List of relevant changes

- English wording was carefully checked by the native co-author, and the abstract was reshaped.

- Following Referee #1 comments, we changed the title for : Candidate sites of 1.5-Ma ice 37 km southwest of the Dome C summit, East Antarctica

- The readibility of the figures was improved (contrasts, size, labels)

- A paragraph was added at the end of the introduction to better set the context of this modelling work, and what should be done with modelling results.

- The work of Van Liefferinge et al (TCD, 2018) is now presented in the introduction part, so that the reader already knows what is going on when comparing both results in §3.5.

Answer to referee#1

The authors present a new modeling study that further characterizes the ice-flow and subglacial conditions that could combine to best preserve ice up to 1.5 million years old near Dome C. The study builds on previous work by applying a 3-D model and by using the latest available bed topography. The authors identify candidate drill sites that attempt to balance needs of the oldest ice possible with the age-resolution desired for ice-core analyses. This was a nice piece of work that advances understanding of the Dome C environment; modeling like this is key to picking a drill site for the "Oldest Ice" ice-coring effort.

I have more substantive comments on the style of the manuscript, and relatively minorcomments on the modeling. Both are given below, and by line number as appropriate. I suggest that the authors work together to improve the writing style (including title, abstract, main text, and summary statement). The manuscript can be understood but the language used is often non-descript, and I think detracts from the impact of the work. Sometimes it is a subtle use of an inappropriate word, and sometimes (at least to me) it gives a context to the sentence that may or may not be intended. I appreciate that this may be a simple translation situation – and in my attempts to learn French I am very sympathetic to how difficult this could be – but, again, I think it is worth rewriting this carefully using different expressions so that the work is clear and has the most impact for all readers. Hopefully all of the authors can work together to achieve this.

We would like to thank Michelle Koutnik for her fruitfull comments, that helped improve this manuscript. English wording was carefully corrected in this new version.

Line 2: "prevent basal melting" – suggest "limit"
l4 : Changed for « limit »

Line 3: "ensure" is strong, really you are making the best estimate
l5 : Changed for the following sentence : « A 3D ice flow simulation is used to calculate five selection criteria, which spatial variability is used to locate areas that have better glaciological properties than elsewhere. »

Line 4: "ice archive is sufficient" – sufficient for what? It hasn't been made clear what is needed, there is a disconnect between first sentence and following sentences.
« Sufficient » is deleted in the new formulation (see previous answer).

Line 5, title, and throughout: I am not against the use of the term "patches", but the term doesn't tell the reader much. Since this is in support of ice coring, one impression is that a patch could be a few meters, or since the study area around Dome C is tens of km+ maybe this is the scale? I would clarify what area a patch covers up front, and I also suggest making a somewhat more general title (why is 37 km so important?). I think "area" is a better term than "patches", but the authors can justify what is best based on use in the community.
As we finally suggest precise drill points within kilometer « patches », we will change the word for « sites ». We think giving the distance from Dome C is important to inform the community that the research of a drill site is now focusing on a kilometer-scale region, and gives the location in a region that lacks place names.

Title: "diagnosed" is not incorrect, but isn't the way I would expect it to be used. None of these have to be used, but some title suggestions could be: "Candidate areas of 1.5-Ma ice southwest of Dome C, East Antarctica", "Flow-model constraints on locations where 1.5-Ma ice exists southwest of Dome C, East Antarctica", . . . something more general, and yet specific to the needs of "oldest ice" seems better. The stated challenge with this is that the model is only as good as the boundary

conditions that may vary in space and/or time. I understand the need to say something strong about the presence of 1.5-Ma ice so that the next steps toward a drilling program can proceed. I think the authors acknowledge this but I'm left with the tension on whether it is better to state the results more confidently, or less. Perhaps this is where language comes in again, for example:
We accept your suggestion, and changed the title for « Candidate sites of 1.5-Ma ice 37 km southwest of the Dome C summit, East Antarctica ». We removed the word « diagnosed » that complexified the title.

Lines 9-10: "Several precise locations of potential 1.5-Ma-old ice are proposed, to nourish the collective thinking on the precise location of a future drill site." I suggest that this sentence is revised to state directly what the community should do with these results – or, is already doing with them! If the results are really just something to think about, I guess that is it. But, if they are the state-of-the-art in modeling and what will in fact be used going forward as a community, say that. (And, "nourish" isn't the right usage here and gives too loose a sense of the value of this work.) Also, from the conclusions it sounds like this modeling has informed where to collect new radar data, right?
L10 : We changed the end of the sentence to be more specific and to link our results to the ongoing field work : « These sites will help to choose where new dense ground radar surveys should be conducted in upcoming field seasons. ».

Also, why is this a "Brief communication"? It seems awkward for a modeling paper that should contain enough detail to evaluate the merits of putting results to use in planning would be published as a "brief". Again, this was hard to evaluate because it wasn't entirely clear how the results from this work should be used. Are they really just something to think about as the other work on this moves forward? Why is this a valuable step? I think that the results will in fact be used more directly hand-in-hand with new data collection, rapid-access drilling, and eventual deep drilling – again, I suggest the authors frame their results more directly in context with the community effort.
We choose to publish our results as a brief communication for two reasons. First, we wanted to shed light on the practical side of our results, that may interest many different persons, mainly researchers that are not ice-flow modellers. A brief communication format is more accessible in this perspective. Second, our modelling work has several limitations, the main one being that we did not made any sensitivity study on the input parameters (lack of time and resources), that would have probably been asked for within a longer article. So we choose to present our results as they are now, and as they are actually used. Finally, the editor agreed that this format fitted our message.

Line 17: Might want to elaborate on whether processes under consideration are external, internal, or both. And, seems that there would be more references other than Clark et al. (2006), so give as "e.g., " or list a few more that are relevant – possibly splitting out after points in this sentence where the references apply.
As we are limited in space and number of reference, we specify to refer to Jouzel and Masson-Delmotte for a more complete overview of the problem.

Line 25: What is "IGE"?
L28 : The acronym is developped : Institut des Géosciences de l'Environnement (Grenoble).

Line 35: "inverts" is not used correctly here, and I would state more directly that this approach solves an inverse problem, making clear the model parameters that are inferred
L39 : The new sentence is now : « The distance between the dated isochrones and the modelled ones was minimized to infer a thinning parameter that characterizes the vertical deformation through the ice column ».

Line 37: “definitely not vertical” – suggest as “. . . are not only vertical”
L43 :Changed for : « the trajectories of the ice particles are not only vertical. »

Line 53: “security margin” – suggest other phrasing, and while I understand there is no better estimate, is that really true? What about ice-flow conditions between candidate sites in this work and Dome C drill site may inform if 60 meters is an under- or overestimate? Did you try other values if there is a chance this is an underestimate (as stated)?
L59 : We changed for « safety distance ». The origin of the ice disturbance in the last 60 m at Dome C is not clear, and the ice layering of very deep ice (bed+200 m more or less) cannot be unambiguously interpreted from the radargramms. So it is difficult to constrain the spatial evolution of this disturbed layer. However, we show later in the paper that 1,5 Ma should stand higher than 60 m above the bedrock, so that this threshold does not prevent us from selecting the best sites.

Line 56: “defavourable” is not a word, here it would be “unfavourable”
L63 : Changed for « unfavourable »

Line 58: missing “the” between “Finally, the location. . .”
L64 : Missing word added

Line 59: Define the “water limit”, I think I understand but since it is used often need to be clear what this is and how it is estimated
L66 : We added the following sentence : « We will call this threshold "water limit", above which there is no evidence of the presence of water in the radargramms »

Figure 1: A scale bar would be helpful
A scale bar is not compatible with this oblique projection. So we added information in the caption : « Mesh, bedrock dataset and basal melt rate used for the simulation on a 83 x 114 km domain. »

Figure 1 caption: I would refer to this a “context map” instead of a “situation map”.
Language of “the hold of the domain” sounds off, and I suggest “. . . shows the domain used for the calculation, and the blue rectangle at the top of the image is the location of Figure 2”
The caption is modified as follows : « The red patch on the context map (bottom left) shows the domain used for the calculation, and the blue rectangle at the top of the image is the location of Fig 2. »

Line 61: Refer to Figure 1?
L86 :Reference to Fig. 1 added.

Line 66: Is the firn really “accounted” for? I would say that your model is in ice equivalent and you adjust the surface height using an assumed density profile to convert the firn layer to an ice layer. It should be clear that you don’t include a process model of firn. Where did your density profile come from? (Assume Dome C, but did you apply that everywhere?)
L88 : Right, we have no firn model, only an ice-equivalent layer. The sentence is restated :
« The model works in ice-equivalent, and we adjust the surface height by assuming that the density profile of the firn is the one of Dome C on the whole domain. »

Does the model resolution as a function of depth vary linearly, exponentially, ?
L91 : It evolves linearly, we mentionned it as follows :

« The resolution of the 20 vertical elements of the mesh evolves linearly, so that the deepest one being 25 times finer than the upper one »

Line 77: “were” should be “where” I suggest using “not present” instead of “null”
L103 :The new sentence is now : « We here focus on a region where basal melting is probably not present or limited, and horizontal velocities are very small, so that, for the sake of simplicity, a no-sliding condition is imposed at the bottom of the ice column. »

Line 84: Instead of “heavy”, suggest “excessive” – and is that really true for the limited domain of your model given that it is steady state? Is the issue that you can’t solve the time-dependent problem and therefore a coupled thermomechanical model doesn’t add much in steady state?
Also, it could be worth noting that it is non-trivial to extend a multi-dimensional limited-domain model from a steady-state calculation to a transient one. Without modeling the full continent you need to impart information to this regional model about how ice-thickness changes and ice-flow changes inside and outside of this domain correspond to changes in the rest of the ice sheet in which it is embedded. So, for the goal of regional modeling you are minimizing even more assumptions (and challenges in setup and computation) by starting in steady state. I looked at this for 2.5-D (flowband) models: Koutnik and Waddington (2012), Well-posed boundary conditions for limited-domain models of transient ice flow near an ice divide, Journal of Glaciology 58, 1008-1020.

Yes, in fact we are meeting 2 different problems that are linked :
- Solving the thermomechanical problem needs a lot of time (even on a restricted area) so that the thermal state first reaches a certain stationnary state that can be used as a starting point.
- Then, solving the time-dependant problem on a domain limited by virtual boundaries is very tricky as it needs to permanently conserving the mass out of the domain, while maintaining the global shape of the dome. To do so, the parameterization of the boundary conditions is very sensitive and would need specific developement.

That’s why we decided to make things simpler, and to separate the problems : 1 estimating as best as possible the melt rate (which is why the ice thermal state really needs to be described), and this was done in a previous paper (Passalacqua et al 2017) and 2- considering these ice temperature and melt rate as true, and dealing with ice mechanics, to see the influence of the bedrock description (this paper).

L113 : We completed the end of the paragraph :
« Solving the coupled thermo-mechanical equations would require excessive computing resources, without radically changing the ice fluidity – which is mainly controlled by temperature. Similarly, we do not account here for long-term evolutions of the ice sheet surface, but are aware that this assumption strongly affects the trajectories of the ice particles. »

Line 94: Instead of “more influent”, suggest “has more influence”
L122 : Changed for « where shearing has more influence »

Line 110: State as 1/lambda?
L137 :Changed for « The age resolution is stated as 1/lambda »

Line 113-114: This sentence wasn’t clear to me starting with “The way ice strains. . .”
L139 : The couple of sentence was modified, to say that a given ice flow depends on the bedrock underneath, but a given bedrock can lead to different ice flows : « The way ice strains by flowing over a rough bed differs depending on the shape of the bedrock underneath. Similarly, a given bedrock shape can be a convergence or a divergence area, depending on the orientation of horizontal ice flow. »

Line 143: What do you mean by “a logic combination”?
L168 : Changed for « boolean combination ».

Figure 2: x-axis and y-axis numeric labels are way too small in the top panels, and probably also too small in the bottom panel. Axis labels are missing. I am not really sure where to read the numbers from each panel and without those they don’t say much – the caption needs to be improved to make sense of these panels, or maybe the top four are not shown? It looks like there would be more overlapping areas, or is it just that age resolution is limited?
Make sure the caption clearly guides the reader through this and that all box colors are identifiable. For example, it took me awhile to see the blue box showing location of Van Liefferinge results.
Seems like colored crosses should receive more discussion in the text. Pros / cons of each choice could be takeaway points. Again, how should the community use these results?

Numeric labels on top panels were simply deleted, as they are identical to the ones of the bottom panel, and axis label was added. Indeed, age resolution is the most restrictive parameter of the 5 criteria. The selected areas are now colored in yellow, which is more visible. We added discussion on the sites in the text :
« Considering that, only a few set of favourable drill sites remain in boxes A, B and C (blue, orange, red and yellow points in Fig2 2. Red and blue points have less risks of basal melting, while yellow and orange have less risks of stratigraphic disturbances. The best choice between these sites should be now guided by local radar surveys characterizing the internal layering of the ice, and the vertical strain rate profile (Nicholls et al, 2015). »

Putting these results in better context with the Van Liefferinge et al. (2018) results seems necessary. Especially that this work is still in The Cryopshere Discussion, the reader is not necessarily sure what to make of these parallel efforts. I might have missed it, but I think the first citation of this work is in the caption of Figure 2. Section 3.5 goes into more depth between the approaches but could be worthwhile to put this context up front, and as part of the framing of why your results matter to the community.

The presentation of VL et al (2018)’s work is shifted to the end of the introduction, and is followed by explanations on what should be done with these data :
L77 : « The decision-making process of a drill site needs both field survey and modelling, the former feeding the latter with geophysical constraints, and the latter reducing the areas of interest for new field surveys, focusing more and more on promising sites. The information brought by the present study should be sufficient for a last dense radar survey to be led on promising sites during the next field season, at a scale of a few hundred of meters. Then the community should be able to take a decision for a drill site in the Dome C region. »

Is there really no way and/or no effort underway to combine these two approaches? Is the limitation only computational? At what point might this be possible? (Or, what are the next steps that can be taken by the modeling community in this effort to find the best drill site?)
Of course we could go further in ice modelling, for example by using VL (2018) probabilities that ice reached the melting point in the last 1,5 Ma as a boundary condition in the 3D model. But we are not sure it would necessary be a good idea :
- Having several points of view give a supplementary information on the strength and weaknesses of each approach.
- At the scale of a few hundreds of meters and less, our choices should be guided by the careful analysis of the future local observation data, not new modelling.

Line 182: I suggest rephrasing “oldest-ice challenge” – even “the challenge of finding the oldest ice” sounds better, somehow the other sounds too loose, and therefore does not have as much impact.
L206 :We changed for a simple formulation « This zone of higher shear should be discarded for a future drill site ».

Line 187: “appropriate ice”, suggest rephrasing what you mean by “appropriate” or using the phrase “candidate drill sites”
L210 : « The best combination of age, age resolution, folding, convergence and melting criteria is shown in Fig. 2 (bottom), revealing several spots of ice reaching the 5 criteria. »

Line 190: suggest using another word than “risks” (there are other usages that I’d also change”; maybe the word is “chance”
L225 : « The ice flow converge, increasing the possibility of insufficient age and positive basal melt »

Line 190: By shorter trajectories you really just want to be closest to the dome / ridge-
L214 : Indeed, we now specify this point : « Locations within boxes C and A are closer to the ice ridge and should be considered first for a future Oldest Ice drilling because of shorter trajectories. »

Figure 3: Why not give the x-axis in km? All labels are very small Is the bed topography the most important boundary condition in your modeling? What resolution would be ideal to be confident from modeling on where to pick a drill location?
The x-axis is now given in km, and labels are bigger. As discuted in the text, the boundary condition is crucial for the basal age and age resolution, and the shear history of the ice. That is why this figure shows the amplitude of the bedrock spatial undulations. Given the underlying assumptions of this kind of work, improving the spatial resolution from 1 km to 500 m or 100 m would not weaken these assumptions. A better bedrock description at the scale of a few hundred of meters is required locally, but, at this scale, the 3D ice flow modelling will not necessary be a better tool than a fine interpretation of the radargramms and their internal layers.

Line 210: How can these results really be benchmarked against Van Liefferinge et al. (2018) given that they use a very different approach? All you have done is qualitative comparison, right?
Maybe the word « benchmarked » is less appropriate than the word « compared » in this case. This comparison is made by overlapping the results of the two approaches. Any model is as good as the assumptions on which it is based, and comparing the two approaches is a way to have two different points of view on the same object (the future drill site, maybe where the model agree), but also to have information on the strengths and weaknesses of each model (where they disagree).

Line 214: should be “constraint”
L73 :Changed for « constraint »

Answer to referee #2

This manuscript uses a three dimensional ice sheet modelling approach to explore the basal age and resolution of the ice sheet at Dome C, in a region where bedrock relief is likely to be conducive to preservation of very old ice: up to 1.5 Ma or more. The study appears well-posed and the work is a nice summation of what this approach can tell us regarding basal age and resolution. It is an important advance that is required for targeting future drilling locations and should be valuable in guiding additional exploratory studies. I have only minor comment concerning the modelling itself. The paper does suffer in places from somewhat non-standard English usage, some of which intrudes a little on readability. While fully appreciative of the authors' first language I respectfully suggest these items be edited for clarity – noting a native English speaking author is on the list. I see the other referee has commented in similar vein – I will not generally specify the linguistic items for correction below.

We would like to thank Tas Van Ommen for his fruitfull comments, that helped improve this manuscript. English wording was carefully corrected in this new version.

Detailed comments: Title: In general I disfavour the usage of Dome C as a "point location" synonymous with Concordia Station, as the entire region is really Dome C. Suggested use would be to have the title read "Oldest Ice" patches diagnosed at Dome C, 37 km southwest of Concordia Station. I however leave it to the authors to consider, as it is not a substantive concern.

Now we changed « Dome C » for « the Dome C summit », when we refer to the Dome C upper point.

Line 25: Define IGE on first use
L28 : The acronym is now developped « IGE (Institut des géosciences de l'environnement, Grenoble) »

Line 37: " and [provide] sufficient resolution"
L42 : « The observed isochrones are compatible with high basal age and provide sufficient resolution »

Line 48: Fischer et al. actually stipulate no more than 20 ka m-1 although this may now be thought too coarse. I have heard targets of 14 ka m-1 used. In any case, the 10 ka m-1 is not consistent with the reference.
This reference was indeed an error. This threshold value was given by J. Chappellaz, following the different Beyond EPICA-Oldest Ice meetings and workshops.

Figure 1 caption: "show the hold [sic] of the domain"
« The red patch on the context map shows the domain used for the calculation »

Line 74: relaxed for 50 years . . .. How is the reader assured that this is adequate? Naively it seems very short. Maybe just reword to say that this period proves sufficient to propagate away initial discontinuities or similar.
Ideally, we should have reached a steady state. But reaching a true steady state for this 3D model with virtual boundary conditions is a very challenging task (ensure mass conservation, limit BC influence on the surface shape, control of the shape of the ice surface). So this relaxation time is a trade off between a fixed geometry simulation (which can lead to misinterpret the results if the surface shape is not consistent with the flow law), and complete free-surface simulation, which would require much more developments. Since the goal of the study is a comparison in-between of different sites, this question of transient vs steady simulations is less crucial than it could be.

We added the following sentence :

L99 : « The ice surface is very flat, and 50 years is enough to accomodate the surface altitude to the ice rheology up to ∼ 1 m, without radically changing the orientation of the ice ridge, on which we have little control. »

Line 77: “focus on a region where basal melting is probably null” - this may be true for the high points, but the domain most certainly includes areas of basal melt, so how is it that a no sliding condition is OK?
A no sliding condition is valid because the horizontal velocities are very small, so that even if there is sliding, the sliding component of the horizontal velocity would be even smaller. We completed as follows :
L103 : « We here focus on a region where basal melting is probably not present or limited, and horizontal velocities are very small, so that, for the sake of simplicity, a no-sliding condition is imposed at the bottom of the ice column. »

Line 95-100: It is not clear from the description why the use of stress exponent n=3 is valid. Indeed there is some varied opinion in the literature over the best value to use in various situations particularly ice divides (see e.g. Martin et al., JGR, 2009; Martin and Gudmundsson,TC, 2012; Petit et al., JGlac, 2007). While not wishing to create imbalance in the treatment in this paper by opening an extensive discussion, some context to the literature would be useful. More importantly for understanding the results of this modelling, could the authors arrive at a statement as to whether an exponent n=3 is likely to under- or over-estimate age and resolution? That is, is it conservative to the aim of finding old ice?
The question of the value of n is a specific scientific question, and its complete discussion largely exceeds the goal of this paper. Even if we know that its value is a matter of debate, we first made sure that the value of n was compatible with observed velocities, which should be sufficient for our purpose, even if it also influences deep layers. Our goal is not to make the best evaluation of the absolute age (which is much more influenced by a bit of basal melting than by a change in Glen exponent), but to compare some locations to others. For this purpose, changing the value of n would not radically change our results.

Lines 128-130: Maybe an example of the language clarity issue, but it is hard to see what is meant by “the outputs still keep their relevance when analysed relatively to themselves”
We changed for the following sentence : « If the absolute value of the age, age resolution, or strain rates can be discussed regarding the choices of the model parameters, we reckon their spatial variabilities are robust since they mainly depend on the shape of the bedrock and of the ice surface »

Line 138: the “water limit” at 480m needs a little explanation, where does it come from and what is the reference height (I assume it means 480 m.a.s.l.).
L156 : We add a sentence : « We will call this threshold "water limit", above which there is no evidence of the presence of water in the radargramms »

Line 171: An example where the “Dome C” not equal to “Concordia” nomenclature issue comes up. I’d favour “Concordia”.
Changed for « the Dome C summit »

Figures 2 and 3: Axis labels in particular are too small. Figure 3 would benefit from all text being larger.

Axis label have been enlarged, or deleted on the upper panels as they are identical as the ones of the bottom panel. Text is larger in Figure 3.

Line 216: “our biggest central patch” isn’t so easy to follow as using the labels provided: I assume it is “Patch A”.
L235 : Changed for « box A »

Answer to referee #3

The manuscript presents a potentially valuable 3D modelling-based study designed to isolate, through a series of spatial masks, candidate locations for “oldest ice” (1.5 Ma) near Dome C, East Antarctica. The analysis is interesting and, I believe, robust (subject to some reservations, below) and I would support publication. I believe the manuscript structure and approach are valid, but I do see the manuscript’s current findings as somewhat undermined by the handling of temperature – and particularly basal temperature and sliding - in the analysis. I would encourage a revised manuscript to consider this in more detail, at least placing some first order approximations of error based on possible temperature scenarios. I accept that this may not be the forum for a full thermo-mechanical analysis but, for the analysis is fit for purpose, it needs to report some approximation of the age errors that might derive from the assumptions made. The writing is occasionally ambiguous and includes grammatical and typographical errors.

We would like to thank Bryn Hubbard for his fruitfull comments on this manuscript. We understand your concerns, as this drill site location needs many glaciological aspects to be discussed. However,we think that handling the problem in a comprehensive way (ideally: 3D transient simulations assimilating radar isochrone layers, basal reflectivites and surface velocities, and uncertainties discussion) is far too ambitious. That is why we handled the problem differently. The age estimation was first done by Parrenin et al (2017), using dated internal layers. Similarly, the influence of the geothemral flux is crucial, and its local value and influence on long term is discussed in Passalacqua et al (2017). Given these two previous studies, we only discuss here one single simulation, which is enough to compare the local glaciological properties given a certain bed shape. The estimation of uncertainties would require sensitivity tests on several parameters (fluidity, Glen exponent, temperature profil/basal melt rate), which is very heavy to do in 3D. In fact, the goal of this paper is not to assess the best estimation of the age and its uncertainties. To make this more clear, we added a sentence to precise this point for the reader:
L51 : « Note that the goal of this study is not to assess the best age estimation, but to evaluate which sites have better glaciological properties than others. »

Some more specific comments follow:

Some of the wording could be improved here between lines 3 and 7.
The abstract was deeply reshaped :
L5 : « A 3D ice flow simulation is used to calculate five selection criteria, which spatial variability is used to locate areas that have better glaciological properties than elsewhere. These selected areas (a few km 2) lie on the flanks of the Dome C bedrock high, where a balance is found between risks of basal melting and sufficient age resolution. »

Page 2 There are at least two typographical errors on this page (“serie” and “defavourable”). The manuscript needs checking to remove other occurrences. I identify a few more below.

We changed for « set » and « unfavourable »

30-31 Is it not possible that changes in ice thickness have an influence here at long timescales? Can this influence and outcome be approximated?
Indeed, and this previous results (Passalacqua et al 2017) already accounted for the ice thickness changes. We here use a melt rate averaged over the last 400 000 yrs.

30-42 Can the simulations and models of others publishing on this topic be summarised briefly?
We restated or added precision on these two models.

L31 : « First, a 1D heat model was run over the last 0.8 Ma to determine the present state (temperate or cold) of basal ice, which was compared to the reflectivity map in the region of Dome C. We could infer the value of the local geothermal flux, and explain} the origin of the local spatial distribution of subglacial water at the ice-bedrock interface. (…) Second, a kinematic 1D ice flow model was used to evaluate the age and age resolution of the deepest portion of the ice sheet. The distance between the dated isochrones and the modelled ones was minimized to infer a thinning parameter that characterizes the vertical deformation through the ice column. »

77-80 This reads as contradictory, including a statement that "basal melting is probably null" (I would use zero rather than null) and "Vertical velocities. . . are equal to the basal melt rate output from previous modelling. . .". I think the manuscript would benefit from an explanation and statement of possible error (in using this basal temperature field) here.

If the melt rate is null, the no-sliding condition is satisfactory. If there is a bit of melting, as the horizontal velocities are small, the sliding part of the horizontal velocities would be even smaller. But there is no contradiction with the *vertical* velocities being equal to the melt rate. So we modified the first sentence as follows :

L103 : « We here focus on a region where basal melting is probably not present or limited, and horizontal velocities are very small, so that for the sake of simplicity, a no-sliding condition is imposed at the bottom of the ice column. »

94 "has more influence"

L122 : Changed for « has more influence »

124 "used"

L152 : Changed for « used »

128-130 This sentence is unclear

132-133 I do not believe this is a valid argument: either the analysis is fit for purpose or it is not. For the former to hold then errors need to be constrained. If, as a consequence of the 'practicable' analysis undertaken, errors are large then the manuscript would benefit from those large errors being stated as usefully as possible.

This sentence was unwise (128-130). The idea is that our approach is more a comparison between sites rather than a discussion on the value of the age. The paragraph is restated as follows :

L156 : « If the absolute value of the age, age resolution, or strain rates can be discussed regarding the choices of the model parameters, we reckon their spatial variabilities are robust since they depend mainly on the shape of the bedrock and of the ice surface. As a consequence, this study focuses on comparing promising locations the ones with the others rather than on discussing the influence of model parameters. »

158-159 Presumably reflected radar power could inform as to the current location of this boundary. Do such data exist and/or has such an interpretation been published elsewhere?

The problem with the bedrock reflectivity as a function of bed elevation is that that needs the ice attenuation, which we generally calculate from correlating bed echo strength against bed elevation (eg Zirrizotti 2012)… which is circular. So we are confident that our approach is the simplest one and the more unambiguous.

170 "arch" and "radargrams"

L195 : Correction made

Figure 2 Given the importance of these domains I find that the green on brown shading isn't working very well. In fact, this figure could be improved in several ways including: formally numbering panels a-e; increasing axis label font size in a-d; changing the depiction of the oldest ice targets considering the underlying green bed elevation band; and rewording the first line of the caption to a more standard format.

We changed the color of the selected areas for a yellow one, and we labelled the upper panels. Axis label of upper front are deleted since they are identical to the ones of the bottom panel.

183 I wonder why this is “discarded” rather than included as a mask in the way that other spatially-distributed variables are?
We could have added a specific mask for « ice that crossed this high-shear zone », but no significant area concerned remained in the combination of the 5 masks, so it was not necessary. Anyway, a basic reasoning on the length of the trajectories is enough to focus on areas upstream of this high shear zone. Now we highlight the higher-shear zone by a specific mattern on the map.

186 This reference to “bottom” highlights the need for panel labelling in the figure
L211 : We now refer to panel e)

200 “However, it allows a restricted area to be defined where. . .”
L224 : Changed for « However, it allows a restricted area to be defined where a new set of observations will be the most valuable »

206 I believe “overhanging its environment” is inaccurate and does not convey the intended meaning. I think this argument needs to be clarified and formalized.
The argument needs a figure to be much simply explained, and is anyway not crucial. So we removed this couple of sentences.

Figure 3 caption “shows”
Correction made

221 I would replace “On the contrary” with “In contrast”
L240 : Changed for « in contrast »

223 Could the references here to “upper part” and “left part” be replaced with compass directions north and west?
We could do that, but as the north is not oriented upwards, indications of orientations are not intuitive at all, so we keep the present formulation.

226 I’m not familiar with the third from final word in the sentence.
L245 : Changed for « criterion »

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Brief communication: Candidate sites of 1.5-Ma ice 37 km southwest of the Dome C summit, East Antarctica

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Abstract.

The search for ice as old as 1.5 Ma requires the identification of places that maximize our chances to retrieve old, well-resolved, undisturbed and datable ice. One of these locations is very likely southwest of the Dome C summit, where elevated bedrock makes the ice thin enough to limit basal melting. A 3D ice flow simulation is used to calculate five selection criteria, which spatial variability is used to locate areas that have better glaciological properties than elsewhere. These selected areas (a few km^2) lie on the flanks of the Dome C bedrock high, where a balance is found between risks of basal melting and sufficient age resolution. Within these areas, several sites of potential 1.5 Ma-old ice are proposed, situated on local bedrock summits or ridges. The trajectories of the ice particles towards these sites are short, and the ice flows over a smoothly-undulating bedrock. These sites will help to choose where new dense ground radar surveys should be conducted in upcoming field seasons.

1 Introduction

Antarctic ice is an exceptional archive of the Earth's paleoclimates across all the glacial/interglacial periods, and the only one that contains direct samples of ancient atmospheres. The oldest available ice archive goes back 0.8 Ma in time (EPICA Dome C ice core, Jouzel et al., 2007), but is not old enough to study a main climatic transition that occurred between 1.2 Ma and 0.9 Ma, known from the temporal variations of the isotopic composition of benthic sediments (mid-Pleistocene transition, MPT, Lisiecki and Raymo, 2005). The main climatic periodicity and the amplitude of climate cycles seem to radically change during the MPT. There is presently no general agreement on the processes responsible for this transition and its origin, and the influence of the trend of atmospheric CO_2, the dynamics of ice sheets in the northern hemisphere, the sea ice extent, or the dust content of the atmosphere have been proposed. Considering these scientific issues, locating a future 1.5 Ma-old ice drill site was identified as one of the main goal of the ice-core community (see an overview in Jouzel and Masson-Delmotte, 2010).

Thermal and mechanical ice flow simulations have to be carried out to assess the potential of possible drill sites (Fischer et al., 2013). The present study is part of a set of three modelling exercises led by the IGE (Institut des Géosciences de l'Environnement, Grenoble) group and collaborators around Dome C, and more specifically on the bedrock high lying ~ 40 km southwest of the Dome C summit, suspected to be a good old-ice candidate (Van Liefferinge and Pattyn, 2013), under the ice ridge linking Dome C to the Vostok region (for a precise description, see Young et al., 2017). First, a 1D thermal model was run over the last 0.8 Ma to determine the present state (temperate or cold) of basal ice, which was compared to the reflectivity map in the region of Dome C (Passalacqua et al., 2017). The value of the local geothermal flux was inferred, and the spatial distribution of interpreted subglacial water was explained. The results of these transient simulations show that the top of this bedrock high is likely to be melt-free on long timescales, or to undergo very limited basal melting. Hence, this subregion is thermally compatible with the archiving process on glacial/interglacial timescales. Second, a kinematic 1D ice flow model was used to evaluate the age and age resolution of the deepest portion of the ice sheet (Parrenin et al., 2017). The distance between the dated isochrones and the modelled ones was minimized to infer a thinning parameter that characterizes the vertical deformation through the ice column. We showed that the observed isochrones are compatible with high basal age and provide sufficient resolution. However, these two previous studies neglect horizontal motion of ice, whereas the trajectories of the ice particles are not only vertical. The travel time of these particles, and consequently their age, is strongly influenced by the shape of the bedrock and the ice surface. These prior studies lacked the description of the local 3D state of stress of the ice, where the geometry of the terrain and the strain history of the ice particles can be properly taken into account.

To evaluate the quality and position of deep old ice, we here proceed to a steady state 3D ice flow simulation, at a regional scale. Whereas these two previous 1D modelling results alone could not help us determine the better oldest-ice targets, we here intend to provide objective criteria that together delimit kilometer-scale areas of old, well-resolved, undisturbed basal ice. Note that the goal of this study is not to assess the best age estimation, but to evaluate which sites have better glaciological characteristics than others. The bottom-most ice recovered should be older than the MPT, ideally as old as 1.5 Ma such that several climate cycles pre-MPT can be recorded. The vertical age resolution has to be better than $10\,\mathrm{ka\,m^{-1}}$ to detect high-frequency climatic variability in the ice core (J. Chappellaz, personal communication). Moreover, basal ice will probably be disturbed, similarly to the deepest 60 m of the EPICA Dome C ice core (Jouzel et al., 2007; Tison et al., 2015). We have no better evaluation of the height of disturbed ice in the region, and we will similarly take 60 m as a safety distance for the drilling minimum distance from the bedrock, but we should keep in mind this cutoff height could be an underestimation. One should look for places where the mechanisms responsible for stratigraphy disturbances (cumulated basal shear, bedrock roughness) should be minimal. Convergent flow should be avoided as well, because it tends to thicken basal layers. This is unfavourable to recovering oldest ice as it will shift older layers downwards, and makes dating process more complex (Tison et al., 2015). Finally, the location of the future drill site should be above the highest subglacial lake detected by the radar survey (Young et al., 2017), otherwise the risk of basal melting could be drastically increased. We will call this threshold "water limit", above which there is no evidence of the presence of water in the radargrams.

Another modelling study has been recently led at a continental scale to locate a future drill site (Van Liefferinge et al., 2018). They used a transient thermodynamical model to compute the highest geothermal flux value that keep basal ice frozen. By comparison with available continental geothermal flux datasets, they locate areas where basal ice has likely remained frozen over 1.5 Ma. The authors also included a further mechanical constraint representing limited impact of bedrock roughness on the preservation of the bottom ice stratigraphy. Our study and the one of Van Liefferinge et al. (2018) differ in the way they account for the heat budget of ice and for the mechanical constraints on the basal ice. We should consider these differences as an interesting source of information for the decision-making process, and they will be discussed in § 3.5.

The decision-making process of a drill site needs both field survey and modelling, the former feeding the latter with geophysical constraints, and the latter reducing the areas of interest for new field surveys, focusing more and more on promising sites. The information brought by the present study should be sufficient for a last dense radar survey to be led on promising sites during the next field seasons, at a scale of a few hundred of meters. Then the community should be able to take a decision for a drill site in the Dome C region.

2 Ice flow model

2.1 Model description

The Stokes equations are solved on a 83×114 km domain, approximately centered on the Dome C (Fig. 1), using the finite element model Elmer/Ice. The surface and bed geometries are provided by the Bedmap2 data set (Fretwell et al., 2013), except on the bedrock high southwest of Dome C, where a dense airborne radar survey has been recently collected (Young et al., 2017). The model works in ice-equivalent, and we adjust the surface height by assuming that the density profile of the firn is the one of Dome C on the whole domain (Schwander et al., 2001). Horizontal resolution of the corresponding mesh is 1 km. The resolution of the 20 vertical elements of the mesh evolves linearly, so that the deepest one being 25 times finer than the upper one, and to ensure that the velocity field is better resolved near the bottom.

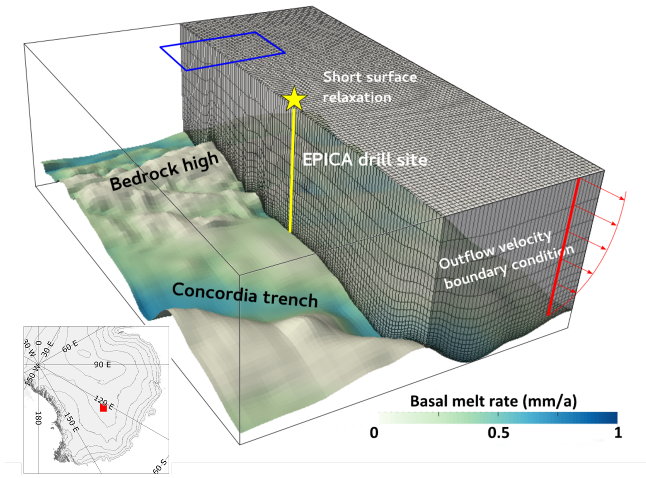

Figure 1. Mesh, bedrock dataset (Fretwell et al., 2013; Young et al., 2017) and basal melt rate (Passalacqua et al., 2017) used for the simulation on a 83×114 km domain. The red patch on the context map (bottom left) shows the domain used for the calculation, and the blue rectangle at the top of the image is the location of Fig 2.

Our domain sits in the center of the Antarctic plateau, and lateral boundary conditions do not correspond to physical boundaries, but to virtual vertical surfaces. As the domain contains the dome summit, the ice flow is divergent and no input flow should be considered. We impose velocity boundary conditions corresponding to the shallow-ice approximation (SIA) (Hutter, 1983). The ice surface as observed today may not correspond to the chosen rheology and is relaxed for 50 years such that the present surface slope does not induce an unrealistic velocity field. The ice surface is very flat, and 50 years is enough to accomodate the surface altitude to the ice rheology up to ~ 1 m, without radically changing the orientation of the ice ridge, on which we have little control. The part of ice motion attributed to basal sliding is not known precisely in the Dome C region, and depends on water circulation. We here focus on a region where basal melting is probably not present or limited, and horizontal velocities are very small, so that, for the sake of simplicity, a no-sliding condition is imposed at the bottom of the ice column. Vertical velocities at the base are equal to the basal melt rate output from previous modelling work (Fig. 7 in Passalacqua et al., 2017).

As we know that the basal ice in the Dome C region is at or near the melting point (Passalacqua et al., 2017; Van Liefferinge and Pattyn, 2013), the temperature profile measured in the EPICA Dome C borehole is a good representation of the thermal structure of the ice in the Dome C vicinity. Hence, we account for ice temperature by using the same normalized temperature profile on all the domain. Solving the coupled thermo-mechanical equations would require excessive computing resources, without radically changing the ice fluidity – which is mainly controlled by temperature. Similarly, we do not account here for long-term evolutions of the ice sheet surface, but are aware that this assumption strongly affects the trajectories of the ice particles.

As the main interest of this work focuses on what happens for deepest ice, mechanical anisotropy of the ice has to be accounted for. The relation between strain rates and stresses are described by the generalized orthotropic linear flow law (GOLF, Gillet-Chaulet et al., 2005), given a certain vertical profile of ice fabric. By introducing a dependence on the second invariant of the deviatoric stress, this law can be extended to the non-linear case (Ma et al., 2010). The fabric profile is only known at the dome summit (Durand et al., 2009), and shows that the ice mainly undergoes vertical compression, but also longitudinal extension in the deep layers (Tison et al., 2015). However, there is no reason to use the very same fabric profile everywhere else, where shearing has more influence or the bed shape is different. In this short study we will not discuss the influence of the chosen rheology, but we first made sure that the computed surface velocities correctly simulated the horizontal velocity field measured at the surface by Vittuari et al. (2004) for different n values and fabric profiles. Hence, we decided to use the widely-accepted value of 3 for n, and a synthetic vertical fabric profile, for which the eigenvalues of the second order orientation tensor evolve linearly with normalized depth, from isotropy at the surface to a single maximum fabric at the bottom.

2.2 Model outputs

Back trajectories are computed from the 3D velocity field using a Lagrangian scheme such that the age is known along the forward trajectory of the ice particles, and an age field is generated. The age resolution could be calculated from the vertical derivative of this age field, but we found it more accurate to track the annual layer thickness λ from the ice surface (Whillans, 1979) using

$$\frac{d\lambda}{dt} = \lambda\, \dot{\epsilon}_{zz} \tag{1}$$

where $\dot{\epsilon}_{zz}$ is the vertical strain rate. This formulation neglects vertical rotation effects that tends to overturn internal layers. This assumption is reasonable if internal layers are mainly horizontal, which is the case over the studied bedrock high. The age resolution is then stated as $1/\lambda$, considered as the layer thickness for a single year.

The way ice strains by flowing over a rough bed differs depending on the shape of the bedrock underneath. Similarly, a given bedrock shape can be a convergence or a divergence area, depending on the orientation of horizontal ice flow. Once the velocity field is known, a local coordinate system (X,Y,Z) can be defined at each point, for which the X-axis is oriented along flow. The curvature of the bed perpendicular to the flow is then computed everywhere, and convergence areas are identified where the bed curvature is positive.

Beyond the computation of ages and age resolutions, the 3D simulation is also useful to detect where deep ice is more likely to be folded. Shearing will tend to amplify small wrinkles in the ice layers and so disturb the ice's basal stratigraphy, whereas longitudinal extension will tend to flatten them. Competition between shear and longitudinal stresses can be represented by a dimensionless shear number (Waddington et al., 2001)

$$\mathcal{S} = \frac{2\dot{\epsilon}_{XZ}}{\dot{\epsilon}_{XX} - \dot{\epsilon}_{ZZ}} \tag{2}$$

where $\dot{\epsilon}_{XZ}$ is the shear strain rates along ice flow, $\dot{\epsilon}_{XX}$ and $\dot{\epsilon}_{ZZ}$, the local longitudinal and vertical strain rates. Waddington et al. (2001) used this shear number as a criterium to detect if a given wrinkle can be amplified by shear. More simply here, we use it to predict the presence of undisturbed ice, whereby the smallest shear number is best.

2.3 Selection of favourable locations

If the absolute value of the age, age resolution, or strain rates can be discussed regarding the choices of the model parameters, we reckon their spatial variabilities are robust since they mainly depend on the shape of the bedrock and of the ice surface. As a consequence, this study focuses on comparing in-between promising locations rather than on discussing the influence of model parameters.

The five selection criteria (age, age resolution, bed curvature, shear number and bed height) are used to compute five masks, thresholded as follows. For bed curvature, $0\,\mathrm{m}^{-1}$ would have been the natural threshold but it was too much restrictive and we decided for a slightly higher value ($2\;10^{-5}\,\mathrm{m}^{-1}$). The shear number threshold appeared naturally by studying its spatial evolution (see §3.3). The bed height threshold correspond to the "water limit", at $480\,\mathrm{m}$. Furthermore, our results show that most of the subglacial elevated bed high southwest of Dome C is favourable to the existence of $1.5\,\mathrm{Ma}$-old ice, so we adopted more conservative age and age resolution thresholds for the selection of smaller suitable locations ($1.8\,\mathrm{Ma}$ for the age and $8.5\,\mathrm{ka\,m}^{-1}$ for the age resolution, 60 m above the bed). A boolean combination of the five masks delineate the areas fulfilling all our five selection criteria.

3 Results and interpretation

3.1 Age at 60 m above the bed

The area identified as possibly hosting oldest ice is elongated along the bedrock high, and stands at an intermediate bed elevation (mainly between $400\,\mathrm{m}$ and $550\,\mathrm{m}$ above seal level, Fig. 2a). Neither the very top of the bedrock high, nor its lowest foothills appear to be suitable for the archiving process of very old ice. The imposed basal melt rates are zero on the upper part of the bedrock high, therefore infinite age are calculated for the very basal ice. In that case, the age of the ice standing $60\,\mathrm{m}$ above the bed is strongly dependent on the ice thickness. On the top of the bedrock high, the ice is at its thinnest and the old ice is at less than $60\,\mathrm{m}$ above the bed. On the foothills of the bedrock high, the ice is thicker, basal melt rates are above zero, and the basal ice is therefore continuously being melted from the bottom.

Some places may host ice even older than 2 Ma, but they all stand below Young et al's (2017) water limit (Fig. 2, yellow line). The presence of very old ice in those areas is not impossible, but may also be the consequence of insufficient imposed basal melting. The transition between melting and frozen ice should stand somewhere on the flanks of the bedrock high, but it is difficult to pinpoint the precise location of this threshold. Despite the promising thick ice in this region that could allow old ages and sufficient age resolution, the risk of basal melting is real.

3.2 Age resolution at 1.5 Ma

Age resolution in the deepest part of the ice column is influenced by two factors. First and obviously, the thicker the ice, the better the age resolution. As a consequence, the tops of the bedrock high are rarely compatible with a sufficient age resolution of oldest ice. Bedrock flanks should be preferred, but some of the thickest ice areas on the flanks will be discarded as well because of an increased risk of basal melting. Second, for ice positioned close to the ice divide, age resolution may benefit from a thicknening effect of the deeper layers (so-called Raymond effect, Raymond, 1983). This results in a band of well-resolved ice, oriented along the ice ridge and perpendicular to the bedrock high. However, no Raymond arch is visible in the radargrams that would indicate a strong Raymond effect here. One explanation is that the shape of the ice surface at these sites is much more rounded than at the Dome C summit, and the produced along-ridge flow tend to dampen the amplitude of the Raymond arches (Martín et al., 2009). Moreover, the characteristic time for a Raymond arch to form here would be several 100 ka (Martín et al., 2009), during which the surface ridge may have moved, smoothing out the developing arches. Unfortunately, the past position and shape of the ridge are unknown, and drilling far from its present position would not guarantee a better resolution.

3.3 Stratigraphic disturbances

At a divide, the shear stress perpendicular to the divide is zero, so that in general the shear number $\mathcal{S}$ of the deep ice close to the ridge is low (~ 10). However, a serie of subglacial peaks lie immediately west of the ice divide. As a consequence the shear number increases very sharply and reaches much higher values (~ 100), and this zone of higher shear should be discarded for a future drill site (Fig. 2d, wavy area). All the areas for which the basal ice crossed this zone of high shear should be discarded as well, so the trajectories of the ice particles need to be evaluated.

3.4 Trajectories for oldest-ice spots

The best combination of age, age resolution, folding, convergence and melting criteria is shown in Fig. 2 e, revealing several spots of ice meeting the five criteria. The areas for which the trajectories are the shorter should be preferred, highlighted here by three magenta boxes. For boxes A, B and C

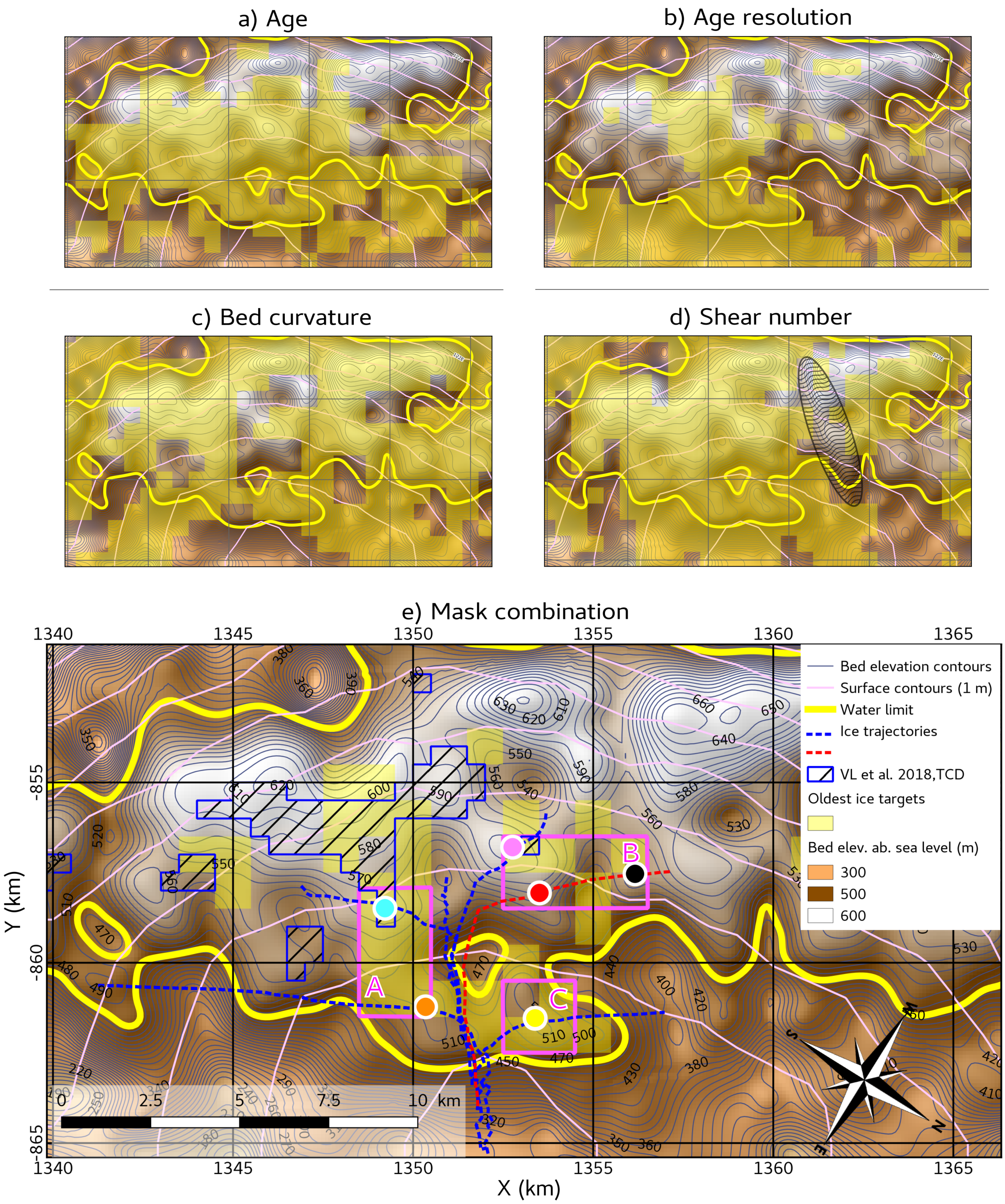

Figure 2. Selection criteria thresholded as follows (yellow areas): a) age $> 1.8\,\mathrm{Ma}$, b) age resolution $< 8.5\,\mathrm{ka\,m^{-1}}$, c) bed curvature $< 2\ 10^{-5}\,\mathrm{m^{-1}}$, d) shear number $\mathcal{S} < 40$, e) boolean combination of the selection criteria. The water limit correspond to a bed height of 480 m and is shown here by a thick yellow contour. Magenta boxes A, B and C correspond to areas that could be considered as our best oldest-ice targets. Dashed lines show trajectories of ice particles, the red one correspond to the profile presented in Fig. 3. Colored points locate possible drill sites, discussed in the text. Hatched areas show the best areas of Van Liefferinge et al. (2018). All the panels cover the same area. This figure focuses on the bedrock high designed as such in Fig.1, located $\sim 40\,\mathrm{km}$ southwest of the Dome C summit. Projection: WGS84/Antarctic Polar Stereographic – EPSG:3031 (kilometers).

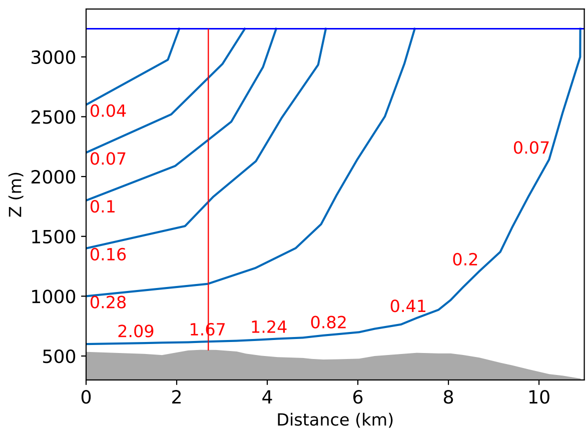

Figure 3. Trajectories of the ice particles from the ice surface towards the black point in Fig. 2 (red trajectory). Red numbers indicate the age of the ice, in Ma. The bed profile is shown in grey. The thin red line corresponds to one possible drill site (red point in Fig. 2).

the ice originates from the divide, guided by the strong lateral divergence resulting from the shape of the ice surface. Locations within boxes C and A are closer to the ice ridge and should be considered first for a future Oldest Ice drilling because of shorter trajectories, and less risk of stratigraphy disturbances. Box B in particular lies on a relatively flat bedrock platform (black point in Fig. 2e), that may ensure stability of ice flow. Figure 3 shows the paths taken by ice from the surface to this site, demonstrating that the horizontal distance travelled does not exceed 10 km from the surface. However, as there is probably no basal melting here, deep ice would closely follow the bed for several kilometers, in a depth range dominated by strong vertical shear, enhanced by an unduling bed underneath. To minimize the bed influence, we could also consider a drill site located 3 km upstream, where the ice age would still exceed 1.5 Ma (red point in Fig. 2e, red dashed line in Fig. 3).

Of course, locating a unique "best" drill site within one of these three boxes is not possible with this 3D-modelling approach only. However, it allows a restricted area to be defined where a new set of observations will be the most valuable. We should focus on local bedrock summits or crest lines, because local troughs make the ice flow converge, but also heat flow (Van der Veen et al., 2007), increasing the possibility of insufficient age and positive basal melt. Considering that, only a few set of favourable drill sites remain in boxes A, B and C (blue, orange, pink and yellow points in Fig 2e). Pink and blue points have less risks of basal melting, while yellow and orange have less risks of stratigraphic disturbances. The best choice between these sites should be now guided by local radar surveys characterizing the internal layering of the ice, and the vertical strain rate profile (Nicholls et al., 2015).

3.5 Comparison with results based on thermodynamical modelling

Van Liefferinge et al. (2018) identified a 8 km-long area covering the SE upper part of the bedrock high, that crosses our box A, but both do not overlap pefectly (Fig. 2e, hatched blue and yellow areas). They also identified a site within our magenta box B, but no site in the box C. These comparative results highlights the complementarity of the two approaches. The 1D thermodynamical model of Van Liefferinge et al. (2018) has a better control on the thermal aspect of the problem than on its mechanical aspect, and selects sites that are more conservative from a heat budget point of view, i.e. preferentially local bedrock heights. In contrast, our 3D approach accounts for the horizontal strain of the ice, and select sites that are more conservative from a mechanical point of view. The upper part of the box A or the left part of the box B validate the constraints of both approaches. In our approach, the bedrock summit in box C is the safest mechanically; however, it was not selected by Van Liefferinge et al. (2018) because of a local bedrock roughness exceeding their threshold of 20 m, despite the fact that their thermal criterion was fulfilled.

4 Conclusions

The three-dimensional ice flow simulation presented here aims at defining and calculating several objective criteria, which represent ideal conditions for the retrieval of old, well-resolved, undisturbed ice. The influence of the bedrock high and of the position of the ice ridge allows us to define only few sites compatible with all our selection criteria. The ages calculated at the base by our model simulations predict ice older than 1.5 Ma old high enough above the bedrock, which gives us confidence that the community's target of 1.5 Ma years should be attainable, with the required age resolution. However, the modelling approach implicitely assumes that the ice flow is regular down to the bedrock, but there is no guarantee it is actually the case. A ground radar survey focusing on a few hundred-meter scale areas presented here is essential to explore the structure of the deep layers. Finally, a rapid-access drill (Grilli et al., 2014) is currently planned to be deployed in the 2018/19 season to assess ice quality and age for a chosen target site, before the final site is decided upon.

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