Reviewer 1

Reviewer Comments – Round 2

Thank you for thoroughly addressing my previous comments. The manuscript has improved in both clarity and structure, and I appreciate the authors' thoughtful and detailed responses to the concerns raised in the first round of review.

Thank you again for your rigorous and insightful review of our manuscript!
This is much appreciated.

My only remaining main suggestion concerns Section 4.5, where the discussion could be further strengthened by more explicitly situating the model within the context of the listed existing approaches. While I understand that a direct comparison with other models is beyond the scope of this paper, it would be helpful if you could elaborate on how you think the new method complements or improves upon previous efforts, and what specific advantages or new insights it may offer.

Thank you for your comment.
Yes, this discussion was missing.
We address this point below.

Aside from this point, I have only a few minor comments, listed below. Once these are addressed, I would be happy to recommend the paper for publication.

Line 34: I think you mean thick enough?

Corrected, thank you.

Line 43: again, this sentence doesn't make much sense to me. I think what you mean is boundary conditions of ice sheet models or understand the internal processes of ice sheets models using inverse methods ?

Corrected, thank you.

Line 61: a steady-state model

Changed 'steady' to 'steady-state' throughout the manuscript.

Line 95: see my previous comment. Depth is still defined as 'd' but never used. Change to elevation 'z'.

Definition of 'd' removed.

Line 131: How do you actually determine R(t)? please elaborate.

We added the following sentence:

$R(t)$ is a positive temporal multiplicative factor which can be determined from ice core data, e.g. using the relationship between the isotopic composition of ice and the surface accumulation rate.

Line 132: Suggested edit for better clarity and text flow: This assumption is discussed in section 4.3. → The implications of using the same temporal variation for basal melt rate and accumulation rate are discussed in Section 4.3.

Suggestion adopted, thank you.

Equation (15): replace full stop with comma

Done, thank you.

Equation (16): missing full stop after equation

Done, thank you.

Section 3: This is much improved and a lot clearer – Thank you.

Thank you!

Line 223: approaches

Corrected, thank you.

Line 248: it would be nice to get some numbers here – e.g. how many meters deviation or depth percentage.

We added:

*...the isochrones simulated by the 2.5D model significantly deviate from them (Figure 7)**, with deviations of >100 m at some places***.

Line 251-252: remove this sentence (repetition of above). There are also no white isochrones in Fig. 7

Sentence removed.

Line 264-265: “But because basal melting is generally small compared to surface accumulation, this assumption should not be too dramatic” - I’d be careful with this sentence. Basal melt may be small but has quite a large impact on isochrone deformation. I suggest rephrasing to something like:

However, in regions where basal melt rates are low, this assumption should not introduce major errors, as long as a realistic time-averaged basal melt rate is prescribed.

Thanks for the suggestion, we adopted it.

Line 265: I think ‘a realistic’ would be better suited instead of real

This is now changed according to your suggestion above.

Section 4.5: Thank you for adding this section. It's helpful to see a discussion of past modeling efforts. One element which I feel is still missing, however, is a clearer explanation of how your model might compare to, or improve upon, these previous studies.While it’s fine to outline possible future applications, I think this section would be much more useful if you could briefly articulate where your model offers particular advantages.

From my understanding, one of the strengths of your model lies in producing a high-resolution depth–age distribution and efficient computation, which could be especially valuable for certain applications (long flow lines, old age, inversion..?). However, I am less clear on how well-suited it is for estimating basal conditions in regions with high basal melt, especially since basal melt rate is coupled to accumulation rate in your formulation.

For instance, models by Buchardt & Dahl-Jensen (2007) or Gerber et al. (2021) assume a steady-state basal melt rate and use time-varying accumulation driven by $\delta^{18}O$ climate curves, with some tuning, to estimate basal melt rates. How would you envision your model performing for basal melt estimates in comparison, especially regarding regions where basal melt is considered to be high?

Whatever your interpretation, a few sentences offering your outlook on how this model can advance research — and where its strengths (and potential limitations) lie compared to the available literature — would be a valuable addition to the discussion.

This is what we added in section 4.5:

The value of this model lies in its high numerical accuracy and very fast computation time. It is appropriate for long flow lines, old age and for parameter inferences. The companion paper of Chung et al. (2024) outlines its interest, since we can optimize its parameters in a couple of minutes on a personal computer. However, this model has physical assumptions which might introduce errors. To overcome this limitation, our model could be used in conjunction with a transient model like the one developed by Buchardt & Dahl-Jensen (2007) or Gerber et al. (2021). For example, our model could provide an initial condition which could be refined using the transient model. It could also provide a 'first guess' and a fast approximation of the Jacobian of the transient model, to speed up the optimization of its parameters.

Reviewer 2

We warmly thank the reviewer for his careful reading of our manuscript.
We took the comments into account and reckon they helped improve the readability of our manuscript.

Answer to specific comments:

L. 30: I cannot read the last word on this sentence.

L. 61: replaced 'steady' with 'steady-state'

L. 63: 'a class to which the age equation belongs'

L. 62: suggestion adopted.

L. 72: The age and spatial origin equations belongs to the transport equations. We replaced 'transport' by 'spatial origin' to be more specific.

L. 83: removed 'will' as suggested, thank you.

L. 88: Yes, the definition of the flow tube width is relative.

L. 93: Added 'local' as suggested, thank you.

L. 95: Definition of ‘d’ has now been removed after Rev. 1 remark.

L. 95: Relative density means it is expressed in % of volume, not mass per volume. We added this precision.

L. 102: Changed equation to: $Y^B(x') = Y(x', B(x'))$

L. 110: Sentence modified.

L. 114: Parentheses removed as suggested, thank you.

L. 116: No, accumulation cannot be zero.

L. 121: Replaced by ‘equal in magnitude by opposite in sign’.

L. 140: It is the section of the tube which is rectangular.

L. 144: Replaced by: ‘is very useful to solve any transport equation, a class to which the age equation belongs’

L. 178: Suggestion adopted, thank you.

L. 204: Suggestion adopted, thank you.

L. 205: Suggestion adopted, thank you.

L. 221: Changed to ‘ice-bedrock interface’.

L. 229: ‘originates 22 km upstream from the drilling site”.

L. 232: Suggestion adopted, thank you.

L. 244: Correction applied, thank you.

L. 263: Comma replaced by semicolon, thank you.

Fig. 1, legend: Correction applied, thank you.