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.

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.**

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?
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 ?

Line 61: a steady-**state** model
Line 95: see my previous comment. Depth is still defined as 'd' but never used. Change to elevation 'z'.
Line 131: How do you actually determine R(t)? please elaborate.
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.

Equation (15): replace full stop with comma
Equation (16): missing full stop after equation

Section 3: This is much improved and a lot clearer – Thank you.
Line 223: approach**e**s
Line 248: it would be nice to get some numbers here – e.g. how many meters deviation or depth percentage.
Line 251-252: remove this sentence (repetition of above). There are also no white isochrones in Fig. 7
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.

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

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.