Review of: Modeling the Greenland englacial stratigraphy
Submitted to: The Cryosphere Discussions
Reviewer: Nicholas Holschuh

General Comments:
In previous work, Born derived and demonstrated a model of ice flow focused on the evolution of layer packages (Born, 2017). An explicit output from this model is the isochronal layer geometry – a field that is directly comparable with radar observations, which have the potential to provide a spatially and temporally comprehensive check on model performance. The formulation presented in (Born, 2017) outperformed existing models which use Eulerian velocity fields to contour the age field of the ice sheet, avoiding issues of numerical diffusion that can result in unrealistically smooth age fields. That work forms the backbone of this manuscript, which is focused on making that framework modular, such that it can be applied to existing 3D models of ice flow and allow for the use of observed englacial layers in model tuning.

At its core, this is a methodological paper, pursuing an important objective at the cutting edge of ice sheet modeling. But the authors spend most of the paper discussing the specifics of their model *results* – what drives model-layer / observed-layer mismatch and the interaction between specific tuning parameters in Yelmo (the underlying ice physics engine (Robinson et al., 2020)). This would be important if the tuned model (i.e. the depth-age model of Greenland since LGM) were the central product of this work, but the real scientific contribution here is what the authors have learned about the *process* – that (1) it is possible to apply the layer tracking scheme in (Born, 2017) to 3D models that do not explicitly track layers, that (2) the resulting layers are an improvement over results of previous methods, that (3) tuning ice flow models (or at least, this ice flow model) to the ice thickness alone can result in large errors in englacial dynamics, and (4) that it is *important* that future models use this method, as Eulerian tracers produce a systematic bias in model-layer age.

While I have only minor questions about the technical work done, the changes that I think are most necessary are to the writing, to maximize the paper's impact and ensure that the scientific contribution of the work is clear. Right now, the key messages are buried in extensive description of Greenland accumulation, and the large, multi-panel figures of model mismatch do little to articulate this work's core message. In the technical comments below, I provide specific changes that I think will help resolve these issues. Ultimately, the layer tracking module developed here has the potential to be a widely used tool and help constrain models across a wide range of complexities, and I want to ensure this work has the impact it deserves.

Technical Comments:

If this were simply another model of Greenland from LGM to today, the scientific contribution would be limited, as there is no articulated "experiment" here probing Greenland dynamics. The discussion of errors in model forcing provides insight into the climate parameterizations chosen, but they distract from the methodological improvements that will be this paper's legacy. To make clear the scientific contribution, I think three primary changes are required:

1. There should be a reproducible description of how the layer tracing scheme couples to the 3D model. There is extensive description of the climate spin up, and the model parameters being tuned, but no description of the implementation that translates output from (Robinson et al., 2020) to input in (Born 2017). This should (1) make clear to the reader exactly how this method avoids the pitfalls of Eulerian tracers, especially while using an ice sheet model that solves the physical equations on an Eulerian depth grid, and (2) enable future application of the method to other ice sheet models.

2. A more succinct description of the optimal model should be provided, but primarily to highlight which model parameters are sensitive to the stratigraphic constraint (indicating which processes / boundary forcings this optimization approach is likely to capture). The extensive description of figures 4, 5, 10, and 11 can be substantially trimmed. In addition, I think the readers would benefit from a deeper explanation of the differences observed and the drivers of that difference in figure 12, which demonstrates the value of the improved parameter optimization.

3. At present, this paper avoids the discussion of an important and active area of research: fitting layer shapes in the dynamic regions of Greenland and Antarctica. This is in-part because the outlet glacier modeling done here is simplified. But layer fitting in these areas has the potential to capture spatial and temporal heterogeneity in the basal boundary condition that no other method can address, and given the high-profile nature of features like the layer draw-down in Northeast Greenland (e.g., Fahnestock et al., 2001) and the complex folding at Petermann Glacier (e.g., Bons et al., 2016), I think it would be appropriate for this work (especially given its title: "Modeling the Greenland englacial stratigraphy") to directly address dynamically controlled folding which dominates the marginal ice. There is an extensive literature on the Weertman Effect (e.g., Hindmarsh et al., 2006; Leysinger Vieli et al., 2007; Parrenin et al, 2007; Wolovick et al., 2016), models of layer shape in Greenland (Leysinger Vieli et al., 2018), and direct comparison of models and data (e.g., Holschuh et al., 2019) that could be used to substantiate the need and interest in developing these layer modeling methods. While you could never provide a complete review of the literature here, the repeated claim that accumulation history is the dominant variable relies on an implicit assumption that we ignore the dynamic outlet glaciers. A full description of your method should include its applicability to the ice sheet margins and how it fits within the existing literature on the subject.

These comments are motivated by the fact that I would like to see this method applied more broadly! A revision focused on clarity of the method, quantification of its improvement over previous methods, and guidelines for its future use will make the scientific merits and novelty clear.

Line-Item Corrections:

Page #: 1 Line #: 6-8	I find this description confusing, as mass transfer happens within Yelmo (outside of the layer evolution scheme). How is it that mass transfer between layers is avoided when the solver exists in depth, not time? (I think this is just part of a larger desire to see a clear description of the coupling).
Page #: 1 Line #: 10	The phrasing "… selecting simulations…" is unclear here -- selecting them for what? Perhaps rephrase to "Using an ensemble of simulations to optimize climate and ice dynamic parameter selection, we show that direct comparison with the dated radiostratigraphy data yields notably more accurate results than choosing parameters based on fit to total ice thickness alone."
Page #: 1 Line #: 16-22	I appreciate the oceanographic analogy here, it adds nice context!
Page #: 1 Line #: 21	"The proverbially glacial flow" -- I'm not sure what you mean here by "proverbially".
Page #: 2 Line #: 28-31	There is an error in construction here, with the sentence that reads: "The ... layers could aid…, where to find…, to reconstruct…, or to determine…." Either each clause should start with an infinitive, or they should follow from a common verb. It could be "The layers could aid, find, reconstruct, or determine", or it could be "The layers could help us to select, to find, to reconstruct, or to determine".
Page #: 2 Line #: 36	Should be "finite-difference" not "finite-differences"
Page #: 3 Line #: 63-64	Given that your layer thicknesses are smaller than the vertical resolution of the solver, I am still a bit confused about how you can solve for changes in layer thickness and still guarantee no numerical diffusion? (This is where a discussion of the coupling would be helpful).
Page #: 0 Line #: Section 3.2	I had difficult following the narrative through this section, especially Figure 10 and 11. If you intend to keep all of this, it would help to guide the reader through it in a more directed way -- referring to specific panels in the figures (not just a grid of 40 Greenlands), and pointing back to the motivating questions that justify the extensive description provided. Ultimately, I did not see any need for detailed description of the specific model output you provided, as there are more sophisticated modeling exercises that one could turn to for full description of dynamics in Greenland. But if you think there is value in dissecting the specifics of this model configuration and output (as opposed to just focusing on the exercise of modeling and optimizing), you need to motivate that more clearly somewhere.

Page #: 19 Line #: 315-329	This section had me thinking about a more general question -- do isochrones add value in constraining processes outside of the time range that they span? Making an explicit statement about how temporal coverage of the data impacts temporal constraint in the model could be very interesting.
Page #: 19 Line #: 345	Space between numbers and units.
Page #: 20 Line #: 350	You regularly state that the Eulerian age tracer (orange curve) in Figure 12 shows older ages, but in all situations it seems that the age of the orange curve falls below the blue curve. Am I misreading Figure 12? "Older" continually appears in your description of the Eulerian method, and I am having trouble rectifying that with the figure.
Page #: 20 Line #: 351-353	A clear description of the coupling will certainly answer this question, but somewhere depth and age must be mapped to one another to couple the ice flow model to the layer evolution model, and I'm still not clear on how the horizontal flow speeds within a given layer are calculated (to prevent flow across boundaries).
Page #: 20 Line #: 357-358	Okay, I think I understand the "older" comment here -- if a model were optimized using the Eulerian scheme, the true model age (when calculated correctly using the new layer evolution scheme) would actually be older than the constraint. But that seems different than the previous statement, that the Eulerian tracer data produces older ages. I am probably just confused, but some clarity through this whole section on the nature of the bias of the Eulerian method would be useful.
Page #: 21 Line #: 378-380	I think this sentence is a bit of a tautology -- the model calibrated to the stratigraphic data fits the stratigraphic data better. It would be better to appeal to a third target variable to evaluate accuracy. Something like: "Models that are optimized to match the ice thickness require unrealistic precipitation histories, resulting in erroneous layer ages. These precipitation histories can be ruled out when constraining model parameters with both thickness and layer age."
Page #: 21 Line #: 381-386	This paragraph is only true because you exclude the dynamic regions of Greenland from your analysis. Of course accumulation matters more when dynamic vertical velocities are otherwise very small. Where the Weertman effect is large, surface mass balance history will be much less important. This is why I advocate for a broader discussion of what affects layer shapes near the margins, contextualized in the literature.

Page #: 23 Line #: 423	Again, this point that "accumulation eclipses the impact of dynamics" is not universally true, and will depend on the target region of interest for future applications of this method. In the outlet glaciers, this is far less likely to be the case, so I would hesitate to use such strong language when this method could be applied beyond just the interior of ice sheets as was done here.

References:

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