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Interactive comment on "Geothermal heat flux and basal melt rate in the DomeC region inferred from radar reflectivity and thermal modelling" *by* Olivier Passalacqua et al.

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The authors do a nice job leveraging available data sets and modeling capabilities to provide constraints on the basal melt rate near Dome C. The methods applied seem reasonable and the results seem to advance how to choose a site that is most likely to have preserved more than million-year-old ice. While this is good work, the way the manuscript is written makes it hard to follow, and I think that it does not communicate results in a way that they can be readily used in the broader community effort in the search for an "oldest" ice site. I hope that the authors can both restructure and reword much of the paper in order to make this work more accessible. I do not know exactly

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how this should be done but will provide some suggestions and identify as many of the starting with reordering and reconfiguring the sections to make sure that each header offers something logical to understand the work, and that text in each section is complete. I recommend that all the authors carefully review the revised manuscript before publication.

We would like to thank the reviewer for the constructive comments made on our work. We agree that the structure of certain sections need to be modified to make the article more clear-cut. In particular, the introduction section now benefits from a better explained 'oldest ice' context, and section 2 (method) and 6 (discussion) were reordered. We also agree with almost all the minor comments and tried to correct the text accordingly.

The present version of the paper has been corrected by a native speaker for english wording.

When needed, the old sentences are colored in green with corresponding line, the new ones in red.

I strongly recommend that the authors change the terminology from "geothermal heat flux" to "heat flux" or "geothermal flux"; geothermal heat flux is redundant. (In the same way that 'thermal heat model' would be redundant.) I know the chosen term is often used but hope that stating what is meant more directly will also help the flow of the paper since the term is used throughout the paper.

That makes sense, and we changed the terminology for "geothermal flux".

Introduction to heat-flux estimates from Antarctica doesn't differentiate between east and west, which is confusing the way it is written.

We understand that the text was potentially confusing. Now we focus specifically on the method to assess the GF under ice sheets in general (§1.2 GF assessment methods unders ice sheets), and we specify East or West Antarctica when needed.

Section 2.1.2 header is misleading when referring to "water circulation", and I am not sure that it is only finding a new word that is needed - is there really a chance of a significant basal hydrological system here? If so, even if a basal spot has not water evident is there a way to constrain that water was not routed through this region?

The idea here is to check if wet and dry areas really correspond to thawing and frozen areas (title changed as follows) :

2.4.1 Correspondance between wet and thawing areas

The true basal hydrological system cannot be observed easily and we are limited to theory. If there is no basal water circulation, Zirizotti map is just enough, but this would be a too optimistic assumption : we need to investigate the pessimistic case where dry areas are in fact thawing. Two clues are giving us confidence that dry areas are frozen, as explained below : the continuous nature of the melting, but also the relatively steeper surface slope at the spots (previously discussed in §6.1.1, now in this section) :

"A continuous hydrological network upstream is only fed by local melting, so that, unless the network is disconnected, the thawed water cannot be driven out faster than the melt rate. As a consequence, some water would always remain at the base of the melting ice. Furthermore, water at the base enables basal sliding, and reduces the basal drag. For a given ice flux, the surface slope gives some indication on the relative importance of internal deformation and basal sliding in the ice motion. Local steeper surface may be associated with more basal drag and ice deformation, whereas local flatter surface may be associated with more sliding. Most of the spots where the model will be run are standing on slopes locally steeper than the regional slope"

I wonder if "emulator" is the right word. If you keep it, make sure it is really clear at the start of section where this is introduced what you are doing. Since you are trying to simply approximate the solution to physical equations maybe "approximation" or "parameterization" is a better word?

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We added the following sentence to define the word properly and say why we need it. We understand "emulator" as the process we use to empirically replace the whole heat model, to save computation time. We do not intend to parameterize "true" physical relations between input parameters, as we do not specifically account for diffusion or advection for example, but aggregate all the effects in one empirical expression.

L.347 : "As computing a given set of parameter takes several minutes, computing the basal melt rate for each point of the Dome region would be far too expensive. Here, the result of the whole forward model is mimicked by an emulator that depends on the input parameters H, Φ_g and p"

If you can find a location that has experienced no basal melting that is obviously ideal, but it seems like some melting at some time in the past million years may have occurred and what you really want to make sure is that very old ice is still at the bottom. If there was no melting how old could the deepest ice be? And, for continuity of the record is it critical that no melting has occurred? There needs to be more context on how your results inform the search for very-old ice and how good your results have to be before picking a site. Only at the end it is stated that this work may inform other modeling efforts - I might have missed it but this should be stated more clearly up front. It would really help to put this work in better context with the larger effort to find a drill site. Of course we won't know what is there until we drill, but I didn't know if your results have really helped to target a site or provided additional information that has to be weighed with all else.

Is the kriged product what would be used in follow-on 3-D modeling? Is this good enough? It seems like the validation you can do for point locations doesn't hold across this whole area in a kriged result, but maybe this interpolated map is the product you need to provide? That wasn't very clear.

If a bit of thawing occurred between 1.5M and 1M years ago, and no melting after, the basal ice age is clearly not "infinite", but 1.5M-year ice still exists somewhere, since it

existed 1M years ago. The age resolution must be affected but this kind of question goes beyond the scope of this paper, and mechanical modelling is needed.

We now better set the wider frame of this paper (observations - heat model - mechanical model) in the introduction (1.1 The oldest ice research). As geothermal flux is largely unkonwn, any new information on it is useful in the decision process for the drill site. The 3D-mechanical model we use now tries to constrain the ice rheology ; as it is an inverse process, the informations given on the basal melting (average + confidence intervals) reduce the parameter space to explore. Now, we use these kriged maps as prior values for 3D modelling. As we are focusing on candidate A, where we do have some contrains on the geotharmal flux, the kriged products are good enough there, and they even are probably more realistic than the existing products in the region (almost uniform value).

L. 45 : "In a context of locating the oldest-ice, our objective was to constrain the value of the geothermal flux and of the basal melt rate around Dome C. Later, this information will be used as a boundary condition for 3D mechanical simulations. As 3D models require many input parameters and are computationnaly expensive, any additional information about the thermal regime of the ice that reduces parameters value range is welcome, and will help select a suitable drill site."

If there was more radar data could you do this a lot better? Where is the community at with respect to drilling for oldest ice - will more data be available that you can use in a follow-on study? As a general approach it seems that you already have a lot more data then you might have compared to most places. But, is more needed to really pick the best drill site?

From this method point of view, a better description of the bed topography and bed reflectivity in the central, flatter, part of our study area could bring some interesting constrains. Elsewhere we do not think we could do much better. The present study gives us confidence in the absence of melting over the candidate A, but to constrain the

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effective age and age resolution we need additional radar information. Internal layers within the ice are now available over the candidate A (Young et al, TCD, in review and Parrenin et al, TCD, in review). By continuity from the EPICA core, these layers can be dated, so that the age-depth relation is described on the 3/4 of the ice column. This information is mandatory to have a constrain on datation models (1D or 3D), and to know the quality of the stratigraphy.

L577 : "More specifically, over the candidate A, a recent steady sate model assimilates radar isochrone layers to invert the value of Φ_g and p, and computes the basal age of the ice (Parrenin et al., 2017)."

Is there no chance for accretion? Or, have you already ruled out spots where that may have occurred?

Accretion is possible where the ice goes from wet to dry during interglacials. This could occurr on the flanks of the candidate A. At least on the candidate A, no accretion clue were visible in the last radar survey, so we did not investigate this particular point.

MINOR COMMENTS

Line 5: "climate forcing lagged by thick ice" - you mean lag in heat transfer through thick ice, right?

The sentence is completed as follows :

L. 5 : "But, as the basal conditions depend on heat tranfer forced by climate but lagged by the thick ice, the basal ice may be frozen today..."

"Temperate" isn't the wrong word, but maybe you want to keep the distinction between "frozen" and "melting"?

We agree with your words, that more focus on the melting process that detroy old-ice layers.

L.5 : "the basal ice may be frozen today whereas it was in average melting in the past."

Line 29: While the measurement at subglacial Lake Whillans is an important one, I think that it requires some assumptions about the thermal stratification of the lake in order to back out heat flux. I am not an expert in this area but more generally I would make sure that this section fits with your paper. How is this estimate from west Antarctica relevant to Dome C?

We just wanted to illustrate that measuring the heat flux is not an easy task in Antarctica, so that inverse methods and modelling are needed on the antarctic plateau. But as it was confusing, the text is now more straightforward on East Antarctica.

L. 52 : "The geothermal flux is usually derived from temperature gradients meassured in boreholes in the ground, but this cannot be done easily below ice sheets, because of the difficult access to the bedrock."

Line 37: Statement is about measurements but aren't the references to modeling efforts?

L37 : "Without any available temperature measurements, the value of the GHF has been estimated from geological considerations"

The sentence was confusing, now reworded as follows :

L.58 : "As deep boreholes are not numerous in East Antarctica, the value of the GF has first been estimated from geological considerations"

Line 49: should be "increase" instead of "increasing"

Correction made L. 72

I would suggest using "infer", instead of deduce (this occurred in multiple places)

Correction made

Lines 51-54: I didn't understand this sentence that talks about water routing models - not only was I unsure about how it related scientifically but I'm just not sure what point

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was being expressed. Perhaps a problem is also that it was a long sentence broken up by citations.

We added a link between the first and the second part of the paragraph, to make clear why considering basal hydrology is needed :

L.75 : "But the presence of water is not a sufficient clue to infer the GF, since water either comes from local basal melting or was routed from elsewhere. Using a collection of water routing models, Schroeder et al. (2014) inferred the value of the GF needed to explain the observed pattern of radar echoes..."

Line 54: Who is "They"? I don't know what study is referred to

L54 : "They derived an average value of 114 $\pm$ 10 mW m -2 for the Thwaites Glacier catchment."

L. 77 : "Schroeder et al. (2014) inferred the value of the GF needed to explain the observed pattern of radar echoes, and derived an average value of 114 $\pm$ 10 mW m -2 ..."

Line 57: "significative" should be "significant"

L. 57 : "The uncertainty on the GHF was estimated $\pm$12 mW m -2 , which is a significative improvement"

L. 82 : "The uncertainty on the GF estimation was $\pm$12 mW m -2 , which is a significant improvement"

Line 57: I am weary about making a point that uncertainties at a given level are a significant improvement - how uncertain are these uncertainty estimates

The current available (continental) estimations are affected by large uncertainties of +/- 20 mW/m2 (Fox Maule 2005 for example). From the "oldest-ice" point of view, these estimations cannot constrain efficiently the local basal melt rate. The key information for us would be a realistic upper boundary for the GF that would anyway allow the ice

to be frozen through time, and the only way is to find places where there is no melting. In that sense, any effort to reduce locally the uncertainties of continental estimations are welcome.

Line 58: I would restate to be something like ". . . but is still too large when trying to find a location that may preserve very old ice"

L. 82 : "The uncertainty on the GF estimation was $\pm$12 mW m -2 , which is a significant improvement compared to the previously available estimations, but is still too large when trying to find a location that may preserve very old ice."

Line 60-62: Doesn't add much to the reader, either be specific about how what you are doing is or is not similar to previous work or I suggest taking it out

L58 :"Moreover, their study area does not completely cover the main old-ice candidates (east and south-west of Dome C). Without using internal layers, which are not available everywhere, we will follow a similar approach to these two last studies, but adapted to the specific pattern of radar echoes under Dome C"

The sentence is reworded to say that we need a new local estimation.

L. 84 : "Their study area does not cover the main old-ice candidates located to the east and south-west of Dome C, so a new local estimation is needed. As dated layers are not available everywhere, we will follow a reflectivity-based approach like Schroeder et al. (2014), but adapted to the specific pattern of radar echoes under Dome C"

Line 67: Suggest finding another word than "triggered" I'm not sure it is clear here what you mean by "interesting" - state directly that the glaciology (not glaciologist) community is interested in sites where it has remained cold at the base without melting

This paragraph is taken out, but the main ideas are put in the first paragraph (§1.1 The oldest-ice research, L. 37).

Line 88: Again, I would state clearly what you are trying to do and somehow ". .

.this paper primarily aims to assess the risk of past temperate conditions at places known to be cold today" doesn't quite get it across. Perhaps something more like "This work constrains sites where basal melting is less likely to have occurred over the past 800ka, even if they are frozen today, so that very old ice has the best chance of being preserved at that site"

L 88: "this paper primarily aims to assess the risk of past temperate conditions at places known to be cold today"

We restated this sentence as follows :

L. 107 : "this work aims at constraining sites known to be frozen today and that are very likely to have been frozen in the last 800 ka as well, increasing the probability that very old ice has been preserved."

Line 90-94: A little bit of introduction isn't always helpful if the reader doesn't really have a good sense of what is coming. Consider expanding this to be more direct about what it means to do this in "forward mode" and "inverse model" It may be more clear to talk about this as "running a forward model" and "solving an inverse problem"? Be consistent with "heat model" or "thermal model" - used interchangeably in title, section headings, and text - but since they mean the same thing just pick one Changed for "solve an inverse problem" and "run the model forward"

Title changed to "heat modelling"

The beginnig of the paragraph is now:

L. 112 : "We present a 1D heat model forced by reconstructed climatic conditions, and run it in two ways. First, we solve an inverse problem with this model to infer the value of the GF in the Dome C region,using radar echoes as observational constraints. The pattern of wet and dry areas allows to estimate a critical ice thickness that corresponds to a threshold between frozen and thawing ice. For a given GF and vertical advection, this thickness is unique, so that we may in turn infer a GF distribution from the pattern

of basal echoes."

Line 96: I think you mean "relationships" instead of "relations"

Correction made, L.124

Line 104: "Characteristic" is more often used than "typical"

Correction made, L. 132

Line 117: "flown" should be "flowed", or better yet "been transported by ice flow" Correction made for "flowed", L. 251

Line 121: "permanently" seems too strong

As "continuous" is characterizing the process, we took out "permanently".

L. 256 : "A continuous hydrological network upstream is only fed by local melting,..."

Section 2.1.3 doesn't add much - why is this a section and why does this get mentioned in this order? It seems like you want to introduce the model equations first, then talk about caveats!

The new version of the "Model" section is reordered in this way :

2.1 Geometry and coordinate system 2.2 Heat equation 2.2.1 Ice thermal properties 2.2.2 Basal boundary conditions 2.2.3 Boundary condition at the surface 2.3 Velocity model 2.4 Proceeding assumptions 2.4.1 Correspondance between wet and thawing areas 2.4.2 GF spatial variability

The last section 2.4.2 is needed as it is one simple but important assumption of the model.

Line 138: typo should be "which" Again, "emulates" seems like the wrong word. Here, maybe "simulates" or "approxi- mates"?

Correction made L. 141. As we answered in the "main concerns", emulates seems to

be the right word.

Line 145: Is D dimensionless? Is K a function of reduced depth? (don't think so)

"Dimensionless" added, L. 154. New ordering of the paper provides information on K just after this section, L. 160. K depends on T, so indirectly on reduced depth.

Equation 2: These might be equivalent or just defined differently, but this equation is not the same as in Parrenin et al. (2007; equation 3) - check to make sure no typos between ζ and (1-ζ)

No typo here, Parrenin et al 2007 is using reduced height, and we use reduced depths

Section 2.6 header - suggest stating as "Basal boundary conditions"

Correction made L. 165 §2.2.2

Line 187: Why is this an unusual choice?

L187 :"This is an unusual choice for such an important parameter, but we argue that Eq.(9) is consistent with the temperature profile of the EPICA Dome C ice core"

This formulation of the melting point temperature has not been published in a reviewed article yet, only in Ritz's PhD manuscript. It is needed since it seems to better match the measurements at Dome C. However, this choice is discussed later and has no crucial impact on the results (L.396.), now we just say :

L. 171 : "This expression is compatible with the temperature profile at the EDC bore-hole"

Line 202: paleotemperatures aren't "known", they are still estimates

Changed for "estimated" L. 186

Line 209: Why is this represented as 1/6.04K - if report this way make sure to have typed as actual fraction since as it is now with inline slash it could be confusing

We keep 1/6.04 for consistency with literature (Lorius and Merlivat, 1975)

L193 : "In addition to the nominal value 1/6.04 K, we will perform sensitivity studies..."

Section 2.8 - Do you concentrate grid cells near the base, and is vertical spacing good enough? Also, are 1000 year timesteps good enough? Can you say anything about uncertainties related to your solution grid? The current statement mentions there was a tradeoff between accuracy and speed, how much was compromised?

The mesh is regular. The dependence of the result (basal temperature) is of the order of 0.1 K when changing the vertical spacing, and of the order of 0.01 K when changing the timestep, whereas the computation time linearly increases with the number of elements in the mesh.

L. 276 : "The dependence of the basal temperature on discretization is limited to a few tenths of K."

Everywhere you use "till" should be changed to "until" Correction made

Section 3 heading is a bit confusing since it isn't yet clear what is being measured, and it is definitely not heat flux! I would change that to something more clearly related to your data analysis. In general, I suggest coming up with another term for "measurement spots".

L 235 : "3) Measurement spots"

We changed for a longer but more explicit title L285 : "3) Spots where GF will be inferred"

Suggest in the text to remind readers what p is. Same for any variable that was introduced awhile back in the text

"Shape parameter" added on L. 287

Line 269: Is there a short justification for the heat flux range of 40-70 mW/mËĘ2?

Empirically, the GF values that were found in this range (in fact 45-65 + safety margin).
L. 319: "All the GF values were in the range 40-70 mW m -2"

Line 272: Suggest that "inferred" is better term than "derived" Correction made

Line 286: "do not mismatch" - do not match? Or, mismatch?

We confirm that "do not mismatch" is what we intended to say. Since the confidence we have in these points is low, we could expect a mismatch between the surrounding GF field and the value at these points, but it is not the case. We reworded as folllows :

L335 : "The inferred values of the GF nevertheless matched the N-S gradient"

Line 299: I would put your estimated relationship between heat flux and ice thickness in words and remind the reader that this is for the explored range of heat flux values, or does it hold for any heat flux (any melt rate)? Then, the relationship between heat flux and melt rate must correspond to a specific ice thickness, right? I lost the details around equations 15-16 and suggest adding more text to explain what this means and how it was derived.

First we explain the goal of the emulator we need to build :

L. 347 : "As the computation for a given set of parameter lasts several minutes, computing the basal melt rate for each point of our domain would be far too expensive. Here, the result of the whole forward model is mimicked by an emulator that depends on the input parameters H, Φ_g g and p."

L. 355 : "Over the positive-melt-value domain, we used a least-square minimization method to compute the following relation:"

-Eq. 15-16 : This paragraph is taken off, since it was unnecessarily complicated with respect to what we want to show. Now we simply present the past basal melt rate calculated with the central values of GF and p, which is the main message of the paper.

Line 337: what do you mean by "at the opposite" here?

L 337 : "However, the average basal melt rate is changed at the opposite by 0.1 mm a -1"

When the GF is affected positively by a parameter change through the inverse problem, the melt rate is affected negatively during the forward problem, and vice-versa.

L380 : "However, the average basal melt rate is changed by 0.1 mm a -1 in the reverse direction"

Line 351: Again, not sure how to take this point about an "unusual choice" and why it was chosen that way to begin with

L351 : "Given that the expression of the pressure melting point is an unusual choice in glaciology (Eq. 9), the inverse method was reiterated with a more common value (B = 0.098 K.Pa -1) as a test"

As explained in section 2, we choose this dependence of temperature on pressure because it better matches with observation, instead of a widespread expression (for example in Cuffey and Paterson 2010).

L. 399 : "Given that the expression of the pressure melting point is an unusual choice in glaciology (Eq. 9), the inverse method was reiterated with a more common value corresponding to saturated air in the ice (T m = 273.16-0.098 P , Cuffey and Paterson, 2010)"

Line 354: Why is "order of magnitude" good enough?

L. 354 : "The order of magnitude of the results remains the same whatever the expression for the pressure melting point"

"Order of magnitude" was awkward, the sentence is reworded : L. 401 : "The results in terms of basal melting are not significantly affected by the expression of the pressure melting point"

Line 357: The leading sentence doesn't seem to relate to the second sentence and I had to read back to see if I understood what was coming and has already been done. Suggest better lead-in to this section.

A new introduction to this section is proposed :

L.405 :"One way to assess the performance of our model is to compare observed basal state (wet or dry, Zirizzotti et al. (2012)) with the model simulation for present time. For p = 2, we compute the (Φ_g , H c) relation, corresponding to the present basal state, by sampling H c between 2 700 m and 3 300 m. The two parameters are linked by the following empirical relation:"

Line 365: "build" should be "built", and really do you mean "calculated"? Correction made L. 414

Line 368: What small-scale structures are these E, G, H, I, L locations referring to?

A sentence now introduces specifically the coordinates we refer to (introduction section) :

L. 121 : "For the sake of convenience, the domain is referenced using pairs of letter and numeral, corresponding to the grid of the Italian survey (Fig. 1). In particular, two promising old-ice candidates are located at C6 and H1."

Line 369: Should be "non-melting"

L 369 : "no-melting areas"

L423 : "non-melting areas"

Line 371: Need to rephrase part about ". . . is however often respected . . ." "undoubtly" should be "undoubtedly"

Line 373: What do you mean by "local gap"?

This paragraph is now mainly restated :

L. 415 : "Superimposed with the observation data, the model output shows that large-scale patterns of wet-dry areas are respected, especially on steep bed slopes (candidate B, C, D, and to a lesser extent candidate A). On these bed reliefs, certain points however show a discrepancy between model and observation, but the gap to the critical thickness is often close to 0 m (D3, D5, D8, M3), meaning thata small change in GF forcing, or a better description of the ice thickness, would better assign theseparticular points. The 1 km-resolution of the Bedmap 2 bedrock dataset (Fretwell and coauthors, 2013) smoothed along-track subkilometric features detected by our RES survey.

The steeper the bed, the sharper the limit between melting and non-melting areas. In the central, flatter, part of the domain, the position of this limit is more blurred. As we could not assess the GF with our method there, it was interpolated. Despite this lack of constraints, several small-scale features are well mimicked (dry areas at I9, G-H8, wet areas at G9 and L7). Other regions are not assigned in compliance with observations (G6-7-8, H-I8), meaning that the GF is overestimated, probably up to 3mW m -2 , which is consistent with the uncertainties given by our method (inversion and interpolation)."

Line 375: Should be "dependent" Correction made L. 432

Section 6.1, maybe "Method validation"? There is no lead in, which may be fine but as is now it isn't clear how come you need to have overarching section 6.1

The structure of the discussion section has now changed, and we changed for "Model assessment"

6.1 Consistency with published data and measurements 6.2 Model assessment 6.2.1 Method validity 6.2.2 Sensitivity to parameters 6.2.3 Spatial variation of the GF field 6.3 Lessons drawn for the oldest-ice research 6.3.1 Interpretation of the wet/dry pattern at the ice base 6.3.2 Old-ice targets

Line 399: What do you mean "the surface slope is the source of motion"? Talking about clues isn't very precise - is there a better word?

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At first order there is motion because of the pressure gradients within the ice, and that depends on surface slope. These explanations are now put in section 2.4.1.

L. 260 : "For a given ice flux, the surface slope gives some indication on the relative importance of internal deformation and basal sliding in the ice motion. Local steeper surface may be associated with more basal drag and ice deformation, whereas local flatter surface may be associated with more sliding."

Paragraph around lines 415-420: Not sure I understood the point of this paragraph, if this is just too hard to constrain state that directly. If it needs to be considered somehow and will significantly affect uncertainty estimate state that directly too. I can't tell if this is something that really could matters.

This paragraph (now L486) correspond to additional tests we made. We wanted to make sure of the influence of accumulation, since there is a NS gradient of both accumulation and GF. As we made the sensitivity test for a, it is worth saying a word about it, even if the sensitivity is low.

Line 422: Not sure what you mean with "litmus"

"Litmus test" means a "test of truth" (changed for "reliable comparison" L. 443).

Section 6.1.4 - by "structure" do you mean "spatial variation"

Correction made for "6.2.3 Spatial variations of the GF field" L. 492

FIGURES AND TABLES Would be worth defining input parameters in Table 2. I don't quite understand what "total on m" represents.

The new caption is the following :

"Sensitivity of the GF (assessed from inverse runs, [mW m -2]) and basal melt rate (calculated with forward runs, [mm a -1]) to input parameters α, β and a. As the final value of m depends on both the inverse run to determine Φ_g, and the forward run to compute the melt rate, the last column accounts for the sensitivity on the whole

procedure (inverse+forward)."

As the final value of m depends on both the inverse run to determine the GF, and the forward run to compute the melt rate, the last column accounts for the sensitivity with the whole method (inverse+forward), which is less than the sensitivity with the forward mode only.

Figure 2: I might have missed it but check that critical ice thickness is defined in the text to this point, or add to caption

Caption completed :

"We define the critical thickness as the minimum thickness that allows basal melting at present."

Figure 4: Not sure it was discussed in the text how the 10 spots were chosen?

The spots are chosen where a correlation could be found between reflectivity and ice thickness. Sentence modified in section 3 :

L. 293 : "Ten corresponding spots are selected (black rectangles in Fig. 1), where reflectivity and ice thickness are somehow correlated, and that are hereafter denoted by the indexes of their central point (Fig. 4)."

Figure 6: A lot of overlapping lines. How many discrete values of m are represented?

Now only 2 series of m-contours are represented, for p=1 and p=10.

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