

Interactive comment on “Brief communication: An Ice surface melt scheme including the diurnal cycle of solar radiation” by Uta Krebs-Kanzow et al.

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This manuscript presents an alternative approach to calculating surface melt on an ice sheet that is more complex than the PDD method, but still simpler than the intermediate complexity alternatives available so far. It is a creative approach, refreshing and a very welcome addition to a relatively small set of melt models available for long time-scale ice sheet modeling. It indeed seems to present significant benefits over other methods. Therefore I would very much like to see this work published in TC.

That said, I find the manuscript in need of major revisions (see major and minor comments below). I simply get the feeling that the manuscript is not complete. I would

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prefer to see a slightly longer paper with at least some sensitivity analysis (How sensitive is the presented model to the parameter choices? How important is the diurnal aspect of the model?) and deeper discussion of subtleties of the approach (e.g., T_{\min} , $PDD(T_a)$ versus T_a – see comments below), and its comparison to other methods.

With such revisions, I would then prefer to see this as a normal article rather than a “Brief Communication” (although I understand this is an editorial decision).

– Major comments —————

The introduction does a nice job of concisely laying out the problem. However, the description of the alternative methods is not very precise, and I think the comparison with them could be more thorough and analytical.

- As pointed out by the editor, a distinction should be made when discussing a melt model alone (which is a somewhat artificial construct in isolation) versus an energy balance model, which may calculate many variables that are useful for ice-sheet modeling (ice temperature, albedo, refreezing, smb). Thus I see PDD, ETIMs, ITM (see next point) and the model presented here in a similar category – melt models that can be used as a subcomponent of smb models – while SEMIC and full EBMs are more wholistic solutions. A bit of clarity here on these definitions would improve the manuscript greatly.

-“ETIM” as used throughout the text seems to be the wrong term for the comparison being made here. ETIMs involve a “temperature index” such as PDD (Hock et al., 2003; Pelliccioti et al., 2005). This is why, in Robinson et al., 2010, we opted to create the name ITM for the model of Eq. 14, since we saw it simply as an “insolation-temperature melt model”.

- Equation 14 is not correct, in representing the approach of Pollard (1980) or Robinson et al. (2010). The second term should not contain PDDs, but the mean near-surface air temperature relative to the melting point: $k_1(T_a - T_0)$.

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- SEMIC also supports the input of monthly temperature data, although the model itself is calculated on daily time steps (from Krapp et al., 2017: “In principle, the use of monthly input data is also supported but would require interpolation to daily time steps.”). I would additionally note that SEMIC is open source and prepared to run easily with MAR data as input, making its comparison with dEBM feasible if the authors wanted to be more thorough. It would certainly be convincing if it could be explicitly shown that dEBM can do a better job than SEMIC for a much lower cost. (This point is only a suggestion, and I would not consider it necessary for revision.)

I find the approach outlined here quite elegant and the physical derivation is nicely described. However, then I am surprised to see PDD pop up in Eq. 6 again. Would it not be simpler keep $(T_a - T_0)$ here? The only reason to use PDDs is to incorporate a measure of variability in T_a . But it seems to me that if you want to include the variability around T_a in the melt model, it would be more appropriate to apply it to the whole equation rather than just to the temperature term (ie, calculate the average melt rate from the distribution of melt rates for the month).

If you follow the path above, this change would make Eq. 6 essentially equivalent to Eq. 14 (also without PDDs), and it maintains its physically-based origins and makes it obvious that the key differences are: - The term q_{Φ} , which scales the insolation according to the time it is actually available. - The term dt_{Φ} , which scales the melt according to the time when it is relevant. - The derivation of the constants c_1 and c_2 .

I note that the used values of $c_1 = 14.4$ and $c_2 = -71.9$ are not too far from values used in Eq. 14 for $k_1 = 10$ and $k_2 = -60.5$. It would be interesting to understand if this is systematic, that generally $c_1 > k_1$ and $k_2 > c_2$, to compensate for the lack of q_{Φ} and dt_{Φ} terms. For example, if in Eq. 6, you set $q_{\Phi} = 1$ and $dt_{\Phi} = dt$, how well does your model perform (after retuning the constants) – as well as before, or is the performance degraded? In other words, I would be happy to see an analysis that specifically shows the value of incorporating the diurnal terms to the model.

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– Minor comments —

Units and variables: Please check the units carefully. For example, T_a is in Kelvin, but then $T_{\min} = (T_0 - 6.5)$ K, right? Also, in Eq. 14, is the first term “SW_0” the same as “SWD_0” defined earlier in the text? Please keep the same terms throughout.

Page 1, line 14: information on => information about

Page 1, line 15: refreeze => refreezing

Page 1, line 23: computational => computationally

Page 1, line 24: temperatures. => temperatures as input.

Page 1, line 25: or paleo-temperature => and paleo-temperature

Page 2, line 1: aproach => approach

Page 2, line 1: “Another empirical aproach, the enhanced temperature-index method, ETIM” <= In addition to the fact that I believe ETIM is the wrong term here, as I already mentioned, ETIM refers to a class of models that can take many forms that generally extend PDD in various ways, not to a specific model formulation. Therefore, I would rephrase here. Alternatively, you can use the term “ITM”, which does refer to the formulation of Pollard (1980). Or, a more descriptive term for this model would be “linearized EBM” (Pollard, 1980).

Page 2, line 19: a surface melt rate => a non-zero/positive surface melt rate

Page 3, line 5 (Eq. 3): I see no reason why e_i should appear multiplied with LW_{down} . This is only relevant for LW_{up} (as in Eq. 4), correct?

Page 3, line 7: Per definitionem => By definition

Page 3, line 23 (Eq. 7): It looks like c_1 is missing the term e_a , following the current equation formulations.

Page 4, Section 2.1: Please make sure to use the same variables and notation as in

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the rest of the text. I guess that the elevation angle Φ in the previous section is the same as the elevation angle θ in Sect. 2.1.

Page 4, Eq. 13: I would suggest adding the intermediate definition of q_{Φ} here to remind readers of your previous definition: $q_{\Phi} = SW_{\Phi} / SW_0 =$ [full definition]. Also again be clear about SW versus SWD.

Page 4, line 23: What is the calculation of $\Phi = 23.5^{\circ}$ used for later? As I understand all tests were using MAR albedo, etc. Is this just an example?

Page 5, line 11: Eqations => Equations

Page 5, line 11: “Eqations (6) and (14) appear formally similar, with the first and third term representing the radiative contribution and the second term representing the PDD contribution.” <= This sentence is contaminated by the mistake in Eq. 14, however, just thinking about it in terms of Eq. 6, it is clear from the derivation that the first term represents shortwave radiation and the second and third terms represent the net longwave radiation and heat fluxes from R combined. Please rephrase.

Page 5, line 11-21: Generally, I find this paragraph difficult to follow. Is the “flat elliptic” referring to the orbital configuration of the Earth, or some pattern in the figure itself? Does “going along with” mean “causing”? I find that “PDD contribution” a not very convenient name for the second term in Eq. 6, since it is easily confused with the PDD melt model itself in this context. I would consider serious revision here for clarity.

Page 5, line 23: derived => obtained?

Page 5, line 25: “defective input” <= I’m not quite sure what you want to say with this sentence, consider rephrasing somewhat. Wouldn’t it be possible to make your ideal input data “defective” for testing purposes, if that was your goal?

Page 5, line 27: due to => Given

Page 5, line 32: It does not seem appropriate to limit the comparison of dEBM to points

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that satisfy $T_a > -6.5$ C. Either the value of $T_{\min} = -6.5$ C is adequate, or T_{\min} should be set to a lower value. In either case, the correct choice of this threshold should be reflected by the comparison to MAR melt. Based on the horizontal line of dark blue points in Figs. 1 & 2, I have to guess that the threshold chosen here is too high, or for some reason the dEBM underestimates melt at low temperatures. This should be discussed in the paper clearly.

Page 7, line 1: biasses => biases

Page 7, Figure 2 caption: lenght => length

Page 7, line 6: refreeze => refreezing

Page 7, line 6: used together with the enhanced temperature index method in => presented by

Page 8, line 13: “This threshold temperature should be considered as a tuning parameter” <= I had understood this T_{\min} simply to be a cost-saving measure, to avoid calculating the melt model for points where melt would be zero. However, this sentence makes me believe that the parameter is more important than I realized. Please elaborate on the role of T_{\min} more in the derivation section for clarity.

Page 8, line 16: Depending on application => Depending on the application

– References —

Hock et al., Journal of Hydrology, doi:10.1016/S0022-1694(03)00257-9, 2003

Pellicciotti et al., Journal of Glaciology, doi:10.3189/172756505781829124, 2005

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