13 Nov 2020

re: “Review of Schneider et al.”, Anonymous Referee # 2, 11 Sep 2020

We appreciate your positive review and are considering your comments as we revise the manuscript. Following below is our response (in blue) to your general and specific comments, which are italicized here for reference:

- *This is a very well written manuscript that is motivated and articulated clearly. I am not an expert in ESMs or their various component models, and therefore cannot comment on the authors' specific implementation of firn compaction models into the E3SM Land Model.*
  Thank you for the kind remarks. We appreciate your perspective and are working to reach readers with and without established expertise in Earth system modeling.
- *The stated goal of this work is to more accurately simulate snowpack evolution in the ELM, including over the Greenland and Antarctic ice sheets. With this goal, why do the authors increase the snowpack from 1m (in the previous version) to up to 60m in this version? In the dry snow zone, it's common for firn column to vary in depth from 50 to 120m (Cuffey & Paterson, 2010), depending on site conditions. Many sites have firn columns deeper than 60 m (and even the 80 m depth that Figure 1 indicates is possible), and therefore this estimate of snowpack evolution won't be valid for large swaths of the Greenland and Antarctic ice sheets. If instead the authors are referring to depths in m SWE, this should be made explicit within the text and in the figure axes.*
  The maximum depth of the new snowpack model – the distance from the surface, not in terms of snow water equivalent (SWE) – is a recurrent point of confusion for readers. While the new vertical grid has a bottom most layer without a bound on its maximum thickness, we choose 60 m because that is roughly how far below the surface the 15 layers of finite thickness will extend when they reach their maximum thicknesses (Figure 1). Therefore, the grid is truly semi-infinite, but we choose to exclude the semi-infinite layer from (and thus restrict the vertical extent of) our analysis because the model cannot resolve dynamic variables deeper than 60 m at a vertical scale appropriate for simulating densification. Because the focus of our study is on getting the bulk density structure of the firn correct, our primary domain of interest is where most of the densification takes place, i.e., closer to the surface. By nature, the representation of firn in an Earth system model has to have some approximations and limits in its accuracy and detail. For our purposes, we deem an evaluation of the top 60 m adequate for capturing the vast majority of the firn densification on ice sheets. Much lower than 60 m, densification rates become relatively small and are of second order importance.
  To alleviate confusion, we changed the wording in the caption of Fig. 1 to better reflect our semi-infinite snowpack grid.
- *In various places the authors describe “improvements” or “slight improvements” between models, especially in Seciton 4.1. How do the authors determine these improvements? Are they quantifiable?*
  The qualitative improvements we describe are mostly justified by directly comparing density profiles and estimated strain rates simulated in ELM to the empirical model of Herron and

Langway (1980). We attempt to show these comparisons in Figs. 2, 3 and 4, but due to our presentation of the results and discussion, it can be difficult for readers to evaluate data from all the experiments. It is also clear that claims of improved density profiles need further quantification.

To improve the readability and interpretation of our main findings, we are splitting Section 4 into two separate "Resuts" and "Discussion" sections. In these sections, we first present density profiles, now separated by ELM grid-cells, from each experiment in the order which they are described in our revised Section 3. To quantify improvements, we add to Section 4 root mean squared errors (RMSE) calculated with reference to a comprehensive dataset of available measurements described by Montgomery et al. (2018). We also replace "improvements" with model "developments" unless justified with quantitative analysis.

- *Section 3.1 – The abstract states that the authors improve the depth of snowpack in the ELM from 1 m to up to 60 m, while the Figure 1 caption states that the new model layers can extend to 80 m. Why is there a discrepancy between these two extended depths?*

  Our version of ELM now has a vertical resolution of roughly 8 m in the top 60 m of firn. With a semi-infinite bottom most layer, ELM can also accommodate a total firn thickness much deeper than 60 m, as demonstrated in Figure 1. Because the grid spacing below about 60 m deep increases indefinitely, we generally exclude results from this semi-infinite layer when its depth exceeds 60 m, hence the 60 m value stated in the abstract.

  Again, to better reflect this important detail, we changed the wording in the caption of Fig. 1, which now includes "semi-infinite."

- *Section 3.3.2 – a brief description of the Greenland sites used from the Mosley-Thompson et al. (2001) study would be helpful here. There were quite a few cores in that thorough study. How did the authors decide which cores to average into a composite GrIS density profile? The accumulation rate varies quite a bit across Greenland, especially north to south. Were northern and southern sites averaged together? Additionally, only sites near Siple Dome were used in Antarctica. Since sites in East Antarctica has much lower mean annual temperatures and accumulation rates that Siple Dome, it may be more appropriate to claim that this applies to West Antarctica than both Antarctic ice sheets.*

  We agree that the comparison against the Mosley-Thompson et al. (2001) study is underdeveloped and over simplified. It is also insufficient to extrapolate results from our Siple Dome case study to the entire Antarctic Ice Sheet(s).

  To improve (and update) these case studies, we are adding to Section 4 a new analysis that controls results for geographic location and includes RMSEs calculated with reference to a comprehensive set of available measurements described by Montgomery et al. (2018). This new analysis, in addition to providing geographically controlled metrics for evaluating model performance, better serves our study's primary objective of improving the representation of dry firn densification in ELM's accumulation zones.

- *Section 4.1 – Lines 237-239: It would be helpful to the reader if the authors indicate which model they're referring here when they say "...this dynamic implementation of eq. (8)..." and "...the original compaction parameterization, from eq. (3)." Are these referring to models vK'17+ and A'76, respectively?*

  Another reviewer also brought up that Section 4 is confusing and that we should revise Section 3 to improve the clarity of the manuscript.

To minimize guess work a reader must endure, we are following Vincent Verjan's suggestion of reorganizing Section 3 so that its (sub-)subsections will correspond to and identify the unique model configurations that appear throughout the manuscript. Our revised Section 4.1 also follows that outline.

- *Lines 225-227: What is the justification for using the 1901-1920 reanalysis data to generate the steady-state density profiles? Is this considered an average period of time? If so, what metric was used to determine that it was best to use the data from 1901-1920?*
  We use the 1901-1920 time period for two reasons. First, according to the IPCC Fifth Assessment Report, these decades represent a relatively stable surface climate in terms of the global mean temperature (Hartmann et al., 2013). Second, these decades are the earliest decades available from the CRUNCEP atmospheric forcing data, which brings us as close to a pre-industrial climate forcing as possible.
  In our revision, we will add this motivation where we introduce the atmospheric forcing (previously Section 3.1.1.).

- *Lines 225-227: The spin-up of the model simulated 260 years of snow accumulation is adequate for creating a typical dry snow-zone firn column in Greenland but would not reach back far enough to erase the natural firn density profiles in East Antarctica. Why was a spin-up of 260 years chosen?*
  We are aware of the difficulties associated with spinning up the snowpack and firn column for East Antarctica. Doing so would simply require computational resources beyond our allotment.
  We choose 260 years for the duration of the spin-up period because it achieves a balance of fulfilling a large number of grid-cells of interest while being computationally feasible. The exact value of 260 years is somewhat arbitrary, which we reached based on rough calculations for how long it would take to reach pore close off based on values from Cuffey & Paterson, 2010)

- *Lines 227-229: are the authors referring to two of the examples given (-27C and -20C) here? Describing results for scenarios with mean annual temperature "within a couple degrees of -25C" is imprecise and leaves the reader wondering if they're missing a panel in Figure 2 for the -25C scenario (and other scenarios in between).*
  We agree that the presentation of results here are imprecise and need further specification.
  To help readers better interpret these results, we are changing the panels in Figs. 2, 3, and 4 to show mean annual temperatures of -39, -32, and -25 $^{\circ}C$, while modifying descriptions in the text to be more precise.

- *Lines 229-230: It'd be helpful to indicate here that the results from the Herron & Langway model are the colored bars in Figure 2.*
  That is already stated in the figure captions, but we will add it to the text to improve readability.

- *Lines 237-239: In the discussion of Figure 3 here, the authors describe that the dynamic implementation of eq. (8) in the ELM (Again, is this the vK'17?) results in characteristics depth (550 kg/m3) more consistent with Herron & Langway for T $>$ -32C, but the authors do no[t] present any of the results for the T=-32C scenario. How was this cutoff determined? Additionally, the panel for the T = -20C scenario shows that neither A'10 nor vK'17+ do a* *good job predicting the characteristic depth. Therefore, how do the authors conclude that the results are more consistent with H&L for T > -32C?*

It is (again) confusing for readers to understand which model configuration corresponds with which equations. This is due to the poor structure of Sections 3 and 4. The -32 °C "cutoff" is a rough one, which would be more apparent with a different (or more complete) set of panels to go with Figs. 2, 3, and 4. The characteristic depth is not well predicted for temperatures greater than -25 °C, where it is likely that melt starts accelerating densification in ELM, an effect not really accounted for by Herron and Langway (1980).

We are applying several changes to the manuscript to clear up these confusing matters in our revision. First, as described above, we are restructuring Sections 3 and 4 so that they will streamline the ELM experiments into (sub-)subsections, hopefully removing some guess work readers must currently endure to interpret the results. Second, we are changing the mean annual temperatures shown in Figs 2, 3, and 4 (i.e., to -39, -32, and -25 $^{\circ}C$) to better reflect the specific objective of the study, which focuses on dry firn densification and thus cannot make definitive conclusions for regions that are warmer than roughly -25 °C. Third, we will clarify that our interpretations stated above do not pertain to warmer regions where melt probably affects densification rates and thus deteriorates the comparison validity against Herron and Langway (1980).

- *Figures 2 & 4: which lines are the authors referring to in these captions when they say "Line graphs show 100-year means from ELM simulations..."? All of the lines (except dashed & dotted?)? This vague statement is confusing due to the number of lines plotted.*

  We are referring to the ELM simulation results.

  Because this is not clear, instead of plotting multiple ELM experiments on the same figure, we will separate ELM results by experiment in Figs. 2, 3, and 4, which we are moving into a new, separate "Discussion" section to emphasize a new analysis in Section 4 that more directly evaluates ELM simulation results against observations.

- *Figure 2 – how are the range of surface densities used in the empirical modeling shown in the figure?*

  We choose 300 to 380 kg m^{-3} for the range of surface densities to plug into the model of Herron and Langway (1980). These values, though somewhat arbitrary, are selected to be representative of surface densities observed on ice sheets.

- *Figure 3 – comparing the steady-state density profiles generated for the 3 mean annual temperatures to sites with similar conditions (Table 2.2, Cuffey & Paterson, 2010), it appears that the ELM and empirical modeling results reach pore close-off density at too shallow of a depth for -20C, empirical modeling results.*

  Yes. Densification does happen quickly for relatively warm regions. This might indicate that the models' temperature sensitivities are too strong, or that the effects of melt are muddying the comparison to what is generally an empirical model for dry firn densification.

  Because this study focuses on dry firn densification, we are removing all our results and interpretations from ELM grid cells that have mean annual temperatures greater than -25 °C. This includes in both Figs. (2, 3, and 4) and in text.

- *Figure 4 – The stated goal for this figure is to better understand what drives rapid densification near the surface. Therefore, it would help the reader to have a second x-axis displaying*

the depth. Since we don't have a depth-age scale, it's hard to interpret where the near-surface is in this figure. What causes the jump in vertical strain rate in the empirical modeling results (colored lines)? Additionally, the error bars make it very hard to see the black and grey line trends. Consider altering the error bars in some way to allow the lines to become more visible. In the lower panel, why do the ELM simulation lines begin near 40 years instead of 0?
We understand that Fig. 4 is difficult to interpret.
We will heed your suggestions when we remake Fig. 4.

- *Technical Corrections:*
  - *L10: remove 'when' from 'compared to when using...'*
    Done.
  - *L25: 'coupled' twice in this sentence, sounds awkward*
    We will rephrase this sentence.
  - *L34: need 'it' after 'implemented' here*
    We disagree, but reworded this sentence ("...and implemented..." → "..., implementing...") to possibly improve clarity.
  - *L225: the authors start what "with a baseline configuration. . ."?*
    We we mean something like "...start our presentation..." but this is an awkward way to begin a section.
    We will reword this sentence in our revised Section 4.
  - *L260: don't need comma after "ELM"*
    Removed.
  - *Figure 1: describe variable 'z' in the caption*
    We will rename this axis label "Depth".
  - *Figures 2 & 3: the bottom row of figures overlaps the y-axis units, space these out slightly so that each axis is legible.*
    We will space out the subplots accordingly.
  - *Figure 4: the top row of figures overlaps the y-axis units, space these out slightly so that the axis is legible.*
    We will space out the subplots accordingly.
  - *Figure 6: add description of density measurements are shown in blue, as well as that the dotted and dashed lines represent the characteristic density and pore close-off density, respectively, to the caption.*
    We will update the caption accordingly.

For a direct look at how we address these comments, please see the forthcoming revised manuscript.

Sincerely,

Adam M. Schneider et al.