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 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 choosed to compare these two approaches. Furthermore, there is not much description of the PISM model, which prevent the readers from understanding the physic of this approach without referring to other manuscript. 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, to choice of appropriate smoothing parameters needs to 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 in an arbitrary way. All these approximation, 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 is very few details on how this location was choosen. 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.

Thank you for the review and thank you for pointing us towards Zorzut et al.. We have chosen to include the Zorzut et al. methodology in our revised manuscript, please see further comments below.

First, we would like to emphasize that a primary goal of the study is to document our reasoning for choosing this particular candidate for a new drill site. We understand that we have not been clear enough about this in the manuscript, and will make sure to put extra emphasis on this in the revised manuscript. Different models were chosen to strengthen the confidence of the drill site candidate. We find that both models agree very well in the area of interest (high elevation, near ice divide, little horizontal flow), which we will make sure to be more clear about in the revised manuscript.

Since the form of the manuscript is a Brief communication we do not have space to give a thorough explanation of PISM, why we have referred to the relevant literature. Further, PISM is a well-known and well-established model, and it would be out of the scope of this manuscript to describe more thoroughly what physics is being solved in PISM. Instead we briefly outline the main features of the model and how that makes it different from a pure SIA based approach:

The reason for choosing more than one method is to increase our confidence that the large thickness at a drill site candidate region is not just a fluke of the method. We have special method requirements because we are on an ice cap and are particularly interested in the highest elevations (near the ice divide). This narrows down our options as it makes flowline methods unsuitable, and standard SIA methods have issues at the ice divide. Therefore we would like to use a method that can be calibrated to perform well in the region of interest - which is exactly what we have developed.

In our comments below a further explanation of the SMB, slope, and smoothing thereof is given. We understand that calling our method a SIA inversion may have been a misnomer, which is why we have chosen to rename it to “semi-empirical ice thickness model (SEITMo)”.

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.

Up to 5 models are used in the consensus estimate. In the case of Müller ice cap the consensus estimate is based on three different models. The consensus estimate has a RMSE of 151 m compared to the Operation IceBridge ice thickness in the non-shaded part of Fig. 2. The three different models used in the consensus estimate have RMSEs of 193 m, 195 m and 198 m, respectively. Furthermore, it should be noted that they all do suffer from gaps and in-consistencies between the different Randolph Glacier Inventories.

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)

SIA itself is not new. The new insight is the realization that a SIA-like model is equivalent to a multiple linear regression. So, it is a statistical rather than a physical insight. The statistical point of view makes it very clear how multiplicative errors in SMB or Q can be ‘absorbed’ in the intercept. This is a major benefit as SMB is often poorly constrained. Further, not all data is of equal quality, and the regression approach allows the method to adjust the coefficients so that it places less weight on noisy data in order to improve the fit. The coefficients may therefore not be exactly consistent with SIA theory, but the end result is more useful in practice. We have chosen to change its name to "semi-empirical ice thickness model" to emphasize the difference to a pure SIA based approach.

The statistical point of view can also be seen as a bridge to the inevitable machine learning methods which have already started to appear in the literature, and which we will also see more of in the future.

One minor point: We note that Zorzut et al. could have benefitted by reformulating their model as a regression problem. This would have prevented a brute force parameter search for their sliding parameter. Thus the statistical insight can be immediately practically useful for other SIA like approaches.

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 ?

OIB is from 30-03-2017 and ArcticDEM is a mosaic and is thus based on data from multiple dates (we use release7/v3). We only use the ice thickness from OIB, assuming that ArcticDEM is the true surface elevation. The overall thickness of the ice cap may have reduced since then, but the relative orders of magnitudes when excluding the outlet glaciers should be the same.

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.

We will make sure to change this in the new manuscript.

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?

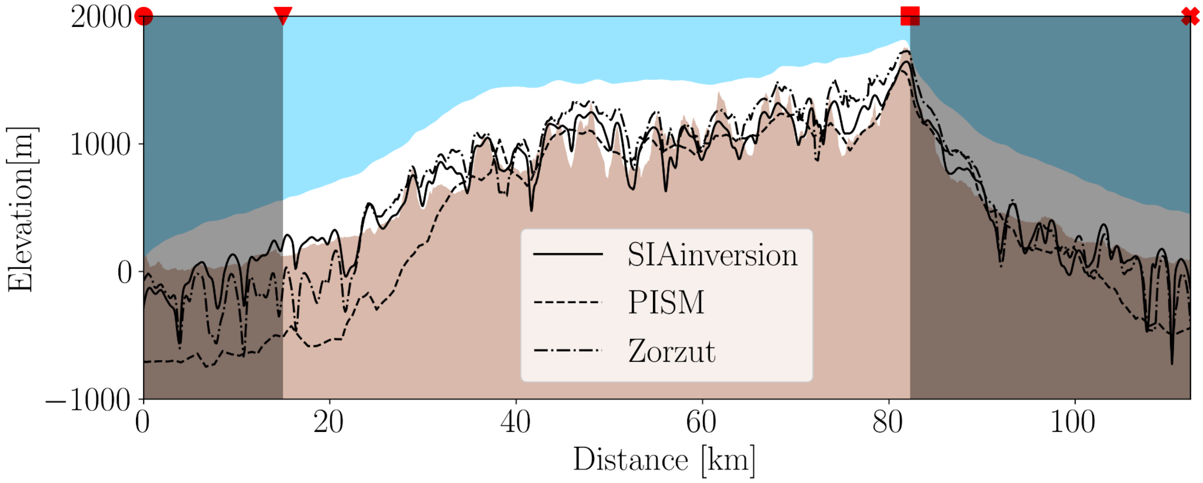
We chose different resolutions as it was not possible to run PISM on the same high resolution due to computational limitations. We use the consensus thickness estimate from Farinotti and merge the different glacier inventories using GDAL (gdal_merge). Since the consensus estimates are based on the different Randolph Glacier Inventories (RGIs), there is not always a perfect match at the borders in between the inventories after the products are merged. This is why the output from gdal_merge gives some single pixel gaps in between some inventory items. These gaps are filled when interpolating the data onto the desired grid. The Farinotti bedrock is only used as a starting guess, and the treatment of these edges is therefore unimportant for the further analysis.

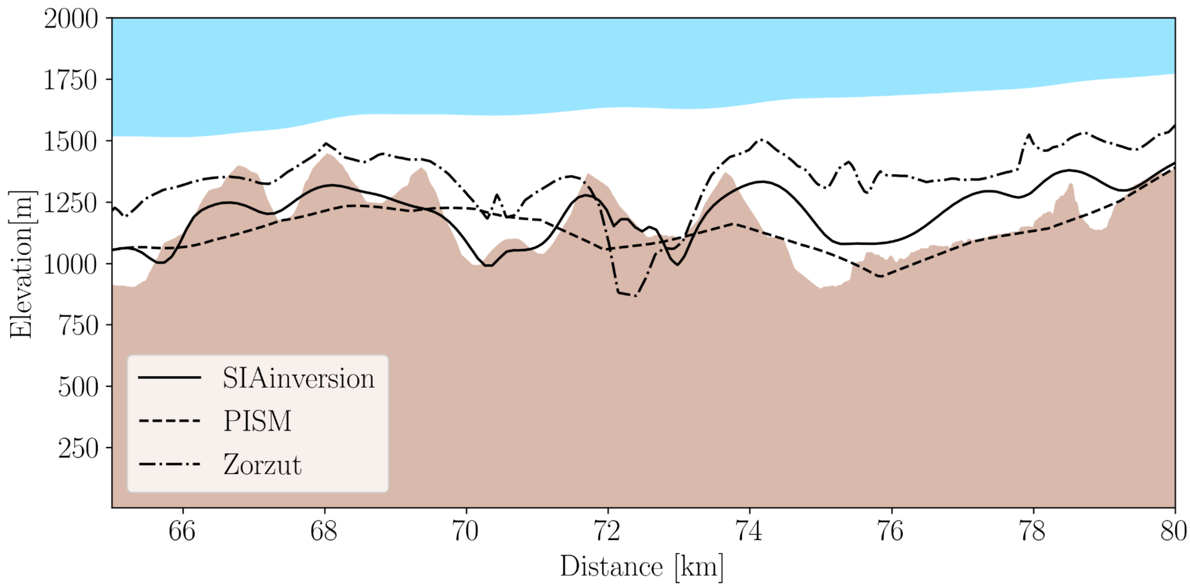
L55. Why did you choose to solve for a non-sliding version of the SIA ? Using ice velocity measurements from ITS_LIVE would allow you to better calculate ice flux, and indirectly accounts for the amount of deformation within the ice (i.e what portion of the flow is caused by either sliding or deformation). 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?

We motivate our semi-empirical formulation using a non-sliding SIA model. However, we ultimately let the regression algorithm find the best fitting parameters which may not exactly correspond to the non-sliding SIA. We have also tried a version of the SIA inversion where sliding was explicitly included, and where we added velocities obtained from feature tracking to the input datasets (MSc thesis). We however found that this did not provide a substantially better fit to the data. This is not very surprising from a statistical perspective as ice flux and surface velocities are highly covariant, and therefore adding ice velocities as a predictor to a model that already includes ice flux will yield little additional information.

The inversion is performed using standard multiple linear regression (scipy.linalg.lstsq), by tuning to the OIB ice thicknesses in between the triangle and the square in the figures.

In the new manuscript we also test the Zorzut approach. As can be seen in the following figures, Zorzut agrees overall with both the semi-empirical approach and PISM. However, velocities are very small near the top of the ice cap, and therefore small errors will lead to a very poor signal to noise ratio. This is critical for the Zorzut method as velocity errors propagate directly to the thickness estimates. We therefore don’t expect the Zorzut approach to work well in exactly the region that is of interest as potential ice core sites. Indeed, Zorzut greatly underestimates the ice thickness (see around 74-78 km in the zoom-in figure). In our case where we are interested in finding a good candidate site to drill a full depth ice core, it is very important that we resolve this particular area well. This is of course due to the fact that little melt is taking place here due to the high elevations, and the horizontal flow affecting the stratigraphy in the ice core is minimal.





In Zorzut et al the sliding parameter can be reformulated as a pre-factor to the entire SIA expression. When we then take the logarithm (following our semi-empirical approach) then this pre-factor becomes the intercept in the regression. This illustrates that having the intercept as a free parameter can account for some sliding (in addition to allowing for multiplicative errors in SMB).

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?

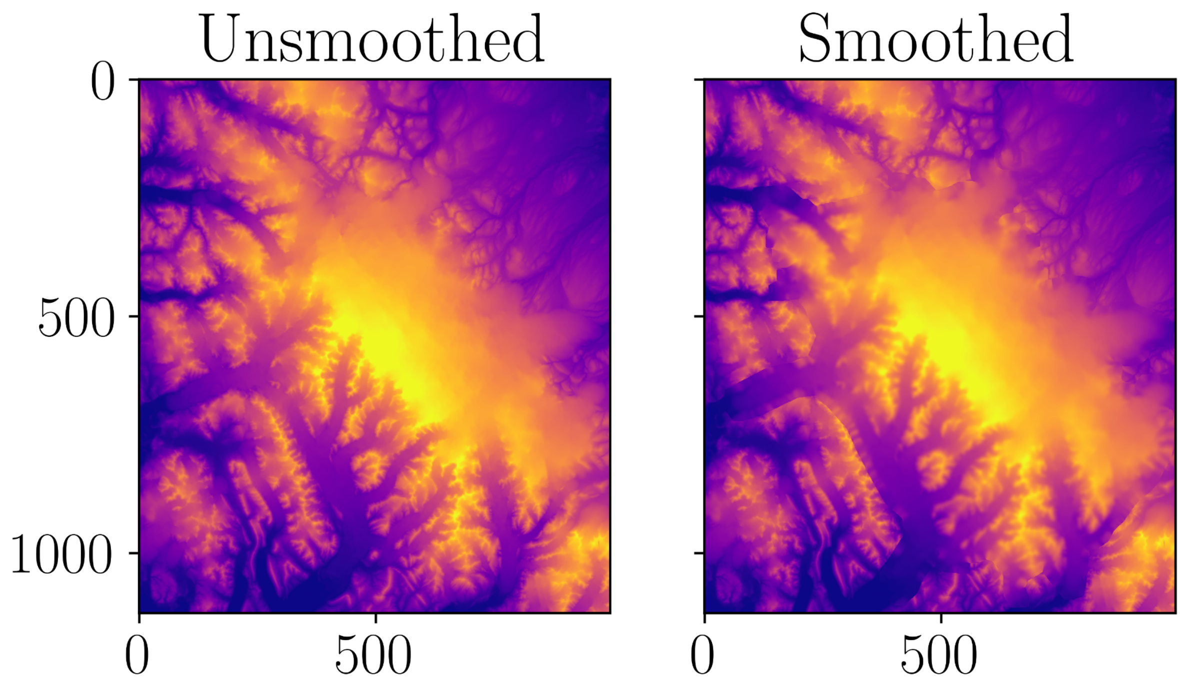
The tuning parameters a, b, and c are constant in space. One set of parameters is obtained from the part of the OIB line marked between the triangle and square in Fig. 1 and 2.

We have the three columns of numbers along the ice bridge line: thickness, balance flux, and terrain slope. We take the logarithm of these columns, and then do a standard multiple linear regression with an intercept with log(thickness) as the predictand. The three coefficients from the regression are the a,b, and c constants.

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?

A gaussian smoothing is not equivalent to choosing a coarser resolution product. The smoothed DEM has more well behaved surface slopes which we need for the semi-empirical approach.

The smoothing we apply is minimal and is only there to ensure that there are no sinks in the surface elevation. The shallow assumption in the SIA means that you cannot expect such models to resolve structure on horizontal scales that is much finer than the typical thickness. Also from this perspective a smoothing with 2σ =500m seems reasonable. It is possible to find the optimal smoothing (=resulting in the smallest misfit) using a brute force search given the very low computational cost of the semi-empirical model. We have not done that in this paper.



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?

We could also have used in-situ data or HIRHAM outputs, which we have also done (MSc thesis), but the key point is that the method is insensitive to any multiplicative errors in the SMB assumptions. And since we know that HIRHAM is overestimating the mass balance, and in-situ measurements are sparse on the ice cap, it is key to us that we can use a model which is not dependent on SMB assumptions. Including SMB as input parameter to the flux calculations and thereby also to the model, has almost no influence on the modelled ice thicknesses (MSc thesis).

We do not have in-situ values from the ice cap, the in-situ values presented in the paper are from White Glacier marked in the black polygon in Fig. 1, and cannot be assumed to be representative for the entire ice cap.

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?

As mentioned in L 105-108 PISM is solving a coupled SIA and SSA model. Due to the form and restrictions of a brief communication we refer to other literature for more insights into the methodology. In the SIA, ice flow can be determined purely from local conditions. This is why a SIA derived thickness can be calculated from local slope and flux only. The SSA is inherently a more non-local flow approximation, meaning that the ice thickness in one point can be influenced by stresses and ice flow further away. This is a major difference to most ice thickness estimation methods.

One of the aims of using more methods is to get more confidence in the choice of drill site candidate, which we will make sure to emphasize in the revised manuscript.

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 choosen. 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.

It is possible to find the optimal smoothing (=resulting in the smallest misfit) using a brute force search given the very low computational cost of the semi-empirical model. We have not done that in this paper. We have, however, carefully chosen the gaussian smoothing with 2sigma=500m with exactly this in mind. We will discuss our choice in more detail in the revised manuscript.

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, despites 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.*

We think that this is a misunderstanding. We agree that ice velocity is useful (as clearly demonstrated by Zorzut et al.). What we are trying to say is that it provides little *additional* information when we already include horizontal flux as a predictor. We will revise the manuscript to be much more clear on this point.

As mentioned to L55:

We motivate our semi-empirical formulation using a non-sliding SIA model. However, we ultimately let the regression algorithm find the best fitting parameters which may not exactly correspond to the non-sliding SIA. We have also tried a version of the SIA inversion where sliding was explicitly included, and where we added velocities obtained from feature tracking to the input datasets (MSc thesis). We however found that this did not provide a substantially better fit to the data. This is not very surprising from a statistical perspective as ice flux and surface velocities are highly covariant, and therefore adding ice velocities as a predictor to a model that already includes ice flux will yield little additional information.

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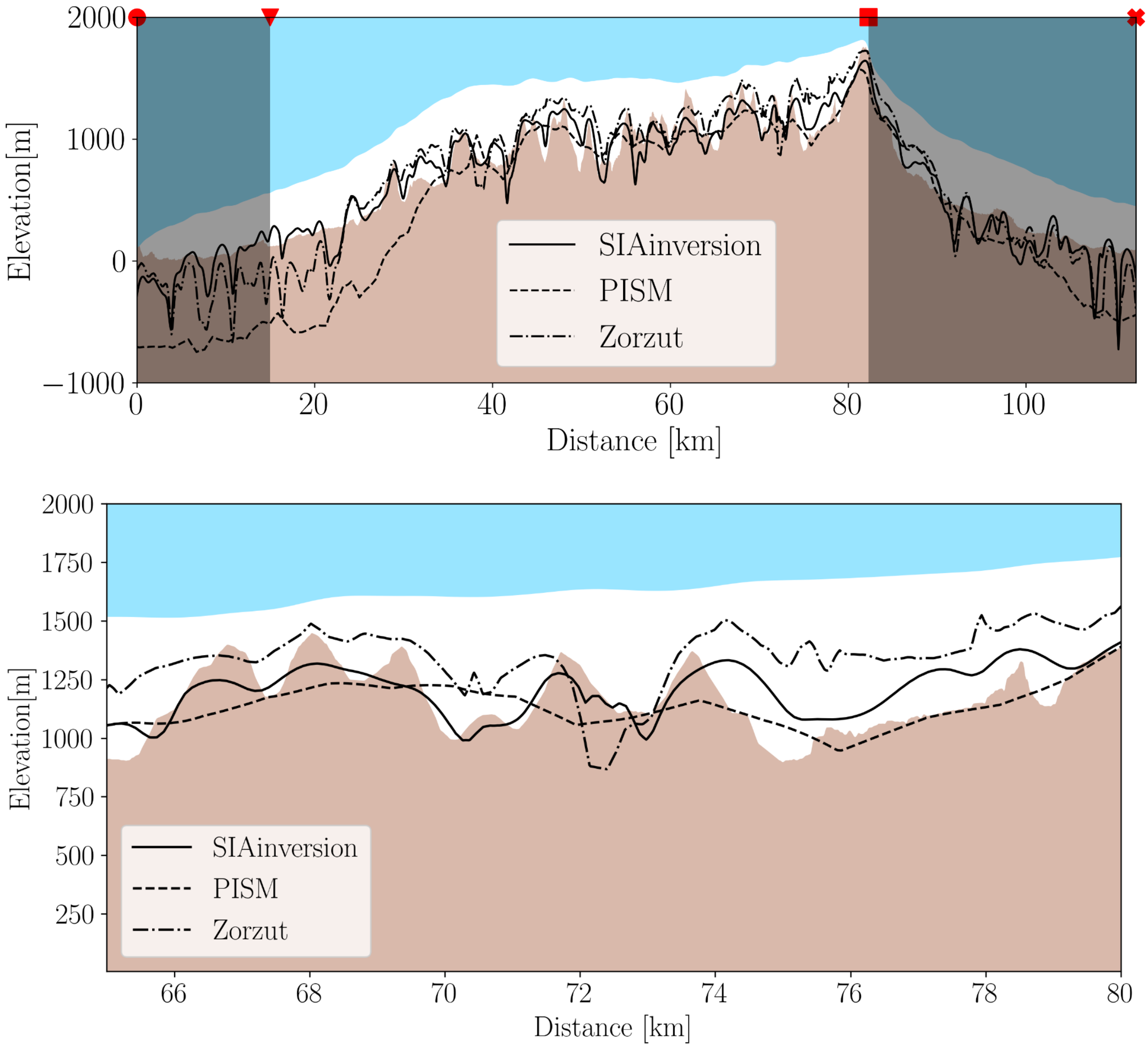

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