Review of “age_flow_line-1.0: a fast and accurate numerical age model for a pseudo-steady flow tube of an ice sheet” by Parrenin et al. 2025

General Comments

This paper presents a 2.5D Eulerian-Lagrangian age model for ice-sheet stratigraphy, assuming steady-state geometry along a flow tube. Using the (π,ϕ) coordinate system introduced by Parrenin et al. (2006), the model efficiently and accurately solves the transport equations. The manuscript is well within the scope of *Geoscientific Model Development (GMD)*, and the figures are clear and helpful. Overall, I recommend this paper for publication.

That said, the manuscript could more clearly articulate the novelty of this work. The introduction would benefit from a broader contextualization of ice-sheet stratigraphy and dating methods, clarifying when modeling is necessary compared to alternative approaches such as layer counting or tephra dating. When discussing existing models, it would be helpful to distinguish between different objectives, such as dating deep ice-core layers, determining ice origin for upstream corrections, modeling the age distribution across an ice sheet, or using isochrones to invert for basal parameters. A brief discussion of key challenges in ice-age modeling, such as numerical diffusion, would also be valuable. Strengthening the motivation of why a new age model is necessary and how this model fills a gap in the existing literature would further improve the framing of the study.

The application of the model to simulate the age distribution between Dome C and Little Dome C is a helpful illustration, but the purpose of this demonstration could be clearer. The paper initially suggests that the goal is to investigate the role of horizontal advection in shaping the depth-age relationship at Little Dome C, yet the model parametrization is later described as unrealistic. This raises questions about the extent to which conclusions drawn from this ‘unrealistic’ model run are meaningful. It would be helpful to disentangle these two aspects: Is the aim to assess the significance of horizontal advection, or simply to demonstrate the model’s capabilities? Clarifying these objectives, as well as the connection to Chung et al. (2024), would strengthen the narrative.

The discussion is thorough but could better address model validation, limitations, and potential applications. The role of horizontal flow is highlighted, yet discrepancies between modeled and observed isochrones are only briefly mentioned. Expanding on possible sources of error and how the model compares to other 2.5D approaches (e.g., Buchardt et al. 2007) would be beneficial. Additionally, while the authors acknowledge simplifications in accumulation and basal melt assumptions, a more critical discussion of potential biases would strengthen the manuscript.

The code is well-structured and documented, and I was able to run the DC-BELDC example without issues. Expanding the GitHub README with guidance on adapting the code to other regions would be beneficial. While Section 2.4 provides useful information for users, its placement disrupts the readability of the manuscript. Consider moving it to the GitHub repository for better accessibility.

Specific Comments

Line 12: Specify the coordinate system more clearly—replace "innovative" with "logarithmic flux coordinate system."

Lines 21-24: The sentence is too long; consider breaking it down or simplifying.

Lines 26-28: This sentence is hard to follow. Consider restructuring for clarity.

Line 29: Add a sentence elaborating on why this is important or provide an example of how upstream effects impact the paleo-record.

Line 30: Clarify that modeling is particularly important where annual layer counting is not possible.

Lines 32-35: This sentence is difficult to understand. Aren't you constraining the *boundary conditions of ice sheet models* and understand *internal processes of the ice sheet*? It would also be good to specify what is being inverted for and give an example with citation

Line 36: "types". Implemented into what?

Line 37: Specify what they are used for.

Line 37: Born & Robinson (2021) could also be relevant here.

Lines 57-62: Explain the motivation for developing a new age model—what improvement does it offer over existing models?

Line 58: Wasn't this already introduced in Parrenin et al. (2006)?

Line 65: Consider specifying "steady-state flow-tube." Indicate that the flow line starts at an ice-sheet dome and ends at the margin (since x is later defined as "distance from the dome").

Line 70: Specify whether z is defined as positive (height above bed/sea level) or negative (depth below surface).

Line 84: "Passing below depth z"—note that depth was previously defined as "d", but I'm not sure that 'd' is actually used. Ensure consistency.

Line 113: How do you justify that basal melt rate and surface accumulation have the same temporal variation? There is no direct response of basal melt rate to accumulation changes.

Line 115: Change to "temporal average."

Line 122: For a general model description, reword as: "using a relative density profile informed by ice core observations or firn models in the simulated area."

Line 132-133: 'a point slightly downstream of the dome' could be more specific. How far downstream?

Line 138: "linear-by-parts function."

Line 141: Delete "that" after "1/a."

Lines 158-166 & 174-178: Consider removing these paragraphs—it sounds more suited for a "README" in the GitHub repository rather than the paper.

Line 179: Clarify—do you mean "flow tube"?

Lines 180-181: Provide more context about Beyond Epica for readers unfamiliar with the project. For example that modeling here is necessary for ice core dating due to small layer thicknesses. It would also be beneficial to already here explain the motivation for this modeling effort.

Lines 185-186: Clarify why you use "mechanical ice thickness" for the bottom boundary conditions. Do you mean that you are using basal conditions from Chung et al. (2023)? Why not use observed ice thickness from your radar survey?

Lines 186-187: The aim of the simulation should be stated earlier in the paragraph rather than after explaining model parameterization. The purpose is unclear—you state that you investigate whether horizontal advection affects depth-age relationships at LDC but then call the setup unrealistic. Separate these two points and clarify the goal here versus in Chung et al. (2024).

Line 191: Where do these boundary conditions come from?

Line 193: BELDC has not been defined previously.

Line 201, Fig. 8: Indicate where the ice divide is located. This would make the statement “the ice particles may originate >20 km upstream along the divide” clearer.

Line 221: Cite as "1D inverse model by Chung et al. (2023)."

Lines 230-231: Missing citations for this statement —add references.

Line 234: "In front of"—do you mean "compared to"?

Line 235: what do you mean with "right" here? How do you know what's right without direct observations.

Line 254: Sentence is incomplete—revise for clarity.

Figure 6: Explain the cause of discrepancies between observed and modeled bedrock.

Line 345: The citation of this thesis seems out of place. Does most of the content of this paper rely on it? Consider revising.

Figure 1: Clarify whether z points upwards or downwards.

Figure 3: A map overview of where this is located in Antarctica could be useful. Instead of surface elevation, I would consider the surface flow velocities a more relevant context for this work.

Figure 7: Consider a logarithmic colormap

Figure 10: I don’t see a green line.

Technical comments

Line 66: e.g.
Line 195: In Fig. 5.

Line 320, 322, 328, 335: missing DOIs.

Site and Ice Core Naming Consistency: The distinction between Dome C and Little Dome C as sites, and EDC/BELDC as ice core names, may be confusing for readers unfamiliar with the terminology. Consider using a single consistent name per site throughout the text. Right now it is a

mix of DC, EDC, Dome C and LDC, Little Dome C, BELDC, and Beyond EPICA. Alternatively, explicitly define all terms early in the paper.