

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

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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 fruitful 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 developed "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"

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"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 $\hat{\Delta}l_{ij}$ 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

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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"

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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"

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