Response to editor decision

We thank the editor for their time and consideration and understand their choice to ask for major revisions given reviewer comments.

We address the critical comments from reviewer 1 by now referencing the article in preprint Parrenin et al. (2025) throughout and adding more descriptions to the methods section.

We have also modified some of the wording choice around the age of the Beyond EPICA ice as some preliminary results have been released to the press but a full analysis of the core is still yet to take place and be published.

Response to review 1

Reviewer comments
Author comments
New text in manuscript *(line number)*

We thank the reviewer for their comments and address their concerns with the lack of description of the forward model here.
A companion article detailing the full analytical method used in the forward model is now available in preprint Parrenin et al. (2025). We hope that it provides the technical details that the reviewer requested here.

We would like to highlight that this article is intended as an application of an existing model at Dome C and a discussion of its effects on the Beyond EPICA drill site. We felt that including both the analytical model development and fully discussing the application would be too heavy for a single paper, and these 2 aspects are relevant for different audiences. Therefore, in this article we focus on the application at Dome C.

This study present a so called 2.5D inverse model for simulating the age–depth relationships along a flow line from Dome C to Little Dome C in Antarctica. This is an important topic in glaciology, e.g., finding a location of an old ice core, which matchs the scope of The Cryosphere. However, I find it is very difficult for me to understand this manuscript, and at some places I feel not much senses to me.

The analytical development is heavily based on the work of Parrenin and Hindmarsh (2007). The idea to infer a mechanical ice thickness is based on Chung et al. 2023 as mentioned in section 2.1.

The main reason is that the authors do not even put some basic explanations of their model. I do not undertand why “a full description of the forward model will be available in a separate subsequent article” is possible if they do not make it clear in this paper? For example, Eqn 1 gives the vertical velocity profile, but what does the horizontal ice velocity (flux) look like? Both horizontal and vertical ice velocity are critical in determing the shapes of age-depth profiles.

We apologise for the cryptic statement here. At the time we submitted this paper, we were still writing the full description of the model. However, we found that the Cryosphere does not allow an article to be referenced as “in prep”, hence the statement. We have now referenced the preprint, Parrenin et al. (2025), throughout.

The horizontal flux is shown in Eq. 1 of Parrenin et al. (2025) and is the integral of the flow tube width, snow/ice density and horizontal velocity. Horizontal velocity depends on the normalised stream function (first defined by Parrenin and Hindmarsh, 2007). For the forward model, in this study, melting flux is zero, therefore the horizontal velocity simplifies to take the form of the derivative of ω.

The authors also do not explain basically the inverse model, e.g., what is p_prior and why they need to use a term “mechanical ice thickess” since they have already the observed ice thickness data - this do not make much sense not using the observed data!

The inverse model is very much related to the method applied in Chung et al. 2023. Indeed, this is where there is a full description of "mechanical ice thickness” and how it’s comparison to observed ice thickness leads to the inferences of the basal condition of the ice. The use of measured rather than inferred/mechanical ice thickness would preclude the existence of a basal layer.
However, given the reviewers comments, we have now re-described these aspects of the model as this is a relatively new method which the reader may not be familiar with.

Text modified in Sec 2.2 (lines 117-126):
The inverse model performs an optimisation to infer three parameters at each horizontal position, x: steady accumulation, $\bar{a}$ (as defined in Sect. 2.1); Lliboutry thinning parameter, p (Eq. 1); and mechanical ice thickness, Hm. The mechanical ice thickness (Hm, first defined in Chung et al. (2023b)) is the best fit ice sheet thickness when considering the shape of the isochrones given no basal melt rate (Fig 1. from Chung et al. 2023). The difference between the mechanical ice thickness, Hm and the observed ice thickness Hobs is used to determine either a basal melt rate m or the thickness of a stagnant ice layer, as in Chung et al. (2023b). Where Hm < Hobs, there is a stagnant ice layer of thickness Hobs − Hm. The label “stagnant" is used as the vertical velocity profile is assumed to be zero at depths greater than Hm. Where Hm > Hobs, there is basal melting m which is calculated using the value of the ice flux Δq over the horizontal distance interval Δx at depth Hobs,

(Eq. 2),

where Y is the flow tube width.

Actually, the age-depth modeling is not new. If the authors can dig a bit in past literatures, they can easily find some nice and important papers, like Greve et al. (2002) and Rybak and Huybrechts (2003). Based on ice flow models and the Eulerian or Lagrangian methods, we can determine the age-depth relationships in a more physical way. I do not see the authors even mention these previous work in the Introduction section.

As mentioned above, this article is intended as an application of the model near Dome C, therefore the introduction is more focused on what has been done previously at this location, not on age modelling in general. A more modelling oriented introduction can be found in the Parrenin et al. (2025) preprint, including reference to Rybak and Huybrechts (2003).

Sentence now added to introduction (line 29):
There are many types of numerical schemes which can be used to model ice flow and the age-depth relationship in an ice sheet (Greve et al., 2002, Rybak and Huybrechts, 2003).

I am not sure what the "forward model" is about if they do not use a physical ice flow model. If I am not convinced the velocity field is correct, it is also hard for me to believe the inversed age-depth profiles are correct either. The inverse model is then just an optimization approach to find the numbers that match the radar chronology record, but without much physics inside.

The forward model refers to the model presented in Parrenin et al. (2025) which uses the coordinate system from Parrenin and Hindmarsh 2007. Both of these papers give a full account of the physical ice flow model, therefore we do not go into so much detail in the article. In this study, we highlight only the adaptations to these previous works in order to make the inverse model.

We modify the introductory paragraph in Sec 2.1 - Forward model (lines 90-92) to make this clearer:
The forward model is based on the analytical development of (Parrenin and Hindmarsh, 2007), with a numerical model presented in our companion paper Parrenin et al. (2025). As the numerical scheme is fully described in these articles, here we outline the benefits of this method and the slight changes required to make this suitable for an inverse model.

We follow this with a more detailed description of the mechanics of the model without going into the mathematical detail already provided by Parrenin et al. (2025) and Parrenin and Hindmarsh, 2007. (lines 92-100):

We use a pseudo-steady model which includes a steady-state geometry and velocity profile. The model determines the path of particles through the ice sheet by considering the total ice flux q through a flow tube with width Y as in Parrenin et al. (2025). In this study, the flow tube width is variable along the flow line in the x direction but constant in the vertical direction z.

We use the equation for ice particle trajectories presented in Parrenin and Hindmarsh (2007) and build a grid that follows these trajectories. This method has the advantage that ice particle trajectories pass exactly through grid nodes so no interpolation is required in the forward model. Given the increasing horizontal flow speed along the flow line, this means that the grid along the x axis is finer near the dome and coarser further downstream. In the z direction, the grid is coarse near the surface and becomes finer towards the bed where ice layers have thinned considerably (for more information see Supplementary material and Fig. S1).

The writings also make me confused often. For example,
L13, "the 2.5D model predicts a basal layer 200–250 m thick at the base of the ice sheet", what is "basal layer 200-250 thick"?
Reworded to (line 13): *the thickness of a modelled basal layer is 200-250 m at the base of the ice sheet. This is a layer of ice above the bedrock which seems to have different flow behaviour to the ice flowing above.*

L68, "There is no direct thermal representation in the forward model as we use an inferred mechanical ice thickness to determine a basal melt rate", this sentence comes from no where, and has no references and no explanations.
This sentence relates to Parrenin and Hindmarsh (2007) (for the forward model) and Chung et al. 2023 (for mechanical ice thickness) which are referenced in the sentences before and after this one. As this is not clear as a stand alone sentence we have now added more description.

We modify the introductory methods (Sec 2, lines 70-88) to:

We present a 2.5D ice flow model that uses inverse methods, constrained by radar observed isochrones, to fit poorly known parameters. The basic forward ice flow model is based on the analytical development presented in Parrenin and Hindmarsh (2007). Their numerical scheme was then developed into a forward model by Parrenin et al. (2025). This method is particularly efficient, as it performs a coordinate transformation to a system where particle trajectories are linear and therefore straightforward to calculate. The forward model does this by considering the ice flux due to vertical compression and horizontal ice flow. The main change from the numerical scheme presented in Parrenin et al. (2025), is that here, the forward model has no basal melting. The basal state of the ice sheet (including basal melting) is instead accounted for by the inverse model. The inverse model works by finding the best-fit value of physical ice flow parameters in the forward model, which lead to the current state of the ice sheet ie. the shape of the radar isochrones. The inverse approach is similar to that of a 1D ice flow model presented by Chung et al. (2023b). We use three inferred parameters—the steady-state (i.e., time-independent) accumulation rate ¯a, the Lliboutry velocity profile parameter p and the mechanical ice thickness Hm. The inferred mechanical ice thickness is used to determine either a basal melt rate or the thickness of a stagnant ice layer as in Chung et al. (2023b).

The observation based constraints required for our 2.5D model are the flow tube width (forward model) and radar isochrones (inverse model). The width of the flow tube was determined using geodetic surface velocity measurements (Sect. 2.3). The inverse model is constrained by isochrones along a radar transect which approximately follows the flow line (Sect. 2.4). The forward model is run for different values of the three inferred parameters, resulting in modelled ages for observed isochrones. The inverse model optimises a cost function by selecting the best-fit parameters, which minimise the age misfit of the isochrones generated by the forward model to the observed isochrones.

L80: what is the form of "horizontal flux shape function"?
The horizontal shape function has the form of Eq. 38 of Parrenin and Hindmarsh (2007) and Eq. 1 of this article.

L97: no definitions for p and Hw
These are defined in the text at:
Line 82: *Lliboutry velocity profile parameter p and the mechanical ice thickness Hm*
Line 103: *The horizontal flux shape function is defined by the Lliboutry vertical velocity profile (ω, Lliboutry, 1979), which depends on the p parameter, (Eq. 1)*
Line 109: *changing the value of p, making the vertical velocity profile more or less linear*
Line 116: *Lliboutry thinning parameter, p (Eq. 1); and mechanical ice thickness, Hm. The mechanical ice thickness (Hm, first defined in Chung et al., 2023b) is the best fit ice sheet thickness when considering the shape of the isochrones given no basal melt rate (Fig. 1 of Chung et al., 2023b). The difference between the mechanical ice thickness, Hm and the observed ice thickness Hobs is used to determine either a basal melt rate m or the thickness of a stagnant ice layer, as in Chung et al. (2023b).*

L110: What is delta q and delta x?
Line 122: *the value of the ice flux Δq over the horizontal distance interval Δx*

Table 1: what is the spatial locations for these 19 IRHs?
Added to Table 1 caption:
These IRHs are traced in the radar transect shown by the red line in Fig. 2

These kind of major and minor issues make me feel very difficult to read and understand this manuscript. Thus, I suggest the authors take another careful round of modifications and re-submit the manuscript after they add the necessary inputs.

References:

Ralf Greve, Yongqi Wang, and Bernd Mügge. Comparison of numerical schemes for the solution of the advective age equation in ice sheets. Annals of Glaciology, 35:487–494, 2002.

Oleg Rybak and Philippe Huybrechts. A comparison of eulerian and lagrangian methods for dating in numerical ice-sheet models. Annals of Glaciology, 37:150–158, 2003.

References

Chung, A., Parrenin, F., Steinhage, D., Mulvaney, R., Martín, C., Cavitte, M. G. P., Lilien, D. A., Helm, V., Taylor, D., Gogineni, P., Ritz, C., Frezzotti, M., O'Neill, C., Miller, H., Dahl-Jensen, D., & Eisen, O. (2023). Stagnant ice and age modelling in the Dome C region, Antarctica. *The Cryosphere,* 17(8), 3461–3483. https://doi.org/10.5194/tc-17-3461-2023

Parrenin, F., Chung, A., and Martín, C.: age_flow_line-1.0: a fast and accurate numerical age model for a pseudo-steady flow tube of an ice sheet, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-3411, 2025.

Parrenin, F., & Hindmarsh, R. (2007). Influence of a non-uniform velocity field on isochrone geometry along a steady flowline of an ice sheet. *Journal of Glaciology,* 53(183), 612–622. https://doi.org/10.3189/002214307784409298

Response to review 2

We thank the reviewer for their comments and provide a response to their suggestions below.

Chung et al. use a 2.5D flowband model and dated internal layers to estimate the age of basal ice along a flowline from Dome C to the Beyond EPICA drill site of Little Dome C. The model finds the best fit spatial pattern of accumulation rate, velocity shape function, and mechanical ice thickness, which can then be translated to either stagnant ice thickness or basal melt rate. The primary result is that the age of basal ice (for Little Dome C this is just above the layer of stagnant ice) is younger than previously suggested, which has implications for the Beyond EPICA ice core.

The modeling is novel and well described. The manuscript is clearly written with a clear and well supported conclusion. There is acknowledgement of the unknown basal process which adds to the excitement of what Beyond EPICA will find when the drilling is completed this season (assuming good fortune). The discussion of ice cores having younger ages than the model predicts is interesting and useful component. The paper is ready for publication, but I hope the authors will consider the including the points below.

The one area that I suggest more of is a discussion of the age results with Lilien et al., 2021. While Lilien et al., 2021 is mentioned in multiple places, it is not clear how this age scale differs. Lilien et al. suggested that a 1.5Ma age, rather than a 1.1Ma age, is likely to be reached, but I think this is not so much a difference in the depth-age relationship as in the definition of "interpretable ice", with this paper using a value of 20 ka/m while Lilien et al. find 14 ka/m. It is a bit difficult for people outside of Beyond EPICA to keep the differences straight, so providing discussions and summaries of the differences more clearly is quite helpful.

We agree that the discussion between past age scales and the one presented here need to be made clearer. In fact we compare not only to the work in Lilien et al. 2021 but also to that of Chung et al. 2023 which both use a 1D model rather than the 2.5D model presented in this work.
We have therefore added some clarifications (line 334-342):

The maximum modelled age of measurable ice at Beyond EPICA from the 2.5D model is 1.12 Ma at an age density of 20 kyr m−1. This is significantly lower than previous estimates using 1D models (Fischer et al., 2013; Parrenin et al., 2017; Lilien et al., 2021; Chung et al., 2023b) and is a direct result of the consideration of horizontal flow in this study, combined with basal melt along the path that ice follows to reach the BELDC site. As ice flowing from the direction of DC encounters the mountainous bedrock relief at LDC, the ice sheet thickness decreases, effectively squeezing the layers and increasing thinning. This increases age density. Therefore the threshold of 20 kyr m−1 is reached when the ice is younger than the 1D modelling in Chung et al. 2023a. Lilien et al. 2021 applied a similar 1D model at BELDC. However, they used the threshold of 60 m above the mechanical ice depth to define the maxiumum age, as this was the basal layer thickness at EDC. Moreover, given the unknown nature of the stagnant ice layer, using this criterion at BELDC may not be appropriate.

A few other minor comments:

- The discussion of ice fabric is appreciated and not including ice fabric in your model is understandable. However, it seems like the effects of fabric could be parameterized through constraints on the shape of the velocity, i.e. the p value. This is obviously future work, but I think considerable progress could be made without trying to model fabric evolution.

As the value of p is optimised by fitting to the isochrone observations, there is only a single value per vertical profile. Therefore, in this type of model we cannot separate the effects of ice fabric from other factors which change the value of p.

- The last sentence of the abstract is frustrating. Why end on such a sour note? Can you finish instead with a concluding note about the progress you have made?

We wanted to be clear about the limitations of the model but we have rearranged the abstract to end more positively.
Line 5: *We present a 2.5D inverse model that determines the age–depth profile along a flow line from Dome C (DC) to LDC that is assumed to be stable in time. This means that flow line features such as flow direction and dome location have not changed over the time period considered.*

Line 20: *Given that the age estimate from the 2.5D model is younger than previous estimates, this work shows the importance of considering the representation of the effects of horizontal flow when modelling the age profile.*