Monday 2 June 2025

Response to review 3 (Report #1)

Reviewer comments
Author response
Text added to manuscript
Line numbers in manuscript submitted with this report

This paper presents a 2.5D inverse ice flow model to reconstruct age–depth profiles along the Dome C (DC) to Little Dome C (LDC) flow line in Antarctica, addressing limitations of conventional 1D models by incorporating horizontal ice flow dynamics. The model integrates radar-derived internal reflecting horizons (IRHs) and surface velocity data to constrain ice particle trajectories and basal conditions. Key findings include the identification of a 200–250 m thick basal layer at LDC, likely composed of stagnant, accreted, or disturbed ice, and an estimated maximum ice age of 1.12 million years (Ma) with an age resolution of 20 kyr m^{-1}. The model highlights the significance of horizontal advection, even in low-flow regions, and provides critical insights for the Beyond EPICA project's ice core drilling efforts.The 2.5D approach represents a significant improvement over traditional 1D models by accounting for lateral ice flow divergence and vertical strain，Although it's hard to say that this model is new. This enables more accurate age–depth predictions, particularly in regions with subtle horizontal dynamics.The identification of a thick basal layer at LDC offers a plausible explanation for ice sheet behavior, including potential stagnant ice or basal melt. This aligns with previous studies while introducing new hypotheses about ice accretion or debris entrainment.The framework is adaptable to other Antarctic regions with sufficient radar and velocity data, enhancing its utility for broader ice core projects. Thus, I believe that this paper is worthy of publication after addressing some of the concerns below.

We thank the reviewers for their time and appreciate their valuable comments and suggestions for improvement. We have replied to each comment below.

Major concerns

The model assumes steady-state flow conditions and static flow line geometry over 1.5 Ma, which may oversimplify glacial dynamics. Paleoclimatic shifts or ice sheet reorganization could alter flow patterns, introducing age uncertainties.Perhaps, a three-dimensional coupled thermomechanical ice flow model (such as Elmer/Ice) could more realistically simulate the ice flow conditions in the flux tube area.

As requested by reviewer 4, we have added further description on how potentially oversimplified glacial dynamics would affect the results.

New text (Lines 359-375)
Whether the 1D or 2.5D model value is more reliable is open to debate, as both approaches have their advantages and shortcomings. On one hand, the 2.5D model incorporates more physical processes. However, the steady state assumption of constant flow direction or a stationary dome, used in this study, may not be suitable in this area. For example, if the location of the dome was mostly around LDC during the past, this might mean that the 1D model is more appropriate for inferring the BELDC age scale. This is because if the location of the dome and by extension the direction of flow from DC to LDC has reversed once or multiple times in the past, particle trajectories would be much more complex than the simple DC to LDC assumed by the 2.5D model. Therefore, the 1D model may average the effects of a reversal in horizontal flow direction, resulting in an age for the ice closer to reality than with the 2.5D

assumptions, despite the simpler model. Preliminary analysis of the BELDC ice core drilled to bedrock in the 24/25 season suggests the ice to be at least 1.2 Ma (Rannard, 2025), already older than the 2.5D model predicts. This could indicate that the assumptions of the 2.5D are not appropriate for the glaciological conditions in this area. If it is indeed shown that the 1D model provides a better age estimate at LDC on further analysis of the BELDC ice core, this would support the hypothesis that the dome has migrated during the period represented by the ice core. Though quantifying dome migration and flow direction changes would require other methods such as more complex 3D modelling or perhaps could be seen using ApRES measurements. Such a dome migration could be important to consider for future ice core site selection around DC. It would mean that very local glaciological features, e.g. the LDC mountainous bedrock relief, are likely to have a more significant effect on the age and continuity of paleoclimatic records preserved in the ice, than the conditions upstream as hypothesised in this study.

Moreover, Radar IRHs were mapped to the flow line with deviations up to 1.9 km, potentially introducing local artifacts.

We have discussed this in lines 331-337, detailing as much as possible why the radar line deviates from the flow line. It is due to restrictions during the field work therefore this is the best data currently available. Improving the data would require a resurvey of the area, something that could be considered in future field seasons but will be logistically challenging

Shallow isochrones (<1000 m) were excluded due to radar resolution trade-offs, limiting constraints on accumulation rates.

Thank you for bringing up this point, we have added further discussion on this.
New text (Lines 261-264)
The uncertainty shown in Fig. 6a is propagated from the uncertainty in radar isochrone depth. However, for accumulation, it is also important to note that as there are no isochrone constraints shallower than 1000 m (see Sect. 4.2). Accumulation uncertainty is larger due to lack of shallow isochrones, therefore we recommend caution with further interpretation of the inferred accumulation rate (Fig. 6a).

Surface velocity uncertainties persist due to low ice flow rates (~mm/yr), comparable to tectonic movements, necessitating some detailed discussion.

The method for determining surface velocities takes into account the effect of tectonic movement as discussed in Lines 153-160. A more detailed description of this can be found in Vittuari et al. 2025 who used the same method for the data surrounding Concordia station.
Quantifying uncertainties in the LDC surface velocity measurements is currently difficult as there are only a few field season's of measurements. Repeating these measurements would allow us to better quantify uncertainties (suggested Line 328) but for now we must use the data we have.

Minor Concerns:

In Section 2. Methods, it would be preferable to provide formulas or definitions related to the steady accumulation rate, Lliboutry velocity profile parameter p, and mechanical ice thickness Hm. Otherwise, readers may find it challenging to comprehend the relationships between these parameters.

We have now defined steady accumulation

New text (Lines 106-109)

The model performs a transformation from accumulation $\boldsymbol{a}$ *to steady accumulation* $\bar{\boldsymbol{a}}$ *using a multiplicative temporal variation factor (Parrenin et al., 2017; Chung et al., 2023b) according to* $\bar{\boldsymbol{a}} = \boldsymbol{ar(t)}$. $\boldsymbol{r(t)}$ *is the ratio of the accumulation at time* $\boldsymbol{t}$ *inferred from the AICC2023 chronology (Bouchet et al., 2023) for the EPICA Dome C ice core (EDC, EPICA members, 2004) to it's temporally averaged value over the last 800 ka.*

p is defined by the Lliboutry profile (Eq. 1, Line 112)

We feel that the definition of H_m (Lines 123-131) is substantive and would not add more as we reference Chung et al. 2023 where there is a similar description as well as a figure and a more detailed discussion.

Line 85: The explanation of how variations in parameter p affect simulation results appears somewhat vague. It would be beneficial to either present a parameterized sensitivity analysis or cite relevant references to support this discussions

We now cite Parrenin et al. 2017 and Lliboutry 1979 who discuss the effect of change p in more detail. (Line 116)

Section 4.1 "Model limitations" (starting at line 220) should consider addressing the influence of bedrock uplift or subsidence caused by tectonic movements on ice thickness evolution. Assuming the basal ice indeed has an age of 1.2 million years, over such prolonged timescales, the bedrock elevation would typically have undergone complex vertical changes. These tectonic adjustments would likely exert non-negligible impacts on ice dynamics and thinning processes.

Changes due to tectonic movements would not have an effect on the modelled flow, as this is a small area far from the ocean. The whole region would have undergone the same changes and therefore the flow pattern would not be affected.

It is the variations in ice thickness that are important as they can have an impact on the thinning function (Parrenin et al., 2007). The estimated max variations of ice thickness are ~150m, ~5% of current ice thickness (Parrenin et al. 2007, Ritz et al. 2001), so this is small, but future studies could aim to incorporate this physical process.

Other minor comments:

Line 8: While the study claims this is a new model, it appears to combine elements from Waddington et al. (2007) and F. Parrenin (2007; or 2011;2017). To substantiate its novelty, please provide specific technical details differentiating this model from previous methods, particularly in terms of equations, boundary conditions, or validation methods.

This model does use elements of previous publications. The new aspect is that it is an inverse model so optimisation of certain parameters can be performed. This is explained in the methods section in more detail. However we have added a sentence to the introduction to clarify this.

New text (Line 10)

This 2.5D model uses a previously developed numerical scheme with the novelty being the inverse methods used to optimise multiple parameters by comparison to radar constraints.

Line 12: The statement regarding "age density of 20 kyr m−1" requires clarification. Please specify whether this metric applies to the entire ice column or specifically to basal ice layers. Additionally, provide a brief technical definition of "age density" for readers unfamiliar with this particular terminology.

We have now reworded this to clarify the 20kyr/m threshold and removed the term "age density" from the abstract.

New text (Line 14)
The threshold for ice useful for paleoclimatic reconstruction is 20 kyr m^{-1} (20,000 annual layers per metre in the ice column). The oldest ice that meets this age resolution requirement is 1.12 Ma at BELDC according to the model.

It is defined on first use in the main text and we have added more details on the 20k kyr/m threshold. (Lines 28-31)
This is especially important for the deepest ice at the Beyond EPICA drill site (BELDC), as the age density (number of years per depth unit) is likely to be very high (Chung et al., 2023b), making extracting a paleoclimatic signal challenging. It is generally agreed that with current understanding and experimental techniques, ice with an age density of < 20 kyr m^{-1} would be useful for paleoclimatic reconstruction (Fischer et al., 2013; Chung et al., 2023b).

Line 14: The interpretation of ice composition ("stagnant ice, accreted ice or disturbed ice containing debris") appears speculative. We recommend either (a) presenting quantitative evidence from particle trajectory modeling to support these hypotheses, or (b) tempering the language to more explicitly acknowledge the interpretive nature of these conclusions.

This is our interpretation of the modelled results so we have edited the text to reflect this. However, until the ice core is analysed, all interpretations will remain speculative.
New text (Line 16)
Looking at modelled ice particle trajectories, interpretations include that this layer could be composed of stagnant ice, disturbed ice, or even accreted ice, possibly containing debris.

Lines 33-40: The basal unit description contains ambiguous terminology.
Whether the radar signature describes a stagnant ice layer with particular englacial characteristics? Or, the interpretation refers specifically to melt-refreeze processes as described by Bell et al. (2011),in which those radar imaging differ from/directly support previous interpretations of basal ice.

Regarding the terminology "basal unit", we follow earlier usage in the scientific literature, e.g. Goldberg et al. (2020) at a different place or Lilien et al. (2021) for our location. The term "basal unit" is generic and can only be interpreted in the context. First, for imaging with radar, it refers to the lowermost zone in a radargram above the bed reflection where radar imaging shows fragmented IRHs or incoherent backscatter or no return power at all. The implication being that this is a property of the ice and not the radar, which would be capable of imaging complete IRHs if there were any at this depth. Second, regarding modelling, as employed in our study or earlier (e.g. Lilien et al., 2021) the lowermost layer of the ice sheet can be considered stagnant, or at least not very actively contributing to ice flow according to the model assumptions. Third, preliminary results from the newly retrieved ice core at LDC indicate that the lowermost layer has different properties than the ice above, notably in dielectric values as well as ice water isotopes (pers. communication Beyond EPICA consortium).

All three observations are coincident in terms of the depth range. We therefore attribute these properties to the same lowermost layer in the ice sheet, termed basal unit. We rephrased the explanation of the basal unit in the text accordingly to avoid confusion.
The questions whether the ice of the basal unit is of meteoric origin but of larger age than the stratified ice above or was formed through basal accretion cannot be answered right now. It requires a full processing of the ice core (happening until mid 2026) as well as further drilling for absolute age dating on a replicate sample to be retrieved in the season 2025/26. We hope to be able to provide an answer by 2027.

Reworded text (Lines 42-47)
At LDC, a "basal unit" has been observed in radar surveys presented by Cavitte (2017) and Lilien et al. (2021). This basal unit is a layer directly above the bed that seems to have different ice flow characteristics to the ice above. Chung et al. (2023b) showed that the basal unit seen in radar surveys is of comparable thickness to a stagnant ice layer predicted by a 1D inverse age–depth model. They then corroborated this with vertical velocity measurements made with an autonomous phase-sensitive radio-echo sounder (ApRES) which suggested that the ice layer above the bed is not flowing vertically (hence the name stagnant).

Lines 101-102: It could be missing key references for the EDC ice core chronology. Please add citation.

We have referenced Bouchet et al. 2023 which is the reference for the AICC2023 ice core chronology and since this is the first mention of the EDC ice core we have added (EPICA members, 2004) (Line 108)

Lines 126-133: The ice flow velocity description lacks critical details.

We are not sure what the reviewer is referring to here. This paragraph (Lines 122-139) talks about the optimisation of accumulation a, mechanical ice thickness H_m and the Lliboutry profile parameter p.
A detailed description of how these parameters link to the velocity field is provided in Parrenin et al. (2025).

Figure 1 should ideally include a schematic diagram indicating the specific location of LDC within the East Antarctic Ice Sheet. Unless readers are highly familiar with this region, it would be difficult for general audiences to easily discern its geographical position and spatial relationship relative to deep ice core sites such as Dome C and Vostok.

In Figure 1, we have now added an inset showing the location of the map area in Antarctica. The EPICA Dome C (EDC) ice core site is marked with a black square.

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Monday 2 June 2025

Response to review 4 (Report #2)

Reviewer comments
Author response
New text in manuscript
Line numbers in manuscript submitted with this report

This manuscript combines a 2.5-D ice flow mode with IPR sounding data to study the basal ice dynamics at LDC, which has important implications for locating old ice and interpreting the paleoclimate record it preserves. This study addresses an important science question and will be of interest to a broad community. The methodology is robust and well described, and the manuscript includes clear and informative visual illustrations. The authors have responded thoroughly to the previous reviewer comments and revised the manuscript accordingly. I have only a few minor comments and suggestions that I believe could further improve the clarity and quality of the manuscript.

We thank the reviewers for their time and appreciate their valuable comments and suggestions for improvement. We have replied to each comment below.

My primary concern relates to the treatment of input data uncertainties and their influence on the model outputs, particularly in regard to the interpretation of critical features such as the inferred basal accretion layer. While the authors provide a good overview of the sources and ranges of uncertainties (e.g., in divergence rates, radar line positioning, IRH depths, etc.), the manuscript would benefit from a more explicit evaluation of how these uncertainties propagate through the inversion process and affect key model outputs. For instance, the identification of an accretion or stagnant ice layer relies on a relatively small difference among Hm, Hobs, and traced particle trajectories. Without an assessment of their sensitivity to uncertainties in IRH positions or surface velocity measurements, it is difficult to fully trust the robustness of this interpretation. Maybe a minor underestimation in IRH depth or flow line misalignment could alter the model results significantly? A formal sensitivity analysis or at least a discussion of plausible error margins and their implications on interpreting basal processes would greatly strengthen the paper and provide readers with a clearer understanding of the reliability of the model's conclusions.

As requested by reviewer 3, we have now expanded the discussion of uncertainties in modelled results. Uncertainties in IRH depth are propagated through the optimisation process and are shown with the optimised parameter values in Fig. 6. We also now discuss the effect of the lack of shallow isochrones on accumulation rate uncertainty.
New text (Lines 261-264)
The uncertainty shown in Fig. 6a is propagated from the uncertainty in radar isochrone depth. However, for accumulation, it is also important to note that as there are no isochrone constraints shallower than 1000 m (see Sect. 4.2). Accumulation uncertainty is larger due to lack of shallow isochrones, therefore we recommend caution with further interpretation of the inferred accumulation rate (Fig. 6a).

As surface velocity measurements at LDC, they only cover a few years of movement, so determining uncertainties from this is difficult. Measurement should be repeated over a longer time to be able to reasonably quantify uncertainties. (Line 327)

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We can appreciate the reviewers suggestion for a sensitivity test and understand its value. Building on this study, we would like to test how results change if lateral flow divergence or non vertical flow tube walls are considered (Lines 303-308). In future work, where there is melting, we would like to use inverse methods to directly infer a melt rate rather indirectly using inferred Hm. This is more physical and would not result in this issue of particle trajectories passing below the observed bedrock depth and therefore perhaps make the flow behaviour of the accretion zone clearer. However, this would require changes to the model and represent a significant amount of work which is outwith the scope of this paper. We hope we will be able to provide these answers in the future.

Another concern relates to the approach used to estimate the accreted ice layer thickness. The authors derive this by comparing the depth of the deepest traced ice particle trajectory with Hm. However, this method seems to assume that subglacial meltwater travels through the bedrock along similar paths as ice particles within the ice sheet, eventually refreezing along those trajectories. This assumption is problematic. The movement of meltwater within or beneath the bedrock is controlled by very different processes than ice flow, such as basal hydraulic potential gradients, bedrock porosity and permeability, and subglacial hydrological routing. It is not physically reasonable to assume that meltwater would follow the same spatial paths as deforming ice, especially over bedrock obstacles or variable basal conditions. I suggest the authors clarify this assumption.

First, we would like to highlight that this is our interpretation of what **could** cause this modelled "accretion zone", it is one possible explanation. As requested by reviewer 3 we have made this clearer in the abstract.
New text (Line 16)
Looking at modelled ice particle trajectories, interpretations include that this layer could be composed of stagnant ice, disturbed ice, or even accreted ice, possibly containing debris.

However, with the accreted zone, we are not suggesting that melt water necessarily follows the ice particle trajectories as we agree the behaviour of liquid water would be very different to that of ice. We are suggesting that ice melts at certain points, then at some point further downstream, water from another source freezes onto the base of the ice sheet. Of course modelling the movement of water would require a different model.

We understand that the confusion may come from how we worded this section, so we have reworded it to make it clearer.
Reworded text (Line 209-211)
Where the mechanical ice thickness from the model passes below the observed ice thickness, ice flow is still calculated. As a result, some ice originates from particle trajectories that begin beneath the observed bedrock. We have named the layer where this occurs "accreted ice" (blue layer in Fig 3).

As discussed above for the previous point, in future work we plan to change how the basal state of the ice sheet is represented in the model to see how that affects the behaviour of the deepest ice.

As mentioned in our review 3 response, there is also evidence from radar observations of a basal layer that behaves differently to the ice above (Lilien et al. 2021 and Chung et al. 2023). Preliminary results from the newly retrieved ice core at LDC indicate that the lowermost layer has different properties than the ice above, notably in dielectric values as well as ice water isotopes (pers. communication Beyond EPICA consortium). The questions whether the ice of the basal unit is of meteoric origin but of larger age than the stratified ice above or was formed through basal accretion cannot be answered right now. It requires a full processing of the ice core (happening until mid 2026) as well as further drilling for absolute age dating on a replicate sample to be retrieved in the season 2025/26. We hope to be able to provide an answer by 2027.

Line 146-152: This paragraph, especially the second half, is hard to understand. The methodological details related to GNSS measurements and pole installation lack sufficient explanation, making it hard for the reader to grasp the technical reasoning. No references are provided to support the statements or the methodological details.

We have now reworded (old) lines 149-152.
Reworded text (Lines 154-158)
Given the small magnitude of the expected ice velocities, a guarantee of high repeatability in centring the geodetic antennas was required during the initial and repeat measurements at each site. This was achieved by using aluminium poles 3m long and 12 cm diameter, installed at a minimum depth of 1 m in the snow, with a forced centring mount for the antennas on the top of the pole, which acted as precise three-dimensional reference points (see Vittuari et al. 2004 and 2025).

Line 246-247: It takes a long time for changes in surface temperature to propagate through the ice column and influence the basal thermal condition and basal melt rate.

It can take a few 10s of kyr for surface temperature to propagate down to the bedrock, so the MPT temperature change (if any) certainly has already propagated.

Line 347-352: The observation that the 1D model appears to yield age estimates closer to preliminary field observations than the more physically comprehensive 2.5D model is intriguing. The authors suggest that this may be due to past migration of the ice divide/dome, but this hypothesis is not elaborated upon. It would significantly strengthen the manuscript if the authors could expand this discussion:

- Why would ice divide migration lead to better performance of a 1D model that assumes no horizontal flow?
- Could this discrepancy be interpreted as indirect evidence supporting past dome movement or reorganization of the ice flow?
- What would be the implications of such migration for site selection or future modeling approaches?

A deeper exploration of these possibilities would provide valuable insight into the glaciological dynamics of the Dome C region and could open a useful discussion about the limits and contextual applicability of 1D versus 2.5D modeling frameworks.

Thank you for these questions, we have now made significant additions to this discussion to answer them.

New text (Lines 359-375)
Whether the 1D or 2.5D model value is more reliable is open to debate, as both approaches have their advantages and shortcomings. On one hand, the 2.5D model incorporates more physical processes. However, the steady state assumption of constant flow direction or a stationary dome, used in this study, may not be suitable in this area. For example, if the location of the dome was mostly around LDC during the past, this might mean that the 1D model is more appropriate for inferring the BELDC age scale. This is because if the location of the dome and by extension the direction of flow from DC to LDC has reversed once or multiple times in the past, particle trajectories would be much more complex than the simple DC to LDC assumed by the 2.5D model. Therefore, the 1D model may average the effects of a reversal in horizontal flow direction, resulting in an age for the ice closer to reality than with the 2.5D

assumptions, despite the simpler model. Preliminary analysis of the BELDC ice core drilled to bedrock in the 24/25 season suggests the ice to be at least 1.2 Ma (Rannard, 2025), already older than the 2.5D model predicts. This could indicate that the assumptions of the 2.5D are not appropriate for the glaciological conditions in this area. If it is indeed shown that the 1D model provides a better age estimate at LDC on further analysis of the BELDC ice core, this would support the hypothesis that the dome has migrated during the period represented by the ice core. Though quantifying dome migration and flow direction changes would require other methods such as more complex 3D modelling or perhaps could be seen using ApRES measurements. Such a dome migration could be important to consider for future ice core site selection around DC. It would mean that very local glaciological features, e.g. the LDC mountainous bedrock relief, are likely to have a more significant effect on the age and continuity of paleoclimatic records preserved in the ice, than the conditions upstream as hypothesised in this study.

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