Dear Harry,

We have carefully revised the manuscript according to comments and suggestions from the two reviewers.

In the revised manuscript a third method (Zorzut et al. 2020) has been added as suggested by reviewer #2. Furthermore, we have made sure to exclude the outlet glaciers from the discussion and comparison of the three methods, as PISM is not tuned to this area at all as pointed out by reviewer #1. This is also means that we have made sure that the focus is on the main ice cap and the area of interest in the discussion of our results.
We have also made sure to acknowledge the limitations of our PISM set up, i.e. variable relaxation parameter and constant SMB, and how this issues could have been solved but with a high computational cost.

Finally, it should be clear from the revised manuscript that the three methods (or especially PISM and SEITMo) agree quite well on the ice cap.

Below a point-by-point reply to the two reviewers is found. In blue are the added comments with what exactly we have done if it differs a bit from our previous reply, or if we have found that it needed further comments.

Kind regards,
Aslak and Ann-Sofie

Reviewer #1:

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.

Thank you for the review.

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. There is no need for this to also yield new 'insights'. This is why we eventually decided to submit this as a

Brief Communication. -Our editor informed us that the site selection would be sufficient to warrant this type of manuscript.

Nevertheless we hope to convince you that our new regression based semi-empirical approach is useful. The key insight is that fitting a SIA-motivated semi-empirical model to estimate ice thickness is equivalent to fitting a simple linear regression model. This may not give insights to the physical processes, but we demonstrate that it performs better. A 'semi-empirical' approach is of course more statistical, which is both a pro and a con of the method compared to more physical approaches. 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.

Lastly, we realize that we have not put enough focus on the fact that the two models actually agree very well in the area of interest, which we will make sure to do in the revised manuscript.

In the revised manuscript we have made sure to point out how PISM and SEITMo (former SIA inversion) agree very well in the area of interest. We have also included a zoom-in of the area of interest in Fig. 2 to emphasize this.

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.

We understand that calling the final model "SIA inversion" might be taking it too far. Therefore, we have changed the name of it to "semi-empirical ice thickness model (SEITMo)". The motivation for not calibrating A and n directly is that the aim is not to achieve the true value of these, but rather have more freedom to achieve the best possible results on the ice cap near the ice divide.

Theory will only match reality if:

1. our physical description is perfect.

2. The input data (H_icebridge, Q and α) are perfect.

We know that neither of those two conditions are true. The SIA model is a simplification, and the assumptions going into it are clearly not valid near ice divides (the key region of interest in this study). Further, no input data is ever perfect. The different input data will be of varying quality and therefore should not necessarily have equal weight in the final thickness determination. It is therefore not surprising that the best fitting parameters will deviate from theory.

We note that A. Zinck has tested a physically constrained Bayesian calibration of a SIA model in the cited MSc thesis. We decided to not include this model in this manuscript because 1) It does not perform better. 2) it has higher complexity and cost. 3) Something similar has already been done in the literature [e.g. Brinker et al. 2016]. 4) manuscript length.

Please also note: In our revised manuscript we have decided to include a comparison to a more clean SIA based method that used velocities instead of Q (Zorzut et al., 2020). We include this method to satisfy reviewer2, and because it is a similarly fast method to our semi-empirical approach.

In the revised manuscript we have made sure to rename the SIA inversion to SEITMo, since the connection to the SIA is limited with the number of free parameters in our model. Furthermore, we have included a third method (Zorzut et al., 2020) which is mentioned is a more clean SIA based method. We demonstrate in Fig. 2 how the velocity dependence of this method leads to an underestimation of ice thickness in the vicinity of the ice divide.

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.

We agree that mass balance is important for some models like the PISM. We also agree that the degree of tuning is not the same for both approaches, and so we also agree that there are limitations to how strong conclusions we can draw from the degree of misfit. We therefore offer to simply completely remove PISM from the manuscript. However, it is important to realize - and we may not have communicated that clearly - that the computational cost of the PISM approach is so incredibly huge that it places severe limitations on how large a parameter space it is possible to explore. The PISM inversion (with 2 parameters) in the current manuscript took months. We are talking 3x4x10x2000 model years ($N_E x N_{phi} x N_{iter} x N_{yrs}$). Adding three variations for two additional parameters for a mass balance offset and scaling would take ~9 times as long. This is a very real practical concern that strongly favors the semi empirical approach (or approaches like Zorzut et al.). So we would like to keep the discussion of PISM in the manuscript as this could be important for other people. We will make sure to acknowledge in the revised manuscript that better fits could be obtained by also tuning the SMB.

In the revised manuscript we have made sure to acknowledge that better fits can be obtained by tuning the SMB as well, but that it comes with severe computational costs (see L. 206-216).

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 conversion 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 decription 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.

It is correct that the bedrock below 500 m is not modified after each iteration. The bedrock shown in Figure 2 and 3 is the end result after the last iteration, so it is not equal to the Farinotti bedrock but the bedrock after 2000 model years (calculated as ArcticDEM minus thickness). However, we agree that we should not include this part of the domain in the comparison. It should be noted that the semi-empirical approach is also not tuned to this area, but only the non-shaded part of Figure 2.

Low elevations have too much melt to be useful ice core sites. Furthermore, the ice flow in these areas is a complicating factor when an ice core record has to be put into a climatological sense. This informed our design choices. We are simply not interested in good performance at low elevations, and thus chose to not adjust K for z<500m and thus did not have to worry about getting ablation right. This is critical for performance as it reduces the parameter space we have to search.

We have excluded the area below 500 m from our discussion and kept the focus on the main ice cap. We acknowledge that the overestimation on the outlet glaciers is caused by our choice of relaxation parameter.
From L. 173-174: *This overestimation is due to the fact that the bedrock is not adjusted on below 500 m due to the relaxation parameter K. Hence, the model is not expected to do well in this area, but only on the main ice cap.*

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.

We agree that the SIA in it's theoretical form is not insensitive to mass balance. However, our semi-empirical approach is insensitive to multiplicative errors in SMB. See also our comments above.

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

We will make sure to be more specific here and change it to:
"Ice of great thickness and minimal horizontal flow is desirable to increase the probability of reaching ice dating back..."

This statement is based on experience from the ice core experts in Copenhagen.

Changed to (L. 15-18): *Therefore, knowledge about ice thickness and flow is important as ice of great thickness and minimal horizontal flow is desirable to increase the probability of reaching ice dating back to the Innuitian ice sheet, referring to the ice sheet in between the Laurentide and Greenland ice sheets during the last glaciation.*

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

We agree with that. Thank you for pointing it out. In the revised manuscript we will swap the two sentences around and make sure they are better connected with a better flow.

The section has been swopped a bit around to ensure a better flow. See L. 13-22

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

Changed.

L21: Please remove the obsolete bracket

Bracket removed.

L22: Remove the obsolete "in"

Done.

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.

Changed to Easting and Northing and the UTM zone has been added to the caption.

The SMB exponents were lost during upload in the processing by Copernicus. We will carefully verify that they are not lost in the next version.

L27: "differs" --> "differ"

Changed.

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.

We disagree. It must be fair to state that the new method is insensitive to errors in the different factors, if the resulting biases are automatically calibrated away. This is at least what we mean when we say that it is insensitive. We also think we very openly and transparently explain that the insensitivity arises from how biases are calibrated away.

L36: "why" --> "and"

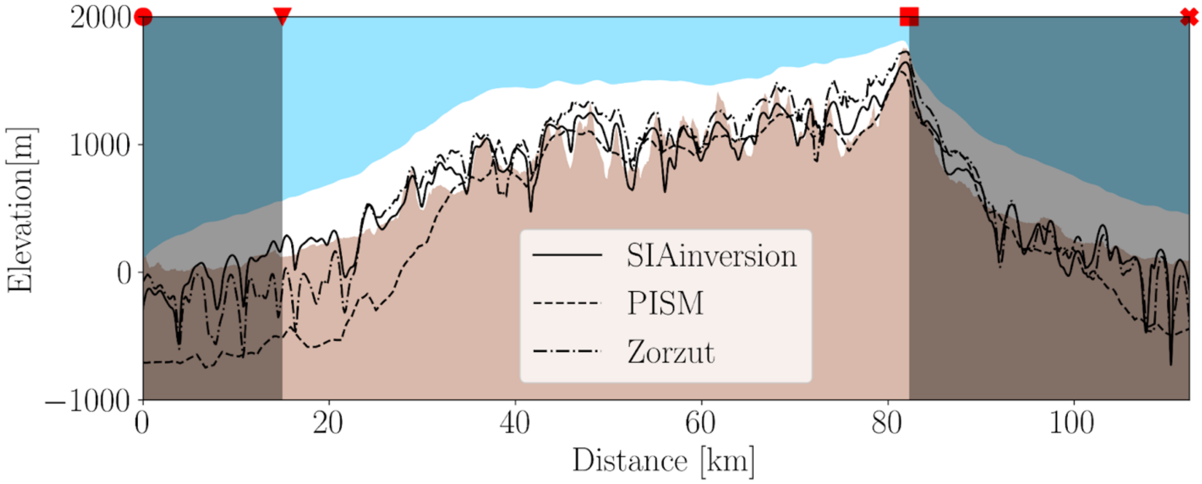
Changed.

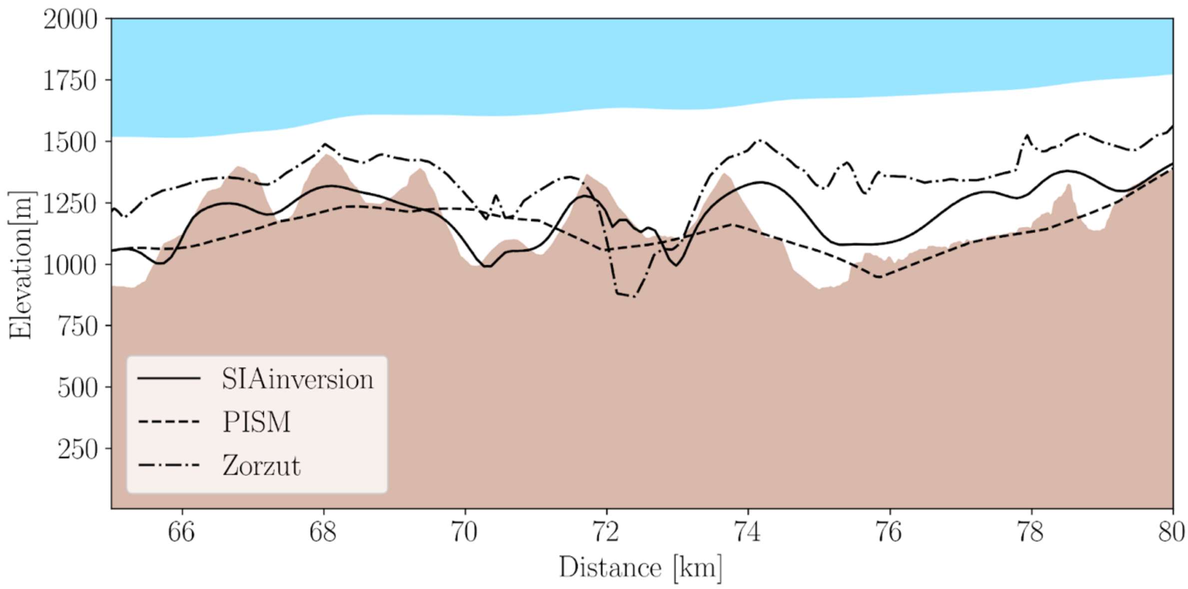
Section 2 (or Introduction): I am missing some information on ice velocities of the ice cap. I suppose these data could for example be extracted from online resources. A source like Its_Live (https://its-live.jpl.nasa.gov/) could potentially be useful. It would give an idea on potential sliding rates 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.

We have already tested a method using surface velocities in the cited MSc thesis. However, we found that this did not really improve the fit, and so we decided to exclude it from this manuscript to keep it short and simple (we used a custom velocity product that would require extensive documentation.)

As mentioned previously we will add the SIA method presented in Zorzut et al., 2020 which also includes sliding. But as you will see, this method also did not provide new insights or added value.

Again, we emphasize that our focus is the top of the ice cap where velocities are small. Small velocities result in a poor signal to noise ratio. This data quality issue severely hampers how well we can expect any method relying on surface velocities to perform. This is indeed what we find using the newly added Zorzut et al. 2020 method. At very high elevations we simply trust balance flux to be more informative than ice velocities.





As mentioned, we have included the Zorzut method as well in the revised manuscript. As demonstrated in Fig. 2, this velocity based SIA inversion underestimates the ice thickness in the area of interest. So when the aim is to aid for a possible drill site the computationally fast SEITMo is preferred.

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.

The slope threshold was chosen simply just to prevent any sinks in the surface elevation, as our method of calculating the balance flux cannot handle sinks in the surface. It is possible to find the optimal smoothing of the DEM (=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. This also implies that there are very few areas where the slope threshold is actually used. We will discuss our choice in more detail in the revised manuscript.

We have made sure to acknowledge this and added a few lines on this (see L. 96-99).

L88: ”why” --> ”which is why”

Changed.

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.

The method is of course sensitive to the correct spatial pattern of Q. We have, however, compared the results of using a balance flux based on a constant mass balance against the HIRHAM SMB (MSc thesis). The ice thickness results were virtually identical (for the semi-empirical model). So, in practice this has little effect. Here it may be of interest to look at the mass balance vs elevation plot in figure 1.

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.

We do not have sufficient info on past SMB to apply this here. But we will of course make sure to acknowledge this in the revised manuscript.

We have acknowledged this issue in L. 121-123.

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.

It is correct that it shuts down the inversion process at low elevations. However, this is by design as explained above. We will be more careful about our PISM result interpretation and exclude the areas below 500 m from the comparison.

We have as mentioned excluded the area below 500 m from our discussion and also added a zoom-in in Fig. 2 to emphasize the area of interest.

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

Changed to: "the following possible combinations of parameter values"

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.

We agree that this is important, and this makes the comparison unbalanced. However, as explained, computational cost limits how large a parameter space it is feasible to search. We will make sure to acknowledge in the updated manuscript that this comparison is not completely fair, and PISM would undoubtedly perform better if we also tuned SMB. We will also explain the computational cost issue.

In our discussion of PISM (L. 206-216) we have made sure to acknowledge and explain this issue. Further, we have made sure to keep our focus in the revised manuscript on the area of interest, where we can see that PISM and SEITMo perform equally well.

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.

We agree that the different models are not that different. This is actually very good news as this leads to agreement concerning which is the best drill site candidate. This is a major reason for estimating the thickness in multiple different ways. We realize that we did not make this point sufficiently clear, and will put an emphasis on this in the updated manuscript including a zoom-in of the cross section figure (Fig. 2) of the ice cap top.

Same comments as before. Also, the calculated RMSE is now based on the area of interest and not the entire flight line. From this we can also see that PISM and SEITMo perform equally well.

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.

You are correct. The calving criterion mostly impacts the margin, whereas we are only concerned with the thickness at high elevation. By using K=0 for z<500m we save computational time as we do not have to include parameters for the calving approximation. However, the calving criterion still has a big impact on the time step chosen by PISM. We dont worry about misfit at the margin, and this allows us to pick a criterion that saves us computational cost.

You are correct, and we have removed this from the revised manuscript. See also our comments above on K.

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.

We agree that the low elevation misfit of PISM is not very informative. We intend to focus our misfit discussion to higher elevations. After all that is also our focus from a potential drill site perspective.

It is correct that tuning the HIRHAM SMB would have yielded better results at lower elevations. However, as mentioned before, it is not possible for us to test the full parameter space. Since our area of interest is around the top of the ice cap we do not worry too much about this.

See our comments above. We have acknowledged the SMB issue and excluded the area below 500 m from the discussion.

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.

We agree that it would have been nice to tune PISM SMB. It is computationally very expensive (as argued above). With this in mind we disagree that we “should” have tuned SMB too.

This limitation should of course be kept in mind in any comparisons. The comparison is not completely fair. We intend to further and more explicitly acknowledge this in the revised manuscript.

Regarding the SIA inversion (or semi-empirical ice thickness model) you are right that the SIA is not insensitive to the SMB, but the semi-empirical ice thickness model is, as mentioned previously.

See comments above and the updated discussion in L. 206-216.

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

Changed.

Reviewer #2:

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)”.

As promised we have included the Zorzut methodology in our revised manuscript. Furthermore, we have ensured that the focus is on the main ice cap and not the outlet glaciers where PISM is not tuned at all. From the updated manuscript it should also be clear how SEITMo and Zorzut differ in methodology, and what consequences it has.

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.

In the revised manuscript we have changed this and included the following sentence from L 55-56: Such misinterpretations are to be expected since the Farinotti et al. (2019) bedrock is based on a flowline approach and relies on the Randolph Glacier Inventories.

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?

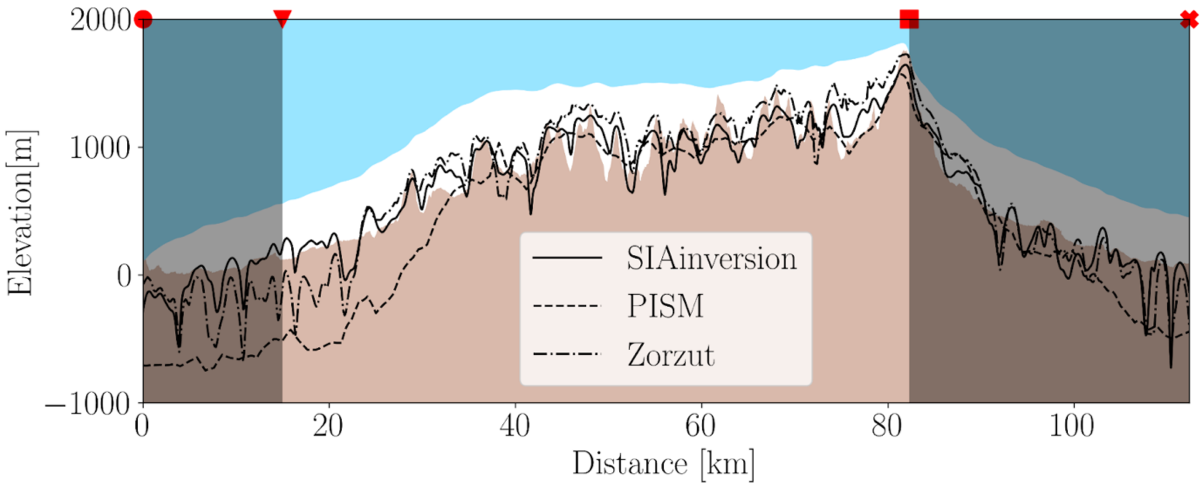
We chose different resolutions as it was not possible to run PISM on the same high resolution due to computational limitations. We use the consensus thickness estimate from Farinotti and merge the different glacier inventories using GDAL (gdal_merge). Since the consensus estimates are based on the different 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.

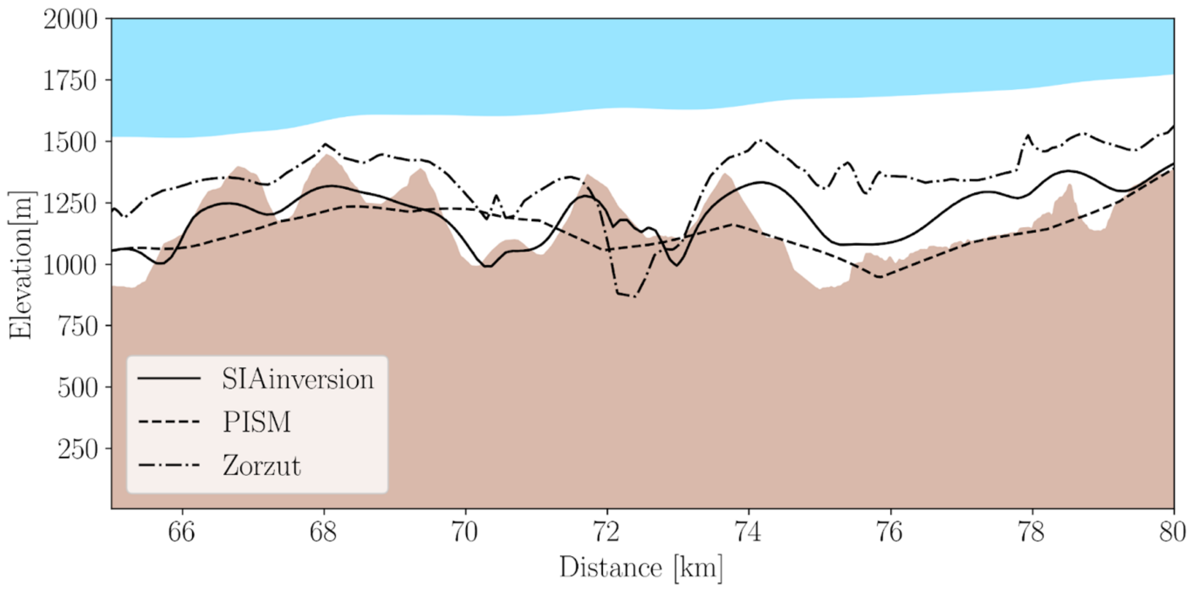
L55. Why did you choose to solve for a non-sliding version of the SIA ? Using ice velocity measurements from ITS_LIVE would allow you to better calculate ice flux, and indirectly accounts for the amount of deformation within the ice (i.e what portion of the flow is caused by either sliding or deformation).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

As mentioned we have included the Zorzut methodology in our revised manuscript. In Fig. 2 the consequences of the strong dependence of the velocities are clearly visible, with a clear underestimation of the ice thickness in the vicinity of the ice divide, which is the area of interest in terms of ice core drilling.

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?

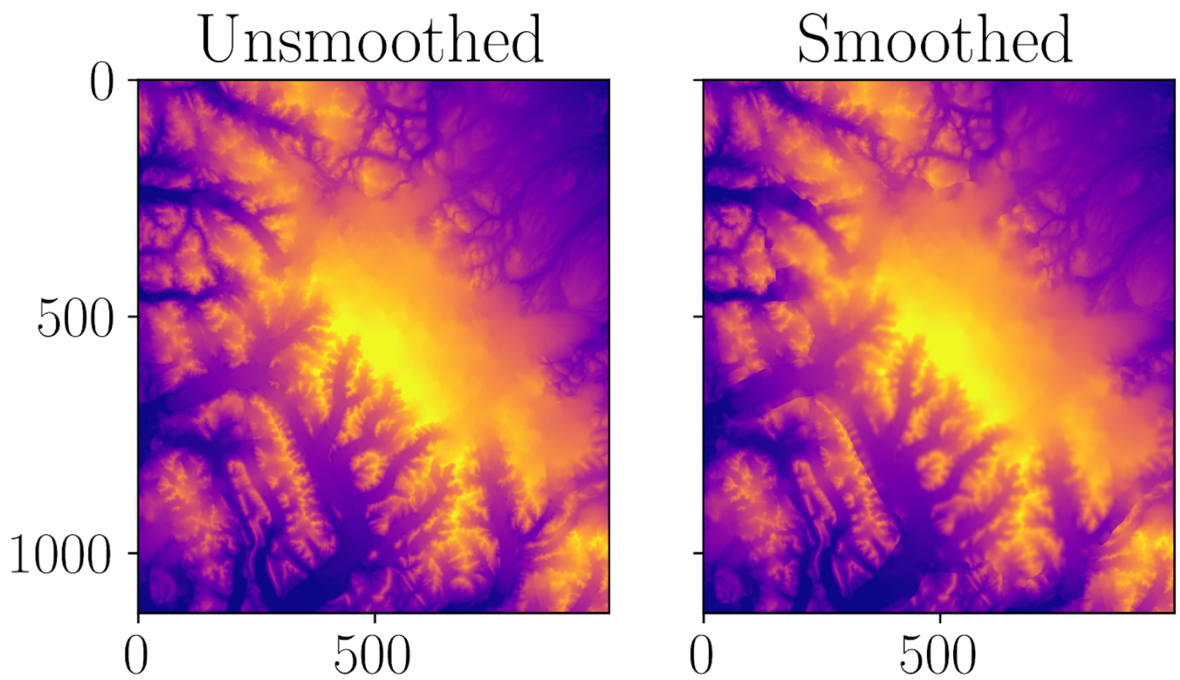
The tuning parameters a, b, and c are constant in space. One set of parameters is obtained from the part of the OIB line marked between the triangle and square in Fig. 1 and 2.

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 $2\boldsymbol{\sigma}$ =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.



In the revised manuscript we have elaborated on out choice of smoothing (see L. 96-99).

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.

We have added further comments on this in L. 96-99.

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.

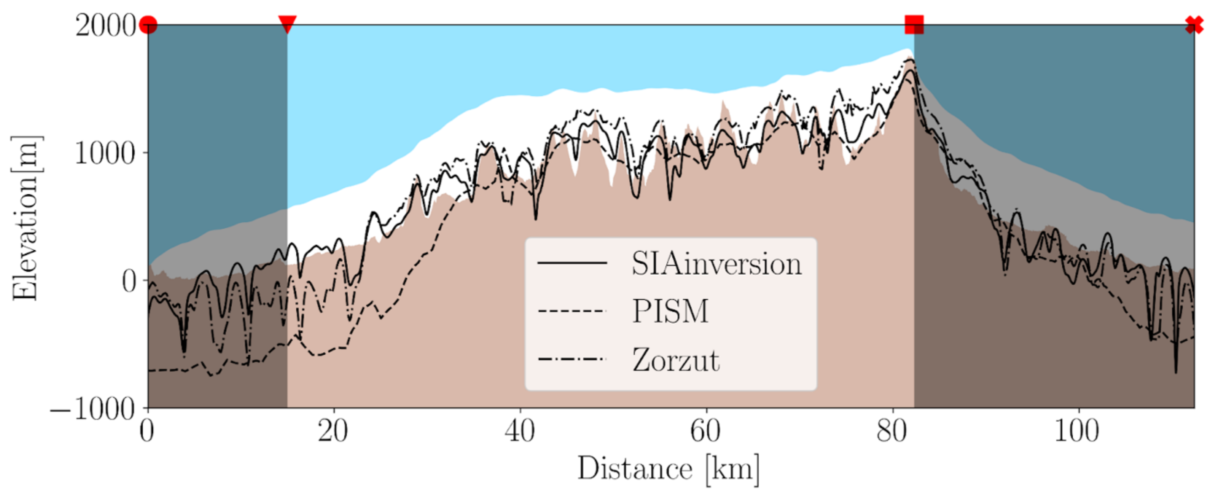
We think that this is a misunderstanding. We agree that ice velocity is useful (as clearly demonstrated by Zorzut et al.). What we are trying to say is that it provides little *additional* information when we already include horizontal flux as a predictor. We will revise the manuscript to be much more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

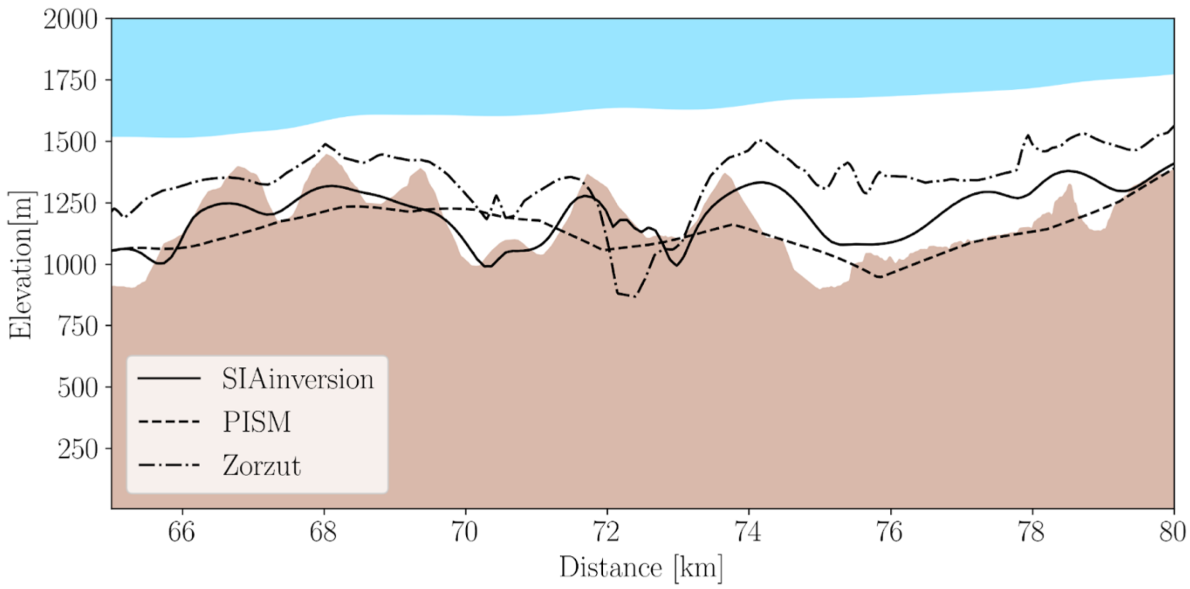
As mentioned to L55:

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.

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.





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