

The Cryosphere Discuss., referee comment RC3
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Comment on tc-2021-298

Cécile Agosta (Referee)

Referee comment on "Sensitivity of Antarctic surface climate to a new spectral snow albedo and radiative transfer scheme in RACMO2.3p3" by Christiaan T. van Dalum et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-298-RC3>, 2021

This article is well written and pleasant to read. A large effort was made to improve the albedo scheme in the state-of-the-art regional atmospheric model RACMO2, and this work is of high interest for the cryosphere community. The method is well described and the implementation was evaluated over Antarctica using diverse datasets. Consequently I highly recommend this article for publication. However, I found some issues that I would appreciate to be answered before publication, as detailed above.

Major : Abstract and message delivered

Abstract and in the text : "We tuned Rp3 by changing one parameter at a time, allowing us to investigate the sensitivity of the AIS to each change." : You did not change one parameter at a time, you changed the parameters incrementally. It would have been good to change one parameter at a time to decipher more clearly the role of each of your tuning parameter. E.g. you advocate along the article that "subsurface heating" is important, but in your experiments, parameters affecting subsurface heating (SLED, and maybe RF grain size?) are used on top of the others, so that at the end it is difficult to compare their relative importance.

Abstract : "Furthermore, the introduction of subsurface heating in Rp3 significantly improves the snow temperature profile." Conclusion : "Nonetheless, subsurface temperatures in CON have improved significantly and match very well with measurements, showing the added value of subsurface heating to model performance." I think there is not enough evidence in the article to support this statement (detailed bellow).

Finally, I have an open question: with regard with your evaluation, it seems that at the end, you don't improve the albedo compared to Rp2, and you obtain very similar bias in

CON and in Rp2. Is it a compromise to obtain similar results as in Rp2 for other variables?

Along the text :

p1 L20 Cook and Vaughan, 2010 : there is much recent litterature on this topic

p1 L22-23 "mass gain by snowfall and riming" : and drifting snow transport ?

p2 L26-27 "can be as high as 3 m water equivalent (w.e.) yr⁻¹ in the western AP (Van Wessem et al., 2016) and as low as 10 mm w.e. yr⁻¹ in the interior of the East Antarctic ice sheet (EAIS) (Van Wessem et al., 2014)" : it would be better to cite observational article here instead of again model output paper.

p3 L73 "a newly introduced tuning parameter that will be changed as an experiment" : can you explain what does this tuning parameters mean physically? (Because I don't understand why you tune the initial radius and not e.g. the time rate τ)

p4 L105-106 : "others contribute to mass loss, i.e., sublimation (SU), drifting snow erosion (ER) (...)" I don't understand why in the SMB definition drifting snow only contributes to mass loss, and why only mentioning drifting snow "erosion" and not drifting snow transport? Drifting snow transport can lead to mass convergence and thus snow deposition.

p5 L131 "a refreezing grain size of 1 mm". You did not defined this parameter and how it affect the snow metamorphism. □ after a second reading I understand that it is the grain size given to the frozen water when it refreezes, seems now evident (...) but maybe you can just explicit it.

p5 L138-140 "SSA is increased from 60 to 100 m² kg⁻¹, reducing r from 55 μm to 37 μm . An SSA of 100 m² kg⁻¹ better matches observations of fresh snow at Dome C (Libois et al., 2015). Furthermore, this changes the dry snow metamorphism rate from the fastest to the slowest regime, reducing snow growth by an order of magnitude (Fig. 1)." : Why doesn't Fig. 1 show SSA greater than 81.8?

p5 L137-138 "This current parameterization, however, is not optimized for Antarctic conditions.". On which basis do you state this?

p6 L155-156 "Modeled SMB is compared with 1870 SMB measurements including isolated observations and traverses on the EAIS (Fig. 2b). Favier et al. (2013) describe this data set in more detail." : Wang et al (2021 <https://essd.copernicus.org/articles/13/3057/2021/essd-13-3057-2021.html>) nicely updated this dataset.

p6 L158 "to specifically produce a melt rate estimate for Neumayer station (Fig. 2b)" : replace "(Fig. 2b)" by "(see location at Fig. 2b)" (I was expecting melt rates at Fig. 2b).
Idem p8 L172 "(Fig. 2b)"

Figure 2, Figure 3, Figure 10, Figure 11 : Add a label(title) on each panel for readability, e.g. Fig. 2a) "GRL - Rp2", Fig2b) "CON - Rp2"

P9 L183 "In summer (not shown), the signal of Fig. 2 is amplified." : Show it in supplement.

P9 L183-186 "A comparison with observations in DML during summer (Table 2), which is the season where any changes in the albedo have the strongest impact on the SEB, shows that the temperature of Rp2 is modeled well, with a small bias of -0.3°C and a root-mean-square error (RMSE) of 1.4°C . The bias of GRL and CON are larger: 2.0°C and -0.8°C respectively." : the table is nice but I am very frustrated by these numbers averaged over the 9 stations. I would like to see a map of the biases during summer (i.e. these same numbers but with colored dots, with one map for each of the main variables) to estimate the spatial variability in these biases. If not in the main text, it should be added in supplementary.

P11 L214-217 "Here, we show that the snow temperatures at Dome C (Fig. 4) match better with observations (Brucker et al., 2011) in CON than in Rp2 and GRL. During summer (Fig. 4a and b), we observe that Rp2 is somewhat too cold compared to measurements. Results improve for CON, showing the significance of subsurface heating, although the skin temperature in DML is somewhat underestimated (Table 2)." : I think "Improvement" from Rp2 to CON shown in Fig. 4 should be interpreted with caution : (1) observed time series are very short (only one year); (2) observations are only at one location, Dome C; (3) on these profiles Rp2 and CON are very similar, meaning the "improvement" can be by chance. Furthermore, "Results improve for CON, showing the significance of subsurface heating" \square there are many difference between CON and Rp2, why do you state that the main processes involved here is the subsurface heating ? E.g. in Fig4a) the "improvement" seems to be driven by warmer surface temperature with similar sub-surface gradients?

Figure 5 : add all the experiments (or at least add them in supplementary)

p12 L230-231 "The CON settings somewhat underestimate snow metamorphism, leading to higher SSA during summer, but this can be fine tuned using α in Eq. (1)." : Can you detail on this? Show how SSA is evolving for different α between 0.25 and 1? Or at least for your different experiments.

Section 5 : as said above, I would like to see maps of these biases for the different fluxes, at least in supplement.

Figure 7a) The divergent colormap is not relevant for showing the continuous albedo variable. Change the colormap for a continuous one.

Figure 8.g) "SSA as a function of depth" : modeled by CON ? + Rainbow colormap should be prohibited, use a continuous colormap instead (e.g. viridis on python, parula on matlab)

p15 L289-290: "Nonetheless, despite a slightly overestimated albedo, CON provides a better representation of the near-surface climate, albedo and near-surface snow state than the GRL experiment." : This sentence is not very convincing. Fig8h) suggests that albedo is better represented in GRL than in CON. So at the end, in Rp2 and CON you have a better air temperature but for the wrong reasons... However, your conclusions are for Neumayer only, you should highlight that in this sentence. The best would be to compare maps of the different SEB components vs. observations, to have a broader view on the models biases for different stations (as asked above for Section 5).

p18 L322-324 : "For the time step currently employed in Antarctic simulations, a SLED of 5 mm leads to slightly overestimated heat buffering in the uppermost part of the snow layer, leading to more internal melt." : On which basis do you ground this statement? From what I can understand, you tuned all your parameter to obtain the closest results to Rp2? But how are you sure that Rp2 it the closest to reality? (after reading the next section, the comparison with QuickSCAT, it's more convincing)

Figure 11a) you use a divergent colormap for a continuous variable (melt), you should use a continuous colormap instead (e.g. viridis on python, parula on matlab)

p19 L340-341 : "This shows the impact of internal heating and melt-albedo feedback, as these processes significantly enhance melt, similar to findings of Jakobs et al. (2019)." : It can just be an albedo effect (in absolute value), not necessarily a "internal heating and melt-albedo feedback" effect. Why do you advocate for the later?

p20 L349-352 : "In conclusion, melt of the AIS is somewhat sensitive to fresh snow SSA

and fresh dry snow metamorphism and is highly sensitive to the refreezing grain size and SLED. Hence, subsurface heating can warm the snowpack considerably, enhancing melt. Despite the low average melt flux in Antarctica, the impact of subsurface heating should not be neglected for a physical description of (sub)surface melt." : This conclusion does not fit in this section as you discuss only CON and GRL here. In fact this is the same remark as just above. If you want to highlight the impact of "subsurface heating", you should specifically show the simulations where you only change parameters affecting subsurface heating, and not the fresh snow albedo; e.g. SLED if I understand well, and maybe the refreezing grain size, but this latter may one also affect the surface albedo (?)

p21 L371-373 : "These biases demonstrate that Rp2 and CON provide a better representation of the surface climate than GRL" : I don't understand why? p21 L374-375 : "The higher (subsurface) temperatures in GRL lead to excessive melt around the margins and on the ice shelves, locally leading to runoff and a reduced SMB." : Why "subsurface"? Can't it be only caused by surface temperature increase?

p21 L381-382 : "In conclusion, introducing a more physically based albedo scheme in RACMO2 that allows for radiation penetration and subsurface heating improves, after tuning, subsurface snow temperatures in Antarctica." In Section 2.1 you explain that the difference with Rp2 is the full albedo scheme, that has been changed for TARTES adapted for RACMO2 (with SNOWBAL). Here you only cite changes in "radiation penetration and subsurface heating", why citing these two changes only?