

Response to reviewer comments RC1 on Promising Oldest Ice sites in East Antarctica based on thermodynamical modelling

We would like to first thank the Editor Eric Larour and both reviewers for their constructive reviews on the paper. In order to address all comments, you will find here our answers point-by-point, which sometimes creates some repetitive answers. We hope that we have satisfactorily responded to all comments and remarks, which can be found here below.

We would also like to bring to your attention that:

- (1) We have updated the references: Cavitte et al., 2018 which is now published, and Karlsson et al., 2018 which is now accepted for publication (in press)
- (2) A few of the figures have been increased in size for clarity, in addition to the relevant changes for the reviews
- (3) We have made a few minor additional wording and aesthetic changes throughout the manuscript.

Reviewer comments are in black, answers in blue and text edits in red.

Throughout MS: "Puruker" -> "Purucker". Changed

P2, L3: Add a semicolon after "ice sheet". Added

P2, L24: "of the crustal" -> "of the crust". Changed

P2, L25/26, "it is crucial to know basal temperature gradients at the ice-bedrock interface and not GHF within the crust": This is not necessarily true and depends on the type of the modelling study. For longer-term (e.g., glacial-interglacial cycle) modelling studies, it is more physical to use the GHF within the crust and apply it as a boundary condition some kilometres below the ice-bedrock interface.

This is indeed an important point. We copy here our response to RC2: Most methods calculate average GHF values within or at the surface of the crust, without accounting for gradients of GHF within the crust. From an ice-sheet modelling perspective, it is more realistic to know the temperature gradient at the ice-bed interface rather than a specific GHF value at the interface. The thermal gradient inside the bedrock has an impact on heat availability to the ice (Lowrie, 2007) as does the thermal inertia of the bedrock. Ritz (1987) shows that bedrock temperature will reach equilibrium after thousands of years, on the scale of several climatic cycles, after a change in ice surface temperature. She shows that the use of a 2 km thick crust for the calculation of the crustal thermal gradient is enough to accurately model the changes induced by surface temperature variations. For climate cycles with a 100 kyr cyclicity, the basal temperature perturbation at the bed is ~40% of the surface temperature perturbation if crustal thickness isn't taken into account, and 15% if it is. However knowing the composition, the thickness and the thermal conductivity of the bedrock is also a challenge. At first approximation, we can use a GHF value without taking into account crustal thickness. This simplification is frequently made in glacier and ice sheet models (e.g. Huybrechts et al., 1996; Ritz et al., 1997; Huybrechts and de Wolde, 1999; Pattyn, 2003; Pollard et al., 2005), particularly in steady-state.

We have modified our discussion to describe this simplification explicitly. The paragraph "Knowledge of GHF values at the ice-bedrock interface is a crucial boundary condition for ice flow modelling, yet it remains the most difficult parameter to measure in-situ. Constraining this parameter is therefore essential. GHF is determined by the geology of the bedrock (type of rock, presence of volcanism, etc). However, bedrock geology is unknown in the Antarctic interior and therefore cannot be taken into account in our model" is changed to "From an ice-sheet modelling perspective, it is more realistic to

know the temperature gradient at the ice-bed interface rather than a specific GHF value at the interface. The thermal gradient inside the bedrock has an impact on heat availability to the ice (Lowrie, 2007) as does the thermal inertia of the bedrock. Ritz (1987) shows that bedrock temperature will reach equilibrium after thousands of years, on the scale of several climatic cycles, after a change in ice surface temperature. However knowing the composition, the thickness and the thermal conductivity of the bedrock is also a challenge. At first approximation, we can use a GHF value without taking into account crustal thickness. This simplification is frequently made in glacier and ice sheet models (e.g. Huybrechts et al., 1996; Ritz et al., 1997; Huybrechts and de Wolde, 1999; Pattyn, 2003; Pollard et al., 2005), particularly in steady-state."

P4, Fig. 1: If I'm not misled, this figure is not referenced anywhere in the text. Fig. 1 was mentioned in the Results section but we have now referenced this figure earlier in the manuscript, in section 2.3 first, and several times later on. Finally, background temperature changes $\Delta T(t)$ are taken from the reconstruction of Snyder (2016), discussed in the section 4.1, scaled to Dome C ice-core temperature reconstruction (Parrenin et al., 2007) (Fig. 1).

P5, L17/18, "horizontal advection may safely be neglected": What about horizontal conduction? This is a good point. Ignoring horizontal heat conduction is widely used as a 1D approximation as well.

Horizontal heat conduction (horizontal diffusivity) may have an effect if the ice thickness is changing rapidly over short distances. For relatively constant ice thickness within windows of the order of magnitude of an ice thickness (3-4 km), the conduction will be low as horizontal temperature gradients and second derivatives are small (because surface temperatures are rather constant over such distances. This is not the case in the vertical, where both T gradients (order of 50K over 3km) and second gradients (shape of the T profile is not linear) are large.

We changed the sentence as follows: In divide-adjacent areas, horizontal advection and horizontal heat conduction may safely be neglected as for areas with a relatively smooth bed, horizontal conduction is much lower than vertical conduction (Hindmarsh, 1999, 2018).

P5, Eq. (2): This boundary condition only holds for a cold base. We agree and therefore explicitly state this in the sentence as follows:

The basal boundary condition for a cold base bed is given by

P5, Eq. (3): This equation should be given a reference. Added. (Hindmarsh, 1999; Pattyn, 2010)

P5, Eq. (3) and L26 vs. P6, L3 and Eq. (5): The surface accumulation rate should consistently be denoted by either "a" or " \dot{a} ". We agree, this was a mistake as we always use accumulation as rate. We have therefore changed the notation everywhere to " \dot{a} ".

P6, Table 1: Why not using the more realistic, temperature-dependent representations of the thermal conductivity and the heat capacity? For the large range of temperatures relevant for Antarctica, the dependence is significant (e.g., Greve and Blatter 2009, "Dynamics of Ice Sheets and Glaciers"). The reviewer makes a good point. We made the test following e.g. Ritz (1987) given $K_i = 9.828 \exp(-0.0057 T)$ in $W m^{-1} K^{-1}$, where K_i is the conductivity depending on the temperature (T) inside the ice.

Using a temperature-dependent representation of parameters has very little impact in our calculations. We did a test close to Dome C site with a G_{pmp} calculated with fixed parameters at $51.7 mW m^{-2}$. The same calculation with temperature-dependant parameters gives a value of 53.6 for the G_{pmp} (3.5 % of difference). Taking temperature-dependant parameter constrains our result less as the G_{pmp} is higher.

P6, L10: I think it is problematic to keep the bed elevation constant in time and then apply a time-varying ice thickness produced by a model that includes isostatic adjustment (Pollard and DeConto

2009). This procedure overestimates surface elevation variability and thus surface temperature variability over time. Why not including a simple local lithosphere-relaxing-asthenosphere model? This should be easy to implement, not consume much extra computing time, and it is quite realistic for the interior of Antarctica due to the enormous horizontal extent. This is definitely an important remark, and probably due to an unclear description of our method. Strictly speaking the variation in surface temperatures shown in the manuscript is obtained by scaling present day temperatures with paleo ice elevation (given by Pollard and DeConto, 2009). We do not use present day bed elevations for the model results but only use ice thickness variations given by Pollard and DeConto (2009). To summarise, we didn't use a bed relaxation model but we used the bed elevation variations and ice thickness variations given by Pollard and DeConto (2009) that already take isostatic adjustment into account. We have now changed the paragraph to: Surface elevation changes with time are obtained from changes in ice thickness with time obtained from a model that takes into account isostatic adjustment, given by $s(t) = b + H(t)$, where b is the varying bed elevation varying with time and $H(t)$ the time-varying ice thickness, ...

P7, L2: "500 m by 500" -> "500 by 500 m". Changed

P10, L3/4, "Although, the regions highlighted...": I don't understand this sentence.

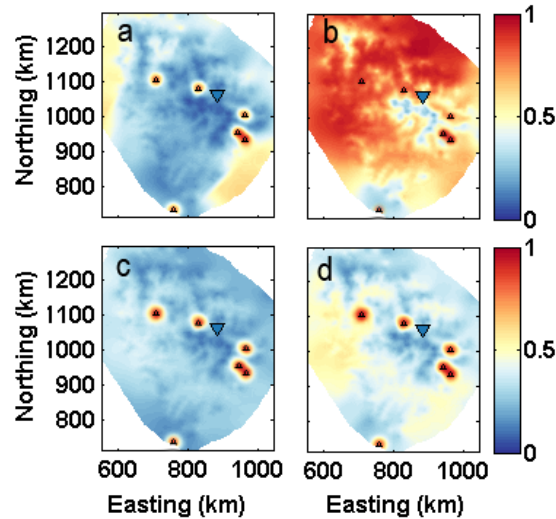
We wanted to refer to regions with very high or very low G_{pmp} values. The sentence was therefore changed to:

Although, the regions with very high or very low G_{pmp} values highlighted in the G_{pmp} distribution map stand out on the three maps...

P10, L7: "our analyse is more contrasted" -> "our analysis is more contrasted". Changed

P11, L4/5: I'm not sure whether the Shapiro and Ritzwoller (2004) GHF values are the best reference. If I interpret Figs. 7b and 8b correctly, this produces probabilities of Dome F and Dome C having reached the PMP of 0.3 and 0.5, respectively. However, if I remember well, direct observations have shown that both ice cores are warm-based today. This challenges the credibility of the presented results. Further, I have found that the Martos et al. (2017) data generally produce better results for ice flow and basal temperature in 3D, large-scale simulations of Antarctica (recent work, unpublished).

We agree this was unclear. We removed the confusing and not correct sentence "The probability map is generated with the $\text{\cite{shapiro04}}$ GHF values, which exhibits the closest GHF to calculations $\text{\cite{seddik11,hondoh02}}$ and therefore places a limit to the probability" Martos et al. (2017) use a completely different method than Shapiro and Ritzwoller (2004). In general, Shapiro and Ritzwoller (2004) obtain higher values of GHF in the interior of the ice sheet compared to Purucker (2013) and An et al. (2015), but lower than Martos et al. (2017). However, looking at dome areas, Martos (2017) GHF values are clearly higher than any of the other methods' results. Here below, we show a table of GHF values at the domes for the five different published data sets. Martos et al. (2017) data does have the advantage of a higher spatial resolution which could be beneficial for 3D model calculations, but a higher resolution can result simply from model mesh refinement and not GHF knowledge accuracy. Fig.6 shows the probability for 3 data sets: xx, xx and Martos et al. (2017). We can clearly see that Martos et al. (2017) show the highest probability of being at the pmp in the dome regions (see Fig. below). Instead of choosing what is the "best GHF data set", we have opted here to use all published data sets together so that they balance out their strengths and weaknesses.



Probability that ice reached the pressure melting point over the last 1.5 Ma according to: a) Purucker (2013) b) Martos et al. (2017) c) An et al. (2015) d) Shapiro and Ritzwoller (2004)

So model results shown in Fig 7d and 8d use all five GHF datasets to constrain the promising sites. However, in panels b of each of those figures, we simply chose to display one of the five GHF datasets. In this case, we chose Shapiro and Ritzwoller (2004) as their mean GHF is less extreme than others.

Geothermal heat flux values at the domes in $mW m^{-2}$

	Dome Fuji	Dome A	Dome C
Shapiro and Ritzwoller (2004)	50	47	45
Fox Maule et al. (2005)	59	53	56
Purucker (2013)	40	36	42
An et al. (2015)	40	46	44
Martos et al. (2017)	65	54	58
Median value	50	47	45
Standard deviation	11	7	7

P12, L4: "on Fig. 7" -> "in Fig. 7". Changed

P14, L22: "the values lies" -> "the values lie". Changed

P14, L26: I think the reference to Fig. 9 is wrong.

Apologies, this sentence was corrected.

The probability maps of frozen bed conditions obtained (Fig. 7 B and Fig. 8 B) refine the Oldest Ice candidate sites first described in Van Liefferinge and Pattyn (2013).

P14, L29, "Spatial and temporal forcing variations with respect to surface temperature and accumulation rate are relatively limited (Fig. 9)": Looking at Fig. 9, these variations don't seem to be so small. We agree that variations can be larger for these variations. But what we meant here is that, if look at areas of interest for Oldest Ice (i.e. where velocity is less 2 m/year), surface temperature and surface accumulation rates don't vary much from one dome to the next, while mean ice thickness and variations in ice thickness are spatially heterogeneous for our areas of interest. We have changed the paragraph to:

Surface temperatures and accumulation rates are spatially relatively homogeneous in our regions of interest (Fig. 9 B and D).

P15, L15, "high probability of being below or close to the pmp": What is meant by "below or close"? We agree this statement is a little useless since temperature of the bed cannot be above the pmp. We have therefore changed the sentence as follows:

The bed in the Dome Fuji region has a high probability of being close to the pmp.

P16, L8, "maximum radial distance of 4 km and 2 km from Dome Fuji and Dome C": Are these numbers correct? If so, I don't understand it. Earlier in the paper (Figs. 7 and 8), much larger windows around these two sites were discussed. How does this go together?

Our sentence was confusing. In both cases the criteria is the same: we require a distance to radar lines less than 2 km or 4 km for Dome Fuji and Dome C, respectively. And not only from Dome F or Dome C (the "from" was problematic). We changed the sentence to:

To take into account the influence of the resolution of the radar surveys, we restrict ourselves to a maximum radial distance from any radar line of 4 km for the Dome Fuji region and 2 km for the Dome C region.