Impact of time-dependent data assimilation on ice flow model initialization: A case study of Kjer Glacier, Greenland – Authors' response (RC2) –

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August 10, 2023

In this study, authors make use of the vast amount of spatial and temporal coverage of satellite ice velocity observations and ice front positions of the Kjer Glacier (West Greenland). With the goal of improving the glacier's initial state and projections using transient inversions of the control parameters (the ice viscosity parameter B and the friction parameter C) in the model. The authors show that their methods can be applicable to two glaciers in the region. They also explore the possibility of including the stress threshold (σ_{max}) of the calving law as an additional control parameter while using the static friction coefficient (C) and viscosity parameter (B) obtained from the transient inversions (T1 in Table 1). Finally, the authors explore the possibility of inverting for all control parameters at once (C, B, and σ_{max}).

They conclude that transient inversions (on B and C) are able to capture the current trend of changes in glacier velocity better than snapshot inversions, and that those transient inversions improve the models ability to predict near-future changes. Even if a short period of observations is used for the calibration.

An additional experiment on the calving control parameter (σ_{max}) shows that it is possible to invert for this poorly constrained parameter via data assimilation techniques and reproduce to a certain extent the retreat of the Kjer glacier.

They also imply in their conclusion (this is not clearly stated) that the calibrated parameters depend strongly on the strength of the regularisation imposed (choice of weights) for each misfit term in the Cost functions, which leads to several solutions for control parameters and to an overfitting, if L-curve analysis is used to estimate the strength of the regularisation.

Overall, I find the manuscript well written, with a clear narrative and description of the methods and experiments. I also find the whole manuscript very interesting to read. I learned a lot!

I will definitely recommend the publication of the manuscript after the authors clarify some of my questions below and make some minor changes.

We thank the reviewers for reviewing this manuscript and thier constructive comments.

Main comment:

The authors do not describe how the L-curve criteria has been applied in their study. I think this should be explained in Section 2.4 (L151-162). There is no information on the values of the (γ) and no L-curves are shown. There should be some information on how these parameters are chosen. In other words, how the authors choose the strength of their regularisation in each Cost function? Maybe some explanation similar to previous studies that use L-curve analysis (Gillet-Chaulet et al. 2012; Seddik et al. 2017; Barnes et al. 2021).

Probably authors could also add a table in the annex with the γ parameter values and the L-curves (or L-surface if that is the case) and describe what criteria they used for choosing γ values and if they keep the same values for all the experiments. They mention some overfitting and that more investigation is needed in this area, I think this is an important point and should be highlighted.

We agree with the reviewer regarding this point. This is also suggested by the other reviewer. We will add the L-curve plot figure and explain how γ was chosen. We kept the same values for this study and we will add that to the revised text as well.

Is also not clear to me why in the SI experiment, the authors do not invert for the ice viscosity parameter (B) and estimate B from modelled ice temperature instead (and only in that experiment). This will just add extra uncertainties to the inverted field (i.e. errors in the ice temperature model will be propagated to the results). This error could be difficult to account for and might influence the results shown in Figure 3 for the SI inversion. Clarifying that will strengthen the results of the manuscript.

We agree with the reviewer. We will run the new snapshot simulation that includes the inversion for the ice viscosity parameter (B), and add those results.

Title suggestion: maybe this should be initialization and projections (or forecast).

We will change the current title to "Impact of time-dependent data assimilation on ice flow model initialization and projections: A case study of Kjer Glacier, Greenland", as suggested.

L17: "accurate mass balance" $->$ "accurate ice sheet mass loss"

We will change this in the revised text, as suggested.

L30: "but often fail at accurately capturing their present-day configuration", add citation.

We will add it to the revised text.

L45-L60: literature review, probably I missed this but it could be nice if the authors relate those studies to transient inversions (what studies use that type of calibration technique, additionally to the use of AD and data assimilation).

We will clarify this in the revised text.

L130: Remind the reader what parameters you are inverting for? It will be good to mention this also in the Introduction.

We will add it to the revised text.

L144-146: "This approach allows to better understand the physical process involved in reproducing the ice stream..." Point to evidence of this in the results section.

We will add it to the revised text, as suggested.

L190: "limit uncertainties from calving parametrisations", I will add (this is optional): that it also avoids having to reconcile the SMB (estimated by RACMO) with the mass loss estimated by the calving law.

We will add it to the revised text, as suggested.

L283-284: "which improves the model's ability" $->$ "which improves confidence in the model's ability to provide realistic near-future projections". Maybe mention that calibration error and its influence on the model projections still needs to be quantified.

We will change this in the revised text, as suggested. We will also mention the calibration error and its influence.

L289: "...2007 to 2018 is overestimated" indicate the colour of the line in the figure.

We will add this to the revised text.

L299-L301: "These results demonstrate that the simulations based on the transient inversion can enhance our confidence in near-future projections, even with a limited period of observations and when these observations include limited variability to properly calibrate the model".

What happens if the observations used for the transient inversions have a lot of variability in ice velocity? For example if you were to use 2010-2013 (where there is more variability than the periods used for Fig 5) would the model be able to predict changes in the following years?

We expect the model is able to predict changes after the inversion period. To show this, we will run additional experiments and add those results.

L306: It will be nice to add a comment (though this is optional as it is not the goal of the study) regarding the quantification of calibration uncertainty in transient inversions and the propagation of this type of error to projections. The error in the inverted parameters for this type of calibration will be very expensive to quantify via state-of-the-art Markov chain Monte Carlo (MCMC) methods (Tierney, 1994. Petra et al. 2014) and/or Hessian-based Bayesian approaches (Isaac et al., 2015, Koziol et al., 2021), as they will require multiple evaluations of the forward model to sample all the variability in the parameter space. For snapshot inversions the forward model is just a single velocity solved and for transient inversions this forward model is a sequence of time steps. Thus very expensive for error quantification in large-scale inverse problems (¿ 100, 000 mesh elements). Probably this is a limitation for large scale ice sheet problems but might be possible for marine-terminating glaciers elsewhere.

This is an interesting point and we will add a comment about uncertainty quantification to the revised text.

L346: The authors write: "Although large spatial and temporal variability in control parameters could improve the model fit to observations, clear physical justification should be made to avoid overfitting". "Physical justification" of what? I get a bit lost in this statement.

We meant the "physical justification of changing control parameters every year" as we did in TR_CTR experiments. We will clarfy this in the revised manuscript.

Figures

Figure 3, 5, 7, 12 and 13a, will benefit by including in the plots the uncertainty in the ITS_LIVE dataset (ideally the standard deviation of the data set) this could be added to the plot by either using error bars in a scatter plot or changing the size of the triangles according to the error in the data base? This will help us identify if model results are within the observations uncertainty at a given location (and time).

We will add this to the revised manuscript.

Figure 4, 6 and 8. Add citation to the legend for the observations.

We will add it to the revised manuscript, as suggested.

Figure 10. There is a mistake in the caption for the third column, seems like it has the same as the Second column caption but they are different experiments according to Table 1. Check for inconsistencies with Table 1.

We will fix this in the revised manuscript.