mathbf{M} - \mathbf{M}_i), \quad w_i = \frac{P(\mathcal{B}|\mathbf{M}_i)}{\sum_{k=1}^{n_m} P(\mathcal{B}|\mathbf{M}_k)}, \tag{R6}$$

which is precisely equation (7).

References

Aschwanden, A. and Brinkerhoff, D. J. (2022). Calibrated mass loss predictions for the greenland ice sheet. *Geophysical Research Letters*, 49(19).

Brondex, J., Gagliardini, O., Gillet-Chaulet, F., and Durand, G. (2017). Sensitivity of grounding line dynamics to the choice of the friction law. *Journal of Glaciology*, 63(241):854–866.

Brondex, J., Gillet-Chaulet, F., and Gagliardini, O. (2019). Sensitivity of centennial mass loss projections of the Amundsen basin to the friction law. *The Cryosphere*, 13(1):177–195.

Bulthuis, K., Arnst, M., Sun, S., and Pattyn, F. (2019). Uncertainty quantification of the multi-centennial response of the antarctic ice sheet to climate change. *The Cryosphere*, 13(4):1349–1380.

Joughin, I., Smith, B. E., and Schoof, C. G. (2019). Regularized Coulomb Friction Laws for Ice Sheet Sliding: Application to Pine Island Glacier, Antarctica. *Geophysical Research Letters*, 46(9):4764–4771.