Response to Anonymous reviewers for the manuscript: A framework for time-dependent Ice Sheet Uncertainty Quantification, applied to three West Antarctic ice streams by Beatriz Recinos et al.

Dear Editor and Anonymous reviewers,

Thank you for taking the time to review and improve our manuscript. We believe that we have addressed all points raised by reviewers and hereby submit the revised version together with a point by point reply to each of the reviewers comments. For minor comments and corrections please referred to changes highlighted in blue, in the diff.pdf file attached to this reply.

Below reviewer's comments are given in italics and our answer in normal font.

Reply to Anonymous Referee # 1

RC: My main comment is that the conclusion indicates that the regularisation weights suggested by the L-Curve analysis seem to lead to priors that are too confident, suppressing the propagation of the uncertainty from the velocity data-sets used for the calibration. However, I found that the method for the L-Curve is not very well described as there is 4 parameters to calibrate, and it is not to clear if they are chosen independently?, and there is a high level of user-judgement in the selection of these parameters; Comparing the values given in section 4.1 to those used in Table 1, it appears that the main differences are on the δ parameters for which the results are not shown. I am also wondering part of the issue cannot come from wrong priors as they are particluarly poorly constrained and here, the prior for the friction parameter α is 0, so that pure sliding everywhere? So maybe the conclusion could be revisited a little to not put too much attention on the L-Curve?.

AR: Both L-curves γ_α and γ_β in sect. 4.1 are computed independently from each other - i.e. varying one parameter over several orders of magnitude while the other parameter remains fixed, following control-method applications found in the ice-sheet modelling literature - e.g Jay-Allemand et al. (2011); Gillet-Chaulet et al. (2012); Seddik et al. (2017); Barnes et al. (2021). We agree with reviewer's #1 comment; in our manuscript and in the literature there is a high level of user judgement and a large variability in the application of the L-curve criterion among ice-sheet modelling studies. Our aim in this section is not to refine or improve upon the application of the L-curve methodology in glaciological inversions but simply to apply it to a standard defined by the glacial literature. We discuss our approach in terms of (i) user-judgement; (ii) calibrating multiple parameters; (iii) consideration of our "variance cost" parameters δ_{alpha} and δ_{beta}; and (iv) choice of prior mean below.

The L-curve criterion used in our methods and in previous studies (listed above) is based on Hansen (1992, 2001), where L-curves are defined as a log-log of the norm of a regularised solution versus corresponding residual norm. The correct regularisation term is chosen by locating the "corner" of the L-curve, which represents an optimal trade-off between fit to data and smoothness of solution. Hansen (1992, 2001) identify the "corner" as the point on the L-curve with maximum curvature. However, as pointed out by these papers, accurate calculation of curvature as a function of the regularisation parameter is challenging and requires dense sampling of the L-curve which could be computationally expensive. Preferably, the finding of the "corner" value is often chosen heuristically. For example, some studies choose the point where the cost function is at its minimum (Gillet-Chaulet et al., 2012) whereas other studies choose points just before the curvature of their L-curve analysis (Seddik et al., 2017). Others

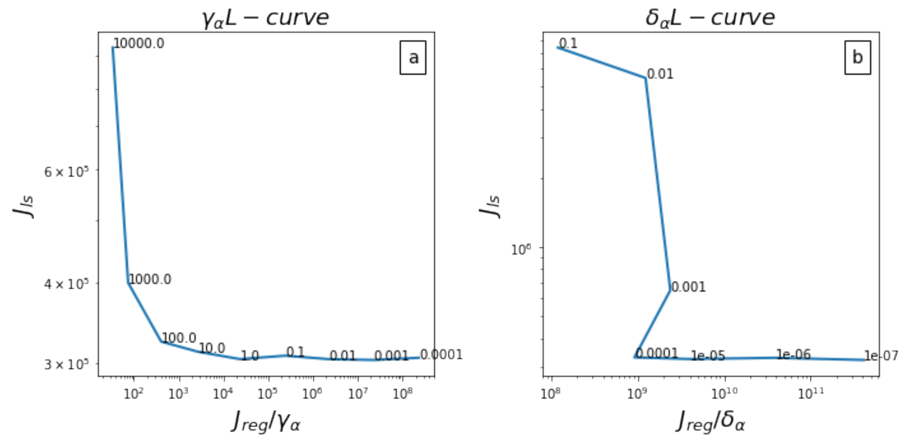

Figure 1: L-curve analysis for **a:** γ_α and **b:** δ_α.

emphasise the goal of finding the "best trade-off", aiming to pick parameters whose values lie near the corner of the L, where neither J_{ls} or J_{reg} take high values (Barnes et al., 2021). Therefore we made our choices in accordance with this loosely defined criterion and decided on $\gamma_\alpha = 100.0$, based on a visual assessment of the corner in a log-log scale plot. In theory, any point between 1000 - 1.0 could be a potential value for γ_α (see Fig. 1), though we know from our physically informed prior that it should be in the order of 1. However, this is not evident if we follow the method of finding the "corner" of the L-curve as suggested in the literature.

The classic L-curve approach is prescribed for a single regularisation parameter, whereas large-scale glaciological data assimilation often involves multiple parameters, leaving some ambiguity of how to proceed. While some studies consider two-dimensional "L-surfaces" in order to jointly optimise parameters (e.g., Fürst et al., 2015; Goldberg et al., 2019), such an approach in our case would have required a prohibitively expensive sampling of four-dimensional space and complicated visualisations. Rather, we followed the approach of Barnes et al. (2021), in which one parameter is varied while others are held constant.

For these reasons and in line with common practices among the ice sheet modelling community, we do not make significant changes in sect. 4.1 or our conclusions but clarify the methods that we follow in constructing the L-curves of sect. 4.1 (see L390-403 of attached diff report), as our main point is to highlight that L-curve analysis leaves room for user interpretation and is not based on existing prior knowledge and physical concepts that define each control parameter. The variance σ^2_{c0} and auto-covariance length scale l_{c0} of each control parameter still provides a more physically informed prior and allows for the study of the interaction between all the poorly constraint parameters.

Additionally to the experiments shown in Sect. 4.1, we constructed a L-curve **only for** δ_α, as in Sect. 5.1 we show that only a strong prior on α will influence the posterior uncertainty of VAF. We have added this analysis here and to appendix of the manuscript (see Appendix A2, L770-772 of the attached diff report). Fig. 1b suggests a value of $\delta_\alpha = 1 \times 10^{-4}$ (we use $\delta_{\alpha,\beta} = 1 \times 10^{-5}$ in the analysis of Sect. 4.1). We know from the δ_α values shown in Table 1 that any $\delta_\alpha \geqslant 1 \times 10^{-7}$ will result in too strong of a prior. Therefore it is unlikely that any value for δ_α (or δ_β) based on an L-curve analysis would be appropriate for uncertainty quantification. As this does not impact our results or overall conclusions, we do not investigate this further but show the L-curve for δ_α in the appendix.

Regarding the prior mean of $\alpha_0 = 0$, we chose zero as α can be positive or negative therefore, a zero mean seems appropriate. Furthermore, any guess not based on sub-glacial exploration would implicitly involve physics of the model.

RC: L124 'constant surface mass balance"; Is it constant and uniform; or is there spatial variability?
AR: Constant means uniform and constant in time. There is no spatial or temporal variability. We have clarified that in the text (see L137 of the attached diff report).

RC: EQ6. Q_T here is defined as the VAF while is it use as the difference of VAF from t=0 in the manuscript. What is the meaning of the "+" symbol?
We have modified this equation to state that Q_T is equal to VAF and time T, and modified the captions and y-axis of Figures 5a and 10d to reflect that we plot trajectories of change in VAF ($Q_T - Q_0$) over time. The "+" refers to the positive part of the volume that contributes to global mean sea level. We have added this explanation, see L156 and Figures 5 and 10 of the attached diff report.

RC: L142 $Hf = max(0, -R(\rho_w/\rho_i))$
AR: Corrected, see L155 of the attached diff report.

RC: L196 if the prior is strong, γ is "large" not"small"? (in agreement to line 342-check for consistency everywhere)
AR: This particular text is not part of the manuscript anymore. However, we checked and this was the only inconsistency found.

RC: Sec 4.1 would be interesting to discuss the smoothing parameters in terms of variance and correlation length scales (Eq. 13-14) as it appears that the parameters used here lead to a very small variance compared to the values used in Table 1.
AR: The variance σ^2_{c0} and auto-covariance length scale l_{c0} has been added to L-curve derived priors in Table A1, see page 47 of the attached diff report.

RC: L389 J^c should be J^c_{mis}? Check for consistency everywhere. I don't understand why it does not change with the number of observations as according to Eq.8 it should depend on the number of observations?
AR: Indeed there was a mistake in the caption and y-axis of Fig. 7, it should have been J^c_{mis}. This has been corrected in Figure 7 in the text (See Fig 7, L445, L457, L461 of the attached diff report). In this experiment and in the results of J^c_{mis} plotted in Figure 7, we do several inversions (with the same priors); at each inversion we retained different % of data points (or observations). For every % of data points retrained, we invert for both α and β and evaluate J^c_{mis}. We do this to find out, if by dropping observations our ability to reproduce the observed velocity decreases. A point of confusion may be the data used to evaluate J^c_{mis} – this was distinct from the inversion constraints, and consistent across the experiment, which we now make clear (see L445 and L457 or diff report).

RC: L477 "as the basal stress does not scale with effective stress in the interior". I don't understand the argument here.
AR: We apologise for not being more clear here. The Cornford sliding law has the approximate form

$$\tau_{\mathbf{b}} \approx \alpha^2 u^{-2/3} \mathbf{u} \tag{1}$$

in the interior i.e. where N is large. It can be seen that this differs from the Weertman-Budd sliding law by a factor of $N^{1/3}$, and thus we expect that the inverse solution for α with a Cornford sliding law should be larger here by approximately this factor. Any prior covariance, therefore, should reflect this change in scale, and hence $\sigma_{\alpha(0)}$ was made larger to reflect this. We added the Cornford sliding law prior configuration to Table 1 and now state (see L545-550 of the attached diff report):

"The prior distribution used is similar to the highlighted parameters in Table 1, but with a modified $\sigma^2_{\alpha(0)}$ – since in the interior the basal stress is independent of the effective stress (Cornford et al., 2020), and thus we expect variations of α to have a different scale (see last configuration in Table 1)."

RC: L486 "is due to insensitivity of basal stress to α when the ice is near floatation". The Weertman-Budd relation Eq. 1 is also insensitive to α near flotation as it depends on N; main difference is that Eq.3 tends to a Coulomb regime, independent of α, for high velocity and low effective pressure. However using eq. 12 for N tends to restrict this domain to the close vicinity of the grounding line (Joughin et al., 2019)
AR: We are unsure which equation is referred to by eq. 12 (as this is relating to prior distributions) so we might be misunderstanding, but we feel that we are mostly in agreement with the reviewer. Since Eq. 3 tends to a Coulomb regime near the grounding line, this mutes dependence on α, which is why, after rescaling of colorbars, there is high uncertainty near the Smith grounding line in Fig 10b (Cornford) but not 10a (Weertman-Budd). In the Weertman-Budd sliding law, there is still quadratic independence of basal stress on α; effective stress (N) becomes small, but at the same time ice speed is large, and the result is that α is better constrained (relative to other regions of the domain) than in the Cornford case – indeed, this is what Figs. 10 (a,b) show.

Reply to Anonymous Referee # 2

RC: The 3 sentences (l52-56) are not enough to introduce basic concepts of Bayesian inference to the community, and especially to connect to the ice sheet model present study. To elaborate, please define clearly here what you mean by prior/posterior/covariance, link it directly to glaciological quantities, and give some intuition on the method. Also, it could be better motivated. If I understand, L54-55, propagation of errors between uncertain control parameters, and VAF could be obtained by proceeding to a massive amount of model realization, which is prohibitively expensive due to the costs of Stokes solving, right? This is what motivates you to take another approach? If yes, I suggest to re-structure your paragraph starting from this motivation statement, and then elaborating (substantially) on Bayesian approach, and what this means in the context of your problem.
AR: We agree with the reviewer's comment and have expanded the explanation of the Bayesian inference framework in that paragraph. We have now defined what low/high dimension means (L47-48 and L50-51 of the attached diff report) and each basic concept of Bayesian inference and relate each concept to variables in our study (L52-68 of the attached diff report).

RC: Despite several passes, Section 2.4.1 and 2.5 remains unclear to me, probably because I have no prior experience in Bayesian approaches, and I have not looked at the references. Here, I would expect to at least get a rough idea from these sections without having to go to references. E.g. where do the finite element matrices use to define Γ_{prior} come from? What is the role of the operator (11) in the story? Justifications and explanations would be very welcome to explain all equations given in 2.4.1. As this is central in the paper, this part must be self-explained (i.e. referencing if not enough). Similarly, Eq. (16) and (17) are highly important, but under-explained, please elaborate, give some intuition, and connect to what this means in the context of your glaciological problem. Several sentences could be founded an other articles on using Bayesian approach for a completely different problem. Therefore, there is room to better connect the approach and the application.

AR: We apologise for any lack of clarity in these sections. Indeed our aim was to place focus in this paper on the experimental results, and avoid a lengthy coverage of the underlying mathematical and numerical framework, which was the reason we only stated key results from previous papers. But we agree that it may still be confusing to readers not familiar with the literature. We have now made extensive changes to these sections in order to provide clarity, and to attempt to introduce concepts in an order that is not too abrupt and have a better progression. We have also added text to give better intuition for the expressions provided. Changes/additions are as follows:

- We do not introduce concepts of variance and autocovariance of the prior until after the discussion of bayesian inversion, as we feel this might have contributed to the difficulty of 2.4.1. The previous section 2.4.1 is now gone, and we discuss the deterministic form of the regularisation cost in 2.4, mentioning only that the name "prior" is due to its Bayesian interpretation. We now introduce a separate subsection (2.6) in which we discuss the statistical properties of the covariance matrix and how they relate to regularisation parameters – we do this in a separate section because of the relevance of these properties to our investigation, and to avoid introducing statistical concepts ahead of discussing our Bayesian interpretation of the cost function.
- We now introduce Bayes' theorem, and how it relates to our cost function, at the beginning of 2.5 (L235-252 of the attached diff report). While this adds additional text and an equation, we feel it will be helpful for those less familiar with Bayesian concepts, and that it is introduced in an informative way.
- We now give more text explaining the meaning and implications of eq 15 (formerly eq 16), and give an example of how these implications might manifest in the context of ice-sheet inversions (L256-263 of the attached diff report).
- We added more text explaining the importance of Eq 16 (formerly 17) (L267-269 of the attached diff report), and directly reference its sources as it is a nontrivial result.

Eq 12 (previously eq 11) fits into the story as follows: If the linear helmholtz equation $\gamma\nabla^2 y - \delta y = F$ is solved for y via finite elements, it would result in a linear system of equations $\mathbf{L}\overline{y} = \overline{f}$, where $\overline{y}$ are the nodal values of the solution and $\mathbf{L}$ is the **stiffness** matrix. We now refer to $\mathbf{L}$ as the "stiffness" matrix, which we feel is sufficient (see L207 of the attached diff report).

RC: Following my last point, several times in the paper, one refers to "priors" or "posteriors" in a generic way, without specifying the meaning (regularization strength). E.g. a number of sentences are general statements with Bayesian vocabulary and unspecific to the ice flow problem considered here, and this contributes to making the paper abstract for non-specialists. Efforts are required to make the paper further "educational", and the choice of words really matters in that respect..

AR: We added a few clarifications of prior and posterior definitions and relate those definitions to the control parameters that we study. However, due to the length of our manuscript we limit these explanations to the introduction, methods and conclusions. See L52-86, Sections 2.4-2.6 and L734-735 of the attached diff report.

RC: The large amounts of data points in TS-inferred velocity max be redundant, with implications for error propagation of VAF: This is not surprising. RS products may be very dense as the efficiency of feature-tracking algorithms has improved. Therefore, trying to fit densely covered (poss. noisy) observation fields is probably more difficult than if we were selecting only a sparse version of the data, with the result of relaxing / giving room to the optimization. This

is an interesting outcome, however, I find it a bit distracting to find this technical point coming back several times in other sensitivity experiments. Why not simply taking 1.6% of the data in all the paper explaining this choice somewhere. I don't feel this is sufficiently important finding to be part of the abstract.
AR: Agreed. This has been deleted form the abstract and only mentioned in Sect 4.3 and conclusions.

RC: I'm not sure to understand when you say that the regularization is too strong (L506-512): Would you have expected the VAF error (propagated) due to regularization higher than the one induced by different observed velocity products? What are the implications, and your recommendations for regularizing in future studies?
AR: An overly informative prior or a strong prior (strong regularisation) means that the information contained in the prior distribution of a control parameter (e.g. prior point-wise STD of the ice stiffness parameter) dominates the information contained in the ice velocity observations being analysed, and hence our error propagation framework underestimate the variability in SLR projections. We find the variability in SLR projections when we compute VAF trajectories using different satellite velocity products to run our time-dependent ice sheet model. By choosing different satellite products our model leads to different estimates of VAF after 40 years. We use this difference in model output to quantify the variance that projections of VAF are expected to have after 40 years and identify prior strengths (regularisation strengths) that can reproduce that variability. We find prior strengths which are weak enough that the variability seeing in the satellite velocity observations can be propagated to VAF projections. Our reasoning is now made more explicit in the discussion (see L580-587 of the attached diff report.)

Therefore, instead of using L-curve inform prior‘s, we recommend to use the variance and length scale arising from a physical interpretation of the prior to define the regularisation parameters or prior covariance; as these definitions (see Sect. 2.6) will inform the ice sheet model with a more realistic spatial variability regarding the basal sliding and ice stiffness parameters. Moreover, this way of computing the prior covariance will allow our simulations to study the interaction among all regularisation parameters, which is not possible via L-curve analysis (see reply to Reviewer #1). This explanation has been added to the Conclusions, see L737-740 of the attached diff report.

RC: Have you tried to include ice thickness as part of the control parameters? or is this not justified in the special case of ice streams?
AR: We take the ice thickness distribution from BedMachine (V.2.0 Morlighem et al., 2020) and assumed that the errors in this dataset play no role in our calibration uncertainty, which is a common approach in current assimilations of ice-sheet velocities. Early approaches (e.g. Macayeal et al, 1995) considered bed and surface elevation as a control parameter (though not ice stiffness) with the view that the incredibly coarse DEMs available at the time would incorrectly attribute velocity variations to basal stress, but there have been significant advances in altimetry since. We now add to the end of Section 2.4 (**Cost Function, L228-233** of the attached diff report):

"Previous assimilations of satellite velocities also considered ice bed and surface elevations as control parameters (MacAyeal et al., 1995), because available elevation products did not capture the small-scale features driving variations in velocity. We consider this to be less of an issue with the elevation products currently available, though future studies with our framework could consider topographic uncertainty and how it covaries with uncertainty of other parameters."

RC: L73: "overly informative prior" is an example of Bayesian wording for which I have no intuition. Please try to in other words, or better connect with glaciological words
AR: See added explanation on L85-86 of the attached diff report and our reply to previous items.

RC: Can you explain what you mean by "low-dimension", "high-dimensional" or "low-rank" in several places in the text (l47, 226)
AR: We use the terms low and high dimension to refer to the number of elements in a finite element mesh, or the number of unknowns in an inverse problem. In other words, how many dimensions are needed to represent the hidden parameter field $C(\alpha, \beta)$ in our domain.
The sliding parameter α and ice stiffness parameter β in these other studies are taken as scalar and global (same at every point in the mesh), in our study these parameters scale with the size of our mesh. – i.e. we calibrate each parameter for every element or point in our mesh (see Fig. 1). We added this explanation to the introduction L47-48, L50-51 of the attached diff report.
A low-rank approximation refers to the approximation of the Hessian matrix using a rank of r where r is smaller than the number of rows and columns of the matrix. We only compute 10^{4th} out of 10^{5th} eigenvectors and eigenvalues (r) of the Hessian matrix thus we do not construct the full matrix but an approximation of this matrix, which is usually referred to as a "low rank" approximation in mathematics. We acknowledge that not everyone is aware of this terminology depending on their field. However, we feel that these are basic mathematical concepts available in algebra books as well as easier searches on-line. Therefore, we do not add an explanation of the term "low-rank" to our manuscript, but we modified our manuscript by relating these concepts to parameters and unknowns in our model. See L47-66, L267-269 of the attached diff report.

RC: The norm $\| \cdot \|_{\Gamma_{obs}}$ is not defined here, I understood later than Γ_{obs} are STD weighting the field in the norm computation, but I don't think this is clearly said, or defined at this point.
AR: We have defined the $\| \cdot \|_{\Gamma_{obs}}$, see Line 189. However we respectfully disagree with the comments regarding the definition of Γ_{obs}, which was defined already in Section 2.4, L191-192 of the attached diff report.

RC: The method section is not well structured (e.g. the section "notation", 1 subsection 2.4.1).
AR: We agree that it is odd to have only one subsection within the main section. We have reorganised Section 2.4 (as well as 2.5) in response to your comments above, and 2.4.1 no longer exists.

RC: L502 "different estimates" : please quantify it in percentage.
We do not feel this is appropriate for the discussion, but now give this percentage where the output is first introduced in the Result section (see Section 4.2 of the attached diff report).

RC: In general, one refrains from starting sentences with mathematical symbols and the paper introduces an impressive number of symbols without any reason, also it would greatly help the reader not to refer to symbols (as it requires the reader to memorize it), but instead to its meaning.
AR: We respectfully disagree. As our manuscript is already long, using symbols allows us to say more with less wording. We also provide a notation section to help the reader with the symbols used in the manuscript, and relate symbols to concepts through out the introduction and methods section of the manuscript. We only use symbols in the results and discussion sections in order to summarised our findings.

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