The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-130-RC3, 2018

The Cryosphere Discussions

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

M. Krapp (Referee)

mariokrapp@gmail.com

Received and published: 15 August 2018

This paper presents a new melt scheme which can be applied over glaciated surfaces such as ice sheets or glaciers. Its novel component is the latitude-dependent diurnal cycle of solar radiation thereby making it flexible enough to be applied for different regions and over different periods of time. The paper introduces an innovative melt scheme to complement existing melt schemes such as PDD or ETIM models. The model accounts for time- and latitude-depending changes in the diurnal cycle of incoming solar radiation which makes it appealing and very relevant to the ice sheet modelling community. The paper is well written and and the derivation of the model

Printer-friendly version

Discussion paper

equations is elegant but the paper has a few shortcomings and therefore needs major revisions before I would recommend it for publication in TC.

I also agree with Alex (one of the other reviewers) that an article rather than a "brief communication" would be the better format for this paper.

Major comments

- Solar elevation angle and surface slope: Whereas large parts of the Greenland ice sheet are rather flat its margins, where most of the melt occurs) are not and glaciers are even more sensitive to the slope of the embedding terrain. I suspect that the daily solar elevation angle depends on how the ice surface faces the sun. How much of an effect would a surface slope have and could that be included in Sect. 2.1?

- I expect the atmospheric transmissivity (Sect. 2.1) to decrease with increasing solar zenith angle. How much of an effect would that have?

- I think that using a single parameter for the emissivity of air (ϵ_a) is also too simplistic and the contribution of cloud cover is missing. $LW^{\downarrow}$ is parameterised using ϵ_a, which is the clear sky emissivity but how do you deal with cloudy skies? In fact, ϵ_a can vary between 0.7 (clear sky) and 1.0 (fully overcast). Therefore, the value for c_2 can vary between -90 and 0 W/m2 if you account for varying ϵ_a. That means that a full overcast sky would add about 90 W/m2 to the surface energy uptake Q.

- I think in Eq. (7), ϵ_a is missing in the term for c_1, i.e., $c_1 = \epsilon_a \epsilon_i \sigma 4 T_0^3 + \beta$. If that is the case c_1 also yields a different value in line 25 on page 4 and my above argument about varying ϵ_a implies that c_1 can vary 13 and 14.4 W/m2K.

- Sensitivity of model parameters: I agree with the other reviewer that this paper benefits from a sensitivity analysis of the paper. First, because most of the parameters have been fixed for a "First evaluation of the scheme" (Sect. 3) and second, it helps the reader to see how the model responds to different assumed parameter choices. For example, is the choice of β, T_{min}, or ϵ_a arbitrary or representative of the Greenland ice sheet? What does the reader need to change to apply this model to Antarctica and/or other ice caps and glaciers? In the conclusion you state the dEBM "can be applied to other ice sheets and glaciers and under different climate conditions". I think statement can be underpinned by a thorough parameter sensitivity analysis and, perhaps, a recommendation for those different conditions (e.g., for the more recent or deeper past)

- The PDD component of dEBM is in general smaller than in ETIM (Fig. 1b). Obviously, the PDD contribution of dEBM would be larger for a larger β which can range between 7 to 20 W/m2K as you said earlier.

- I would like to see a plot showing the time series of monthly melt and different diagnostics (as is shown in the supplement). For example, melt rates and its individual components (the PDD and the ETIM-related term) in Eq. (6), or the parameterised short- and longwave radiation SW and $LW^{\downarrow}$ would help the reader to understand what the model is doing internally. Specifically it would be nice to see how q_{ϕ}, which is the novel part of your melt scheme, changes over time.

- To me everything in the conclusion, except for the first paragraph, is more like a "summary and discussion" section than an actual conclusion. Please revise.

- I guess if you consider a revision as article you can easily move Figure S1 (which is the only item in the supplement) to the main text.

- Out of curiosity (not needed for the revison): If the melt scheme just uses a few input parameters, is it possible to force it with atmospheric data from available observations of the GrIS? For example, GC-MET (http://cires1.colorado.edu/steffen/

Printer-friendly version

Discussion paper

gcnet/) or PROMICE (https://www.promice.dk/home.html)

Minor comments

- p2 ll.27-29: It is not clear whether SW_0 or SW_ϕ mean surface or TOA shortwave radiation.
- p.5 l22: Please, specify what the atmospheric forcing variables from the MAR model are.
- Please add a table with model parameters and parameter values used in the main text and analysis.
- Fig. 2: add units to axis labels; duplicate y-axis labels ("PDD", "ETIM", and "dEBM")
- Fig. 3: the min/max colors are really dark and hard to see

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-130, 2018.