Interactive comment on “Is there 1.5 million-year old ice near Dome C, Antarctica?” by Frédéric Parrenin et al.

H. Conway (Referee)
conway@ess.washington.edu

First of all, we would like to thank you for your constructive and careful review!

The authors make a compelling case for the existence of >1.5myr-old ice near Dome C; nice work!

Thank you!

They leverage existing ice-core data from nearby EPICA Dome C, airborne radar data, and an established 1-D ice-flow model, [Concerning the radar, you might also cite the recent manuscript: Winter et al. 2017. Comparison of RES measurements and synchronization with the EPICA Dome C ice core. The Cryosphere, 11, 653–668, www.the-cryosphere.net/11/653/2017/, doi:10.5194/tc-11-653-2017].

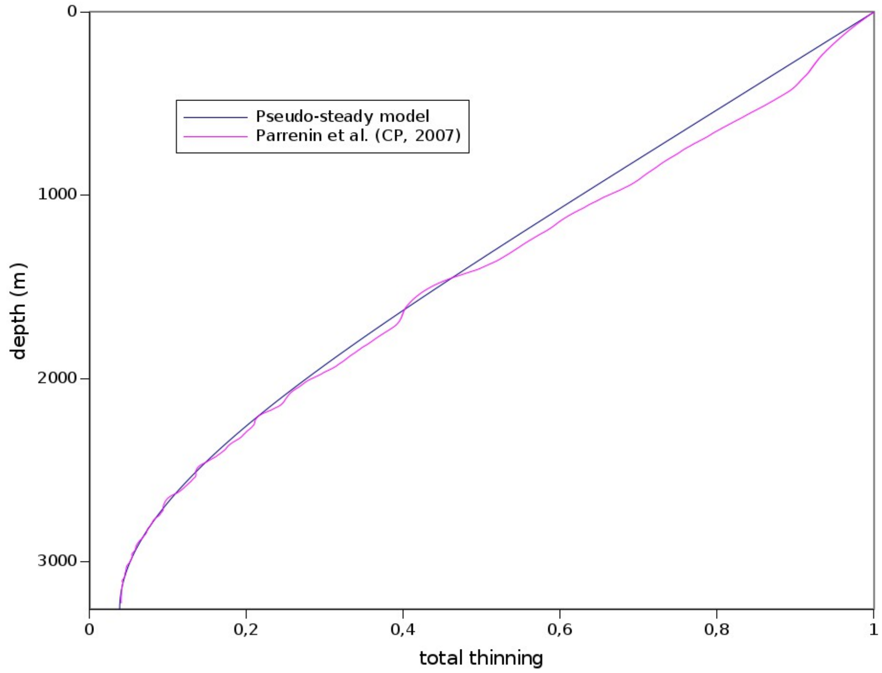
We now cite Winter et al. (2017):

In this study, we concentrate on airborne radar transects (Figure 1), which are all related to the EDC ice core. These data resolve the bed (Young et al., 2016) and internal isochrones (Cavitte et al., 2017) ***and are suitable for Oldest Ice search (Winter et al., 2017)****.*

I have a few questions/comments:

1. Eqn. 2, suggests that R(t) is spatially invariant and that temporal variations in accumulation rate and melt rate are covariant. Is that the assumption? If so, how does this fit with statements on lines 75-80 that melt rate varies with ice thickness, geothermal flux and accumulation rate? I would have thought that R for melt would vary both spatially and temporally. Perhaps this is related to your model result of high spatial variations of geothermal flux in the region (eg. Line 185).

We indeed assume that R(t) is spatially invariant and is the same for both accumulation and melt rates (this is the pseudo-steady assumption). This assumption is very convenient numerically because in this case, there is an analytical expression for the thinning function (Eq. 3). Therefore, there is no need to use a costly Lagrangian scheme. Of course, the reality is more complex than that because the temporal variations in melting and accumulation rates are not related and are not the same for each point in space. In Parrenin et al. (Climate of the Past, 2007, “1D ice flow...”, fig. 5), we used a more complex age model with a ratio \mu=m/a and with an ice thickness allowed to vary in time. The results are very similar with the pseudo-steady model (see Figure below), with difference of 6% at maximum mainly due to the ice thickness variations:



This is because melting is small compared to the accumulation and the variations in ice thickness are small compared to the total ice thickness. Moreover, the 1D assumption dominates the uncertainty since we don't take into account horizontal advection and dome movement. Therefore, we reckon the pseudo-steady assumption is good enough for a 1D model. The best would of course be to use a more realistic 3D model, but this is a problem far more complex to tackle...
We added the following paragraph at the beginning of the discussion section:

In our forward modeling, we used the 1D pseudo-steady assumption. This assumption is very convenient numerically because in this case, there is an analytical expression for the thinning function (Eq. 3). Therefore, there is no need to use a costly Lagrangian scheme, following the trajectories of ice particles. Of course, the reality is more complex than the pseudo-steady assumption because the temporal variations in melting and accumulation rates are not related and are not the same for each point in space. In Parrenin et al. (2007), we used a more complex age model with a ratio and with an ice thickness allowed to vary in time. The results are very similar with the pseudo-steady model. This is because melting is small compared to the accumulation and the variations in ice thickness are small compared to the total ice thickness. Regarding the spatial pattern of accumulation, we assumed that it is stable in time, which is roughly confirmed by the inversion of internal layers (Cavitte et al., 2017). Moreover, the 1D assumption dominates the uncertainty since we don't take into account horizontal advection and dome movement. Therefore, we suggest the pseudo-steady assumption is good enough for a 1D model.

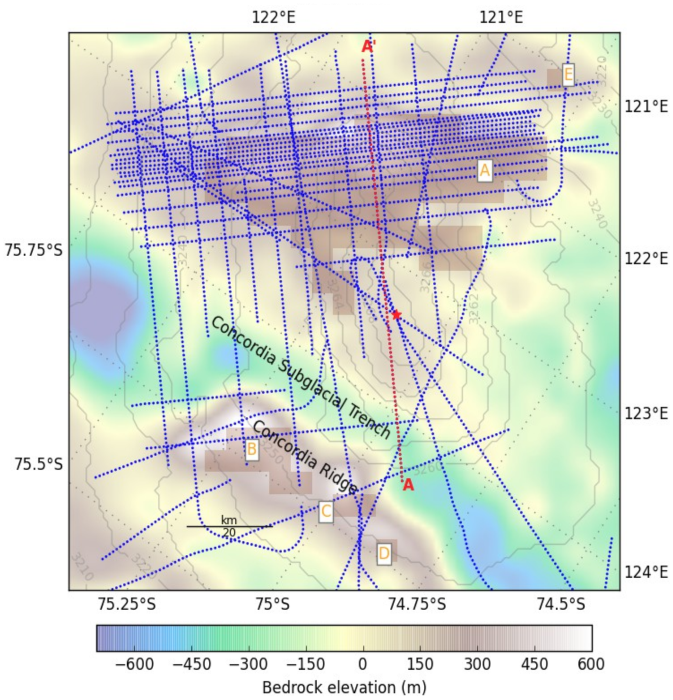
2. Lines 199-201: It is not clear that there are sufficient data constraints (especially histories of accumulation rate and thickness) to construct a realistic 3-D model. However I agree that using new rapid access technologies are the next step.

We agree that inverting a 3D model is a complex task and it could be an under-constrained problem in general, especially where the radar data are sparse. We believe it is still possible to invert the isochrones with a 3D model if we make relevant assumptions.

3. Figures are my main concern:

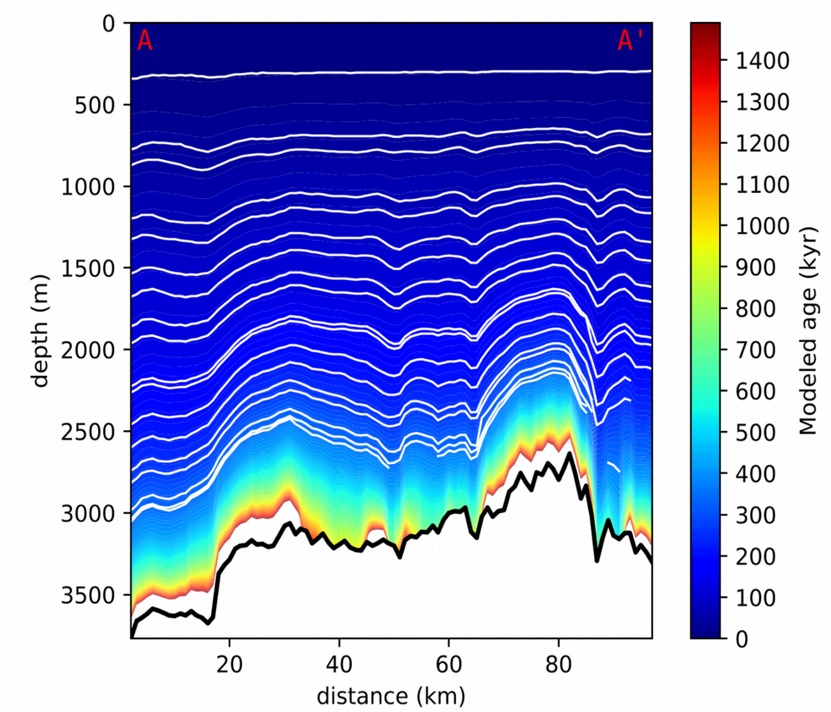
(i) Fig. 1. Text refers to directions south-west of EDC (line 164) and north-east of EDC (line 164). Fig. 1 labels longitudes but not latitudes; at a minimum a north arrow would help orient the reader.

We now label more latitudes and longitudes in Figure 1.



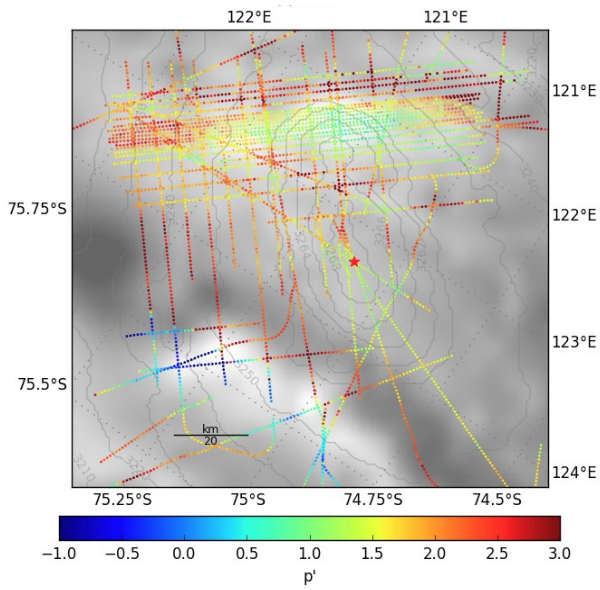
(ii) Fig. 2 shows UTIG profile and gives locations of NP and LDC along this line, and yet Fig 1. shows them offset from the profile by ∼10km and 15km respectively. Please clarify. It would also be useful to show start and end locations of the radar profile in both Figs so the reader does not have to figure it out.

NP and LDC (now LDCP) refer to broad regions of calculated basal age >1.5 Myr and not to particular points along the radar transect. They are now labeled on Fig. 4 (previously Fig. 3). Start and end of the X45 radar profile are now shown on Fig. 1 and Fig. 3 (previously Fig. 2).



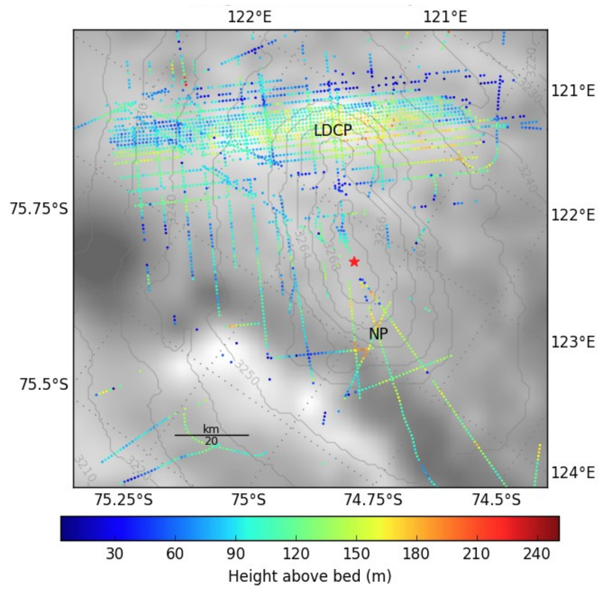
(iii). I struggle with color scales in Figs. 3&4. It is hard t discern gradients in inferred properties that are discussed in the text. Perhaps either changing the range of the color scale, or constructing line plots of a and m would help illustrate the spatial patterns discussed in the text.

We changed the colormap of the bedrock to greyscale and we increased the dot size for a better readability, for example:



(iv) Fig. 3. Line 155 you state why you use 60m above the bed as the transition between disturbed and undisturbed stratigraphy. Hence why do you show results for 150m above the bed (Fig 3, Bottom left)?

60 m is the height of disturbed ice at EDC but we cannot guarantee that it is not larger at LDCP. Therefore, we now plot the height above the bed of the ice older than 1.5 Myr:



(v) Fig. 4: are the accumulation and melt plots the temporally mean values at each point? It would be good to discuss the relevance of the spatial pattern of the p' plot, and I think a plot and discussion of R(t) would also be most informative.

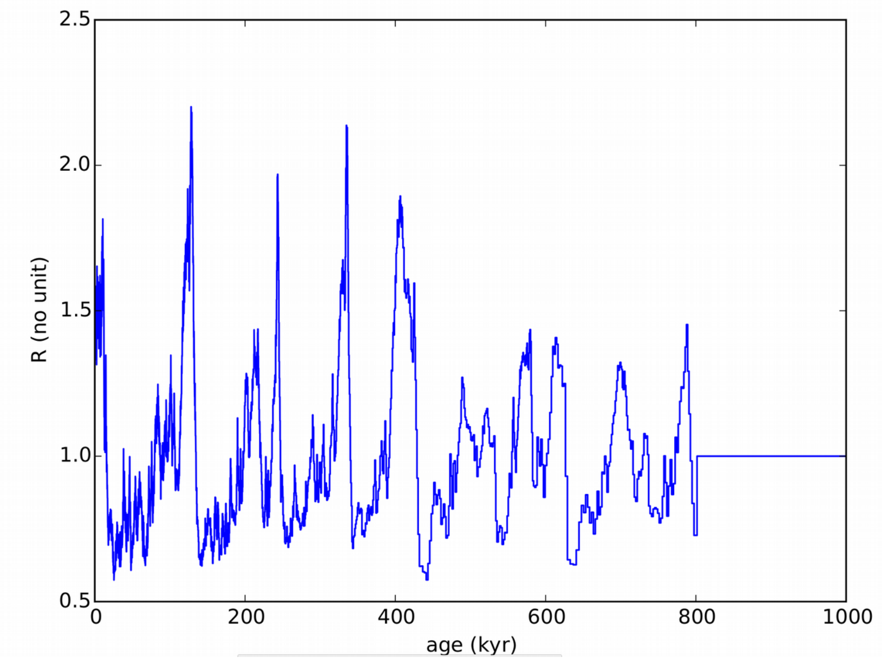
Yes, the accumulation and melt maps are the temporally averaged values at each point. We made this clearer in the figures (by using the $\bar{a}$ and $\bar{m}$ variables) and in the legend:

(Top-Left) Modeled ***temporally averaged*** *basal melting. (Top-Right) Inferred* ***temporally averaged*** *surface accumulation rate. (Left-Bottom).*

We now discuss in more details p' at the end of the discussion:

The p value inferred at EDC is 2.63, compatible with the value of 1.97±0.93 inferred from the inversion of the EDC age/depth profile (Parrenin et al., 2007). ***Over the LDC relief, our method infers low*** p' ***values, in agreement with the absence of basal melting and therefore basal sliding.*** *This value increases over the Concordia Subglacial Trench and* ***on the south-west side*** *of the LDC bedrock relief, which is probably a sign of increased basal sliding due to the presence of melt water at the ice/bedrock interface. The very low* p' *values on the Concordia Ridge adjacent to the Concordia Subglacial Trench are again probably an artifact of the 1D assumption.*

We now plot $R(t)$ in Fig. 2 and we discuss the pseudo-steady assumption at the beginning of the discussion section (see answer to your comment 1):



Other editorial comments: Lines 21, 156, 170, 242, 243, and perhaps other places. One of my mentors has pointed out many times that "inverting" or "inverted" means "to turn upside down"; "to reverse in position, order, direction, or relationship". That is, although you use inverse problem methods, you are not inverting solutions. Rather you are inferring, or deducing solutions.

Corrected throughout.

Line 89: "poorly known" is used twice in the same sentence.

One has been removed:

Radar observations allow estimates of poorly known ice-sheet parameters, such as the geothermal flux (Shapiro and Ritzwoller, 2004) and past changes in the position of ice domes and divides.

Eqn:13, perhaps better written as m= (Go-G)/(piLf); [when Go>G; 0 when Go<G]

Corrected:

$$m = \frac{G_0 - G}{\rho_i L_f} \quad \text{(temperate base)}, \qquad \text{or} \quad m = 0 \quad \text{(cold base)}, \tag{13}$$

Line 156: "stratigraphically" rather than "stratigraphycally".

Corrected:

*We use 60 m above the bed as this is the height at EDC below which the ice is disturbed such that it cannot be **stratigraphically** interpreted (Tison et al., 2015).*