The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-259-SC1, 2020

Interactive comment on "Impact of updated radiative transfer scheme in RACMO2.3p3 on the surface mass and energy budget of the Greenland ice sheet" *by* Christiaan T. van Dalum et al.

Vincent Verjans

v.verjans@lancaster.ac.uk

Received and published: 23 October 2020

This study is a valuable contribution to improving modelled surface energy balance of ice sheets. RACMO is a widely used climate model on both the Greenland and Antarctic ice sheets, and developing a model scheme to account for subsurface energy absorption is important. I find the manuscript well-written and the study is comprehensive in many regards. It provides much insight into radiative energy absorption, which has so far been neglected in regional climate models and firn models. The main objective of this short comment is to support the publication of the study, to provide a few suggestions for improvement and to ask questions about the findings. My suggestions

are only minor, and I know that a future comprehensive evaluation by reviewers will be more substantial and will address several issues that I do not mention.

For readers who are not familiar with surface energy balance and radiative energy transfer, it can be complicated to grasp all components at play. Part of the radiative energy is directly re-emitted, part of it contributes to the surface energy absorption, part of it contributes to the internal energy absorption and this partitioning depends on both wavelength and material properties. In my opinion, Figure 9 helps to how these aspects intertwine. But it comes rather late in the manuscript and it may be difficult for the reader to have a clear understanding of the radiative transfer when evaluating the results. I think it would be helpful to have a graph similar to Figure 9 alongside Figure 1, maybe for theoretical snow/ice layers and radiation conditions. Alternatively, a comprehensive table could also help. Three columns could be used ("Reflected", "Surface absorption", "Internal absorption"), with two rows ("Fresh snow", "Ice"). Each row can subsequently be split into three subrows ("UV", "Visible", "Near-IR"). The columns for each subrow can then be filled with theoretical % values or qualitative estimations ("Very low", "Low", "Medium", "High", etc.).

The impact of internal energy absorption on melt in the interior should be better explained. I find the explanations provided a little contradictory: Section 3.1: " In the interior, significant differences are observed only in south Greenland, with a considerable increase of snow melt (Fig. 3a). As radiation penetration now allows internal heating, it consequently raises subsurface temperatures and increases melt." Section 6.2: " In a broad elevation band around the center, less snow melt and refreezing is observed, illustrating the importance of subsurface energy absorption, which prevents the surface from reaching the melting point." Concerning the statement in Section 3.1, Figure 12b shows that melt differences in south Greenland are essentially albedo-driven and that the contribution of internal heating is minor (as explained at the end of Section 6.2). I understand that there might be a delicate balance between the skin temperature not reaching the melting point and extra subsurface melt. The distinction between dry and melt conditions is important for many reasons, and I hope that the authors can better quantify the isolated impact of internal energy absorption on melting in interior Greenland (which would also be crucial for large areas in Antarctica). Figure 12b shows that internal heating increases melt in the interior. This contradicts the statement of Section 6.2. However, in some localised interior areas, internal heating decreases melt. Why do such differences occur in similar climatic conditions?

The authors have demonstrated how subsurface melt can liberate pore space in ice layers and subsequently increase refreezing capacities. This is an important point with respect to meltwater infiltration and SMB modelling. What can the authors say about the physics of that process? Is it physically plausible that subsurface melt makes ice layers more porous? Or is this simply a model artefact? I understand that the density of an ice layer affected by subsurface melt decreases in Rp3. However, could it be that it is the ice layer thickness that decreases in reality (its uppermost part melts away)? In this case, no additional porosity would be generated and runoff may be enhanced. This is a genuine question.

Finally, here are a few technical comments. I think there might be a sign error in Figure 1. Since F(z) is decreasing with depth, should dF(z)/dz not be negative at all depth? Values on the y-axis are shown as positive however. Can the authors consider changing Figure 12 from (Rp3 WIE - RP3) to (RP3 - RP3 WIE)? This would make a comparison between Figures 3 and 12 more straightforward, so that the reader has a clearer picture of changes related to the modifications between (i) Rp2 and Rp3 and (ii) Rp3 WIE and Rp3. I think that "temporal variability" should be replaced by "spatial variability" on line 234. I believe that there is a typo on line 297. If I understand correctly, it is a lower fraction that contributes to the SEB at S6 compared to Summit and not a "larger fraction". It would be interesting to plot more than the 20 upper layers of Rp3 in Figure 10b to allow a better comparison of the model with the observations. There are some typos/repetitions in Section 6.3.

I wish the authors good luck with the publication process.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-259, 2020.