

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-300

Anonymous Referee #2

Referee comment on "Brief communication: Estimating the ice thickness of the Müller Ice Cap to support selection of a drill site" by Ann-Sofie Priergaard Zinck and Aslak Grinsted, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-300-RC2>, 2021

Zinck et al are presenting two approaches to estimate ice thicknesses over the Müller ice cap in the Canadian Arctic. The authors are inverting ice thicknesses using (1) an inversion based on the shallow ice approximation, and (2) an inversion based on the PISM model, and compare the results. With the present version of the manuscript, it is not clear to me why the authors chose to compare these two approaches. Furthermore, there is not much description of the PISM model, which prevents the readers from understanding the physics of this approach without referring to other manuscripts. Since we have here a manuscript that is solely based on a method comparison, I think that more details are needed regarding the PISM inversion. Concerning the results, it seems that the inversion based on the SIA is dominated by highly frequent variations on the surface slope. Indeed, the choice of appropriate smoothing parameters needs to be addressed before being able to compare this method to the PISM inversion (see comments below). Similarly, the ice flux in the SIA inversion was calculated, using strong assumptions on the surface mass balance. Furthermore, the SMB value for the calculation seems to have been taken quite arbitrarily. All these approximations are adding up to the SIA assumptions, and could have been avoided by using a formulation of the SIA relative to the surface and basal flow velocity (Zorzut et al., 2020). Since this method is more straightforward, and that the authors have all the data needed (ice velocity can be downloaded here <https://its-live.jpl.nasa.gov/>), I would suggest to add this method as a comparison. This would indeed provide an assessment of the differences between two SIA formulations, in addition to the PISM inversion. Finally, the overall aim of this paper was to choose an appropriate drilling location. There are very few details on how this location was chosen. I think that having a paragraph within the result and discussion section about the choice of drilling location would make the story around this paper much more interesting.

Specific comments.

L20-25. Something needs to be stated here about the Consensus estimate of 2019. How

does the different model outputs agree over Müller ice cap.

L28-30. It is not clear to me how using the shallow ice approximation to calculate the ice thickness is new (cf. ITMIX; Zorzut et al., 2020)

L40. Which dates where the OIB data acquired ? How does it compare to the date of the Arctic DEM ? How do you account for the offset between the different surface dataset ?

L49-50. The misinterpretation at the ice divides is not due to the fact that Farinotti provide a global product, but to the flowline approach that is used, as it was mentioned earlier in the paragraph.

L51. Why do you choose a different resolution for the SIA and PISM ? The Farinotti et al. data do not have missing values, but really thin ice thicknesses near the ice divides. How did you choose to mask out the dataset of Farinotti ? Can you provide a Figure of the interpolated bedrock topography product?

L55. Why did you choose to solve for a non-sliding version of the SIA ? Using ice velocity measurements from ITS_LIVE would allow you to better calculate ice flux, and indirectly accounts for the amount of deformation within the ice (i.e what portion of the flow is caused by either sliding or deformation). See Zorzut et al., 2020 for more details. Finally it is not clear how you perform the inversion. Do you calculate ice thickness at the basin scale or just along the line?

L70. How does the a, b and c values varies along the Icebridge line ? Can you provide us with an histogram, and statistical analysis on the variations of these parameters?

L82. Why did you choose a 250 m as a smoothing parameter ? Can you provide a map of the unsmoothed and smoothed map ? Why not choosing a lower resolution version of the Arctic DEM?

L95-103. This section is unclear to me. Why do you take a uniform SMB instead of using average In-situ data value or HIRHAM outputs?

L105-106. Can you provide more insights on what physics is solved into PISM, how does it differ from the SIA, and why would that be useful to use it in addition to the SIA?

L159-170. As it was suggested by Bamber et al 2000, the SIA suggests that the flow is linked to surface slope at the scale of multiple ice thicknesses. Hence the smoothing distance of the surface slope should carefully chosen. Slope processing is a well-known SIA limitation (Zorzut et al., 2020, Farinotti et al., 2009; ITMIX-1; ITMIX-2), hence I would recommend the authors to run their model again with better slope smoothing parameters, before comparing with PISM. It is obvious from Figure 2 and Figure 3, that the spatial distribution in ice thicknesses is completely dominated by artefacts in the surface slopes.

L173-175. This sentence is not clear here. Do you mean that information on ice velocity will only provide limited additional information ? Please rephrase the sentence to make it clearer. Moreover, despite what the author says, I will argue that the ice velocity will provide crucial information on the distribution of the ice thicknesses. Indeed, using a formulation of the SIA that includes the ice velocity (Zorzut et al., 2020), will allow you to easily account for the amount of internal deformation vs sliding, which can be important to mitigate for with Müller ice cap (Copland et al., 2017). This will also reduce the strong hypothesis that are made here for the calculation of the flux Q , which depends on the SMB values, that was defined in quite an arbitrary way.

