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Interactive comment on "Snowpack and firn densification in the Energy Exascale Earth System Model (E3SM) (version 1.2)" *by* Adam M. Schneider et al.

Anonymous Referee #3

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The manuscript by Schneider et al. is concerned with improving the simulation of snow and firn in the E3SM Land Model (ELM), in particular snow density. The subject is timely and fits the purpose of GMD well. The topic is also relevant, as firn acts as a major control on the surface hydrology and surface mass balance of ice sheets, which is relevant when coupling dynamical ice sheet models with Earth System Models. For this reason, I was happy to learn that E3SM is developing into this general direction. The authors demonstrate good knowledge of the literature, and they tested and recalibrated models for their purpose, which I applaud. The quality of the figures is good and the writing as well. Unfortunately, I have three major concerns with the study, which I will lay out below.

The first concern is about the statistical modelling approach explained in Sect 3.2. I didn't get very warm feelings about this. For instance, in the direct comparisons carried out in Sect. 4.1.1 and Figures 2-4, output from a coarse resolution ELM simulation with 6-hourly CRU-NCEP forcing is compared to steady-state profiles from the Herron & Langway model with idealized (synthetic) forcing. To me this feels like comparing apples to pears. Unnecessarily, it seems, since the ELM (like CLM) can also be forced with synthetic data in single-column mode, offering a more direct comparison. After all, to quote Arthern et al. (2010), "Changes in weather and climate can cause temperature, accumulation rate, and depositional density to vary. Consequently, and in violation of Sorge's Law, the density profile r(z, t) will fluctuate with time t." It is to be expected that this variation causes differences mainly in the upper $\sim$10 m of the firn pack, i.e. the active layer, which is unsurprisingly where the largest differences between the Herron&Langway model and the ELM simulations is found (Figure 2 & 3). Arthern2010 further notes that "Alternative models, broadly based upon the Herron and Langway [1980] parameterization, have employed different formulations for the sensitivity to temperature [Li and Zwally, 2004; Helsen et al., 2008]". This quote is a hint that the temperature dependence of Herron & Langway is not to be taken as the truth, which is kind of what the authors seem to be doing in Section 4.1.2, but also in the next section (4.1.2) where they calibrate model coefficients using their synthetic HL density profiles. I feel the heavy reliance on the HL model and synthetic data isn't properly justified.

My second concern is with the readability of the manuscript, and in particular the range of different model configurations that are presented, and the purpose for all of them. The title of the paper is "Snowpack and firn densification in the E3SM", however at the end of the manuscript I'm lost to which results are now representative of the improved E3SM and which aren't. Line 184 seems to suggest that the coefficients in the new E3SM are optimised from one of the other models, however it isn't stated which one, and the final configuration in E3SM is not named. The wording in Line 320 ("might expect") and Line 376 ("a first step") is also contradictory to this, suggesting that the coefficient optimization isn't really applied at all in E3SM. Does that mean that the entire Section 4.1.2 is actually superfluous and could be removed? Are none of the models discussed in this paper actually adopted in E3SM? I encourage the authors to make this more explicit. Alternatively, the authors could choose to take the focus off E3SM and shape it into a more general firn-modelling paper, I'll leave that up to them.

My third major concern is with the comparison to observed firn core data (Section 4.2). Here the authors aggregate ELM data from across the GrIS from a coarse resolution simulation, and compare this a single point measurement (or at least, an approximation to this). Again, this seems to me like comparing apples to pears and a pretty crude approach. For instance, there will most certainly be grid cells in the composite that experience melt, whereas the interest is on dry firn compaction. Can the ELM not be forced in single-column mode with high-resolution meteorological data from e.g. ERA5, or a high resolution run with E3SM, more approximate to the actual weather at the site?

All in all, I find this paper not convincing in its methodology, and I feel further justification or experiments are needed.

Specific comments

L61: pressure not pressures

L130: Actually, Muntjewerf et al. (2020) provides little detail on their model setup, and none on snow/firn modelling. Suggest to remove, and optionally replace with the following reference, which does actually provide more detail on snow modelling within CESM:

van Kampenhout, L., Lenaerts, J. T. M., Lipscomb, W. H., Lhermitte, S., Noël, B., Vizcaíno, M., et al. (2020). Present-Day Greenland Ice Sheet Climate and Surface Mass Balance in CESM2. Journal of Geophysical Research: Earth Surface, 125(2), e2019JF005318. https://doi.org/10.1029/2019JF005318

L143: This title suggests that the new surface density scheme is only applied over ice sheets. However, this is not explained anywhere, so the title should be changed?

L180: Could you comment on what basis the values for T and A were selected? In Line 220 you define the “dry snow zone” as 0.5 m SWE / year, whereas here the value of 0.4 appears.

L195: Could you comment on the quality of the meteorological data in CRU-NCEP over the regions of interest, i.e. ice sheets? Do you think the outcome of the simulations depends a great deal on the choice of meteorological forcing ?

L196: please specify what nominal resolution (in degrees or km) does the ne11 resolution correspond to?

L215: Since this manuscript concerns a global model, and there are only 16 layers to begin with, two reference firn density profiles are probably justified. Just be aware that there are more firn cores out there, e.g. see Figure 1 in the recently published TC paper by Verjans et al. :

Verjans, V., Leeson, A. A., Nemeth, C., Stevens, C. M., Kuipers Munneke, P., Noël, B., & van Wessem, J. M. (2020). Bayesian calibration of firn densification models. The Cryosphere, 14(9), 3017–3032. https://doi.org/10.5194/tc-14-3017-2020

L260: remove comma after ELM ?

L 373: near-surface firn densities that are too low, not large?

L 376: E3SM Project: not clear, is this a reference?

L 380: and AIS? Surface melt is believed (or known) to be important for the stability of ice shelves.

Reference list: to avoid cluttering, I’d suggest to remove the URLs and replace with DOI where needed.

L 446 : fix title L 483 : fix title L 512 : fix title

Figure 4: The caption describes the meaning of the line graphs in the first row, but not the crosses used in the second row.

Figure 5: The titles of the subfigures could be made more informative. Also, the colour bar could be made more restrictive, it appears densities < 300 kg/m3 do not occur at all.

Figure 6: The legend appears not consistent with the previous figures, e.g. ELM-A'10 instead of just A'10.

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