

The Cryosphere Discuss., referee comment RC1  
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## Comment on tc-2021-300

Anonymous Referee #1

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Referee comment on "Brief communication: Estimating the ice thickness of the Müller Ice Cap using an inversion of the shallow ice approximation" by Ann-Sofie Priergaard Zinck and Aslak Grinsted, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-300-RC1>, 2021

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### General comments:

Priergaard Zinck and Grinsted present a comparison of two models to estimate ice thickness distribution of the Müller Ice Cap in the Canadian Arctic. The thickness information is further used to select a best site for drilling an ice core. The brief communication paper is generally well structured and written and discusses a relevant topic. There are however some fundamental issues that make that I can at present not recommend this article for publication. My major concerns are related to the methods and concern both the simple SIA inversion and the PISM-based inversion. In my opinion, certain modelling choices are not justified and the comparison of the two methods in its current form does not give too much useful insight. Thorough revisions would be required to make the study worthwhile and would require a change of strategy. My major and minor comments are further explained below.

### Major comments:

#### *SIA inversion:*

The only difference of this method and other SIA-based inversion methods (see e.g. Farinotti et al. 2017, 2021) is that this method takes more freedom to calibrate the inversion method. But by individually calibrating  $a$ ,  $b$  and  $k$ , which are normally dependent on another as they all are a function of exponent  $n$ , the model is no longer really the SIA model after calibration. So the separate calibration of  $a$ ,  $b$  and  $k$  in practice leads to a model that no longer follows the same physics as the SIA and it is unclear what physics it does follow instead. Even when a relatively good fit with observed bed data can be found with this tuning of  $a$ ,  $b$  and  $k$ , it does not give much confidence in good performance of the same model with these parameter values elsewhere. It would make much more sense to only calibrate parameters like Glen's exponent  $n$  or rate factor  $A$ , which would not change anything to the physics of the model.

#### *Mass balance uncertainty:*

In the SIA inversion it is claimed that the mass balance has not much impact on the reconstructed ice thickness. This in reality is of course not the case since the mass balance is a major factor that affects ice extent and average thickness. The reason that it does not play a major role here is that any biases in the mass balance (as clearly seen in Fig. 1)

are indirectly compensated for by the tuning of parameters  $a$ ,  $b$  and  $k$ . But this does not mean that ice thickness is not sensitive to mass balance, any offsets are simply calibrated away. From Figure 1 and the Discussion section it becomes clear that there is a major overestimation of mass balance in the HIRHAM product. This bias is "calibrated away" / corrected for in the SIA inversion but the same is not done in the PISM-based inversion, where only parameters affecting ice flow speeds are used for tuning. It is hence not surprising that the PISM-based inversion provides worse fits to the bed data as the degree of tuning is much less. In other words, the degree of tuning of the two models used in this study is very different and makes it hard to draw any strong conclusions from the current results.

#### *PISM-based inversion & factor K:*

In the Methods it is mentioned that the PISM-based inversion uses a factor  $K$  that varies between 0 and 0.5 from 500 m a.s.l. to 1000 m a.s.l. This implies that below 500 m a.s.l. elevation  $K=0$  and the bed is not modified at all after every iteration. Effectively, that means that at these elevations the bed remains at the initial height which, correct me if I am wrong, is taken from the global estimates by Farinotti et al. (2019). In the Discussion it is argued that the overestimation of ice thickness of the outlet glaciers (which are mostly below 500 m a.s.l.) is a result of the lack of a calving criterion, but I do not think the calving criterion plays any role here since the bed is fixed anyway in these lower areas of the outlet glaciers. The large deviations of bed heights between the PISM inversion and the observations, as shown in Fig. 1, nearly all happen in areas below 500 m a.s.l. Effectively, we are looking at a comparison of the Farinotti et al. (2019) bed and the observations for a large part of the domain. My suspicion is that the choice of variable  $K$  values with altitude was based on problems to make the PISM based thickness inversion converge. The lack of convergence should however not have been solved by choosing a variable  $K$  value, but rather by correcting a bias of the mass balance. A strategy with combined calibration of a mass balance correction and ice flow parameters, and a fixed value of  $K$ , would probably have yielded much more reasonable results. It is good to realize that a more detailed inversion technique, i.e. with a more accurate description of ice motion and boundary conditions (mass balance, calving), should theoretically yield better thickness estimates than simplified approaches as long as the input data (DEM, mass balance and/or ice velocity) is of sufficient quality.

#### **Specific and minor comments:**

L5-6: See the second major comment above. The SIA inversion is not insensitive to mass balance (or at least it should not be), but mass balance biases are indirectly calibrated away through tuning of  $a$ ,  $b$  and  $k$ .

L12-13: Large ice thickness does not always mean very old ice at great depths, only if it is close to an ice divide.

L17-18: "However, field work constraints..." This does not connect well to the previous sentence, more to the one before that.

L18: Please replace "to be clever" with "to be selective".

L21: Please remove the obsolete bracket

L22: Remove the obsolete "in"

Figure 1: UTM  $x$  and  $y$  should be replaced with UTM Easting and Northing instead and the UTM zone should be mentioned in the caption. Furthermore, exponents in the SMB units are missing.

L27: "differs" --> "differ"

L29-31: The new method is not necessarily less sensitive to mass balance, steady state assumptions and ice flow physics. It just collectively calibrates any biases due to these factors away. But the problem is that by doing so the physics of the model are also changed, which is hard to justify. See my first major comment above.

L36: "why" --> "and"

Section 2 (or Introduction): I am missing some information on ice velocities of the ice cap. I suppose these data could for example be extracted from online resources. A source like Its\_Live (<https://its-live.jpl.nasa.gov/>) could potentially be useful. It would give an idea on potential sliding rates which is relevant to know because the non-sliding SIA is used in the SIA inversion, which may be a poor assumption of sliding is significant.

L84: The slope threshold for ice thickness inversion is a critical parameter for SIA based inversions. The chosen value is however somewhat arbitrary which could be acknowledged as a source of uncertainty.

L88: "why" --> "which is why"

L98-100: Under normal circumstances, the mass balance and its distribution in space should have a large impact on the thickness distribution. I suppose better fits could be achieved with a better spatial representation of mass balance.

L107: The PISM inversion is known to work best when a variable climate forcing is applied, i.e. when the modelled ice cap does not reach steady state. The fact that a fixed climate forcing is used here hence adds to uncertainty of the PISM inversion, which needs to be acknowledged.

L118: See my third major comment above. Setting K to zero below 500 m a.s.l. effectively shuts down the inversion process in these areas. This is probably not desired.

L122: "combinations" --> "parameter ranges"

L144: See also my first and second major comment above. In the SIA inversion the entire mass continuity equation, including ice dynamics and mass balance, is tuned through a, b and particularly k. In the PISM inversion, no tuning of for example the mass balance is done, only of a sliding coefficient and enhancement factor. That makes the comparison somewhat odd, since the SIA inversion is much more widely tuned to match the available thickness data.

L151-152: The larger RMSE for the PISM based method is not surprising. Based on Figure 2 this seems to be completely dominated by the errors below 500 m a.s.l. where the bed is not allowed to change in the PISM inversion. Above 500 m a.s.l. there does not seem to much difference between the two approaches (?). An additional mass balance tuning of the PISM method could help to make a better justified comparison of both methods.

L178-179: See also the third major comment above. I cannot imagine that the calving criterion is of much influence, since the bed is not allowed to change below 500 m a.s.l. ( $K=0$ ), which means that a too large extent due to a lack of calving does not really play a role.

L183: See also the second major comment above. There may be an overestimation of the SMB in HIRHAM5, but right now the overestimation of the ice thickness is a direct result of fixing the bed under these outlet glaciers to the Farinotti et al. initial bed. The apparent

problems with the HIRHAM5 SMB data are exactly the reason why also in the PISM approach the mass balance should have been included in the tuning process, like is also done in the SIA inversion.

L188-189: Again, the SIA inversion is not insensitive to mass balance, it just removes mass balance (and other) biases by tuning  $a$ ,  $b$  and  $k$ . Similarly, mass balance biases could (and should) have been tuned also in the PISM inversion to enable a direct comparison.

Figure 3: "UTM x" --> "UTM Easting"; "UTM y" --> "UTM Northing"