

***Interactive comment on* “Brief communication: “Oldest Ice” patches diagnosed 37 km southwest of Dome C, East Antarctica” by Olivier Passalacqua et al.**

Olivier Passalacqua et al.

olivier.passalacqua@univ-grenoble-alpes.fr

Received and published: 18 May 2018

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

[Printer-friendly version](#)

[Discussion paper](#)



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

[Printer-friendly version](#)[Discussion paper](#)

variability is used to locate areas that have better glaciological properties than elsewhere. These selected areas (a few km²) 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

[Printer-friendly version](#)[Discussion paper](#)

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."

[Printer-friendly version](#)[Discussion paper](#)

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 pattern on the map.

186 This reference to 'bottom' highlights the need for panel labelling in the figure

[Printer-friendly version](#)[Discussion paper](#)

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"

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-19>, 2018.

Printer-friendly version

Discussion paper

