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The Cryosphere Discussions

Interactive comment on "The diurnal Energy Balance Model (dEBM): A convenient surface mass balance solution for ice sheets in Earth System modeling" *by* Uta Krebs-Kanzow et al.

Anonymous Referee #1

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This manuscript presents a low-complexity energy and mass balance model for the surface of the Greenland ice sheet. Special attention is given to a parameterization of the diurnal cycle of melting and refreezing, based on a previous implementation of a very similar model, published by the same authors. Different climate boundary conditions are used to test the performance of the model. The text is complete and pleasant to read. The figures are adequate. I think the results are interesting and that this new model is a valuable addition to the existing literature. My comments are mostly minor but they include some questions on the underlying equations. In general, I would like to read more about the expected use cases for this new model. How fast does it run? Is there room for future improvements or would they slow it down too much? If

dEBM is used in the framework of AWI-ESM or other AOGCMs, is the computational speed relevant compared to the climate model? What is the reason for the focus on monthly input data when GCMs can supply daily data? I believe that most of these questions will be very easy to answer but they are valuable as context.

Detailed comments:

figure 2: The distribution appears to also be bimodal in air temperature.

fig. 4, 5: Are good matches in variability and mean mutually exclusive? Why (not)?

fig. 9: dEBM underestimates both melting and refreezing as compared to MAR. What consequences does that have for the sensitivity under different climate boundary conditions?

fig. 10: Albedo too high in dEBM, especially in low accumulation North East where snow aging is most important.

p2, l3ff: You could cite Aschwanden et al. (2019) in this paragraph for showing that calving becomes gradually less important in the future.

p2, l15: The purpose of this sentence is not clear.

p2, l20: Avoid brackets

p2, l26: Please consider referencing van de Berg et al. (2011) and Robinson and Goelzer (2014). van de Berg et al. could also be discussed in section 5.3.

p2, l32: "around zero", replace with "melting point"

p3, l1: The optimal resolution is a matter of debate. I do not think that we can reliably say that 10 km is enough.

p3, l4: Typo: Noël

p3, l 28: "the first section" This should probably read "second" or "following".

p3, l33: remove "used in the following"

p4, l9ff: I found the three paragraphs at the center of the page hard to follow, possibly because they invert the logical progression of the following sections (2.4 -> 2.3 -> 2.2). Also, they contain several assumptions and it is not always clear if they have been tested, maybe in previous work, or if they are otherwise justified.

p4, l15: Within the two clusters there is a clear range of temperatures (Fig. 2), probably related to an implicit spatial signal. Please discuss how this impacts this important assumption. In addition, please discuss how the limited coverage of the PROMICE stations may skew the results.

p4, l31: "implicates" -> "implies" (throughout the paper)

p5, l8: typo: temperature; Does this statement concern monthly average values?

p6, l1: This is a strong statement that requires a reference.

p6, l7: Maybe "upper limit" is a better phrase than "bounded above"?

eq. 4: "mth" is not defined. I suspect is is the month, but that would be inconsistent with equation 14. I suggest to find a suitable symbol. Also, the square brackets are not necessary, I think.

p6, l15: "is" -> "was"

section 2.3: Almost every section starts with a reference to Krebs-Kanzow et al.

eq. 5: Why is the linearization around T0 needed? The presentation of these central equations would greatly benefit from a more detailed description of the individual terms. The term containing "a" contains the downward longwave flux, but I do not understand why this term contains the emissivity of ice. This is also included in Krebs-Kanzow et al. (2018b).

p6, l19: missing: "at THE melting point"

p6, l27: Why not use the LW down forcing directly?

eq. 7: sigma has not yet been defined. This is inconsistent with the Stefan-Boltzmann constant. I am confused over the units of SW, Q, LW, etc. The second line on equation 7 suggests that SW_MP is smaller than SW_fair, because both the fraction of times and the q factor are smaller than one. This makes sense if SW is an energy. However, SW is also used in equation 5, where the second term uses the (linearized) Stefan Boltzmann law. It should thus have the unit power, i.e., energy per unit time.

section 2.4: It might help to move the information about pre-processing further up.

p7, l24: add: "...law DOWNWARD longwave radiation can be..."

p8, l2: I think H_int has not been introduced yet (see line 8).

p8, l16: typo: "."

p8, l19: typo

p9, l4: I think tau_cloudy is missing in the second equation.

eq. 13: So eps_fair and eps_cloudy are both smaller than eps_a?

p10, l9: "PDD" twice

p10, l10: Does that mean that T_MP is not a temperature? The same equality between T and PDD is used in equation 7.

p10, l27: typo

eq. 14: Inconsistent use of dT and "-1". See also "-12" on the second equation on the same page.

p11, l11: remove "choice of the"

p11, l13: typo

p12, l5: This should reference equation 5, not 6.

p12, l6: Does R not have units? Why were no positive values tested?

p12, l8: Please include information about the grid resolution earlier in the ms., e.g., in section 2.4.

p12, l11: typo: "data setS"

p13, l9: units of RMSE missing

p13, l10f: Why are the biases negative. Is this shown in figure 5?

p14, l1: The best value of R is at the extreme of the tested range. See comment above about positive anomalies for R.

p15, l16: typo

p15, l18: The biases cancel out, but their existence indicates that important physical processes are not captured. Please discuss.

p16, l3: "multitude of reasons" I agree, but we need more detail here given the seasonal biases that may point to missing processes that are important under different climate boundary conditions.

p16, l5: I disagree with this assessment. Sub-monthly variations may play a role, but the lack of a snow aging algorithm is likely also important in the dry interior of Greenland. This is consistent with the anomalously high albedo (line 12).

p21, l7: typo

p21, l9: typo

p21, l17: Is this grid different from the one above? Please make this explicit.

p25, l10: typo "surface typeS"

p25, l11: typo

p25, l16: typo

p27, l16: "imply"

References: Aschwanden et al. (2019), Contribution of the Greenland Ice Sheet to sea level over the next millennium, doi:10.1126/sciadv.aav9396

van de Berg et al. (2011), Significant contribution of insolation to Eemian melting of the Greenland ice sheet, doi:10.1038/NGEO1245

Robinson and Goelzer (2014), The importance of insolation changes for paleo ice sheet modeling, doi:10.5194/tc-8-1419-2014

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