General comments

This paper rigorously evaluates the radiative effects of blowing snow on the surface radiation balance near the western margin of the Greenland Ice Sheet, using a one-way offline coupling between the blowing snow scheme (PIEKTUK-D) and the radiation scheme (ecRad-1.4.1) in the regional climate model RACMO2.4p1. The authors demonstrate that blowing snow, modeled as a low-level ice cloud, increases downwelling longwave and slightly reduces downwelling shortwave radiation, thereby improving the representation of the surface radiation balance in the model.

The manuscript is well written, clear, and pleasant to read. The methodology is robust, and the discussion is both rich and concise. The contextualization of the results for the ablation zone of Greenland and the comparison with Antarctica are particularly valuable, as they highlight the broader significance of blowing snow radiative effects and open avenues for future research. The topic aligns perfectly with the scope of The Cryosphere, and I strongly recommend publication after addressing the small and minor comments below. My comments primarily aim to make the discussion more actionable for readers (e.g., by providing concrete avenues for model improvement). The paper is already very good, and my suggestions are intended to further enhance its impact.

Finally, I sincerely apologize for the delay in providing this review for such an already accomplished paper. My schedule has been unusually busy, which, while not an excuse, at least offers some context.

Specific comments

L75: Please specify how the blowing snow frequency is computed (e.g., threshold for defining a "blowing snow day," temporal scale: daily? hourly?). This is essential for reproducibility and comparison with other studies.

L106: The exclusion of the largest particle radius class (245 μm) from the horizontal mass flux calculation may introduce a bias. Could you quantify its impact (e.g., % underestimation of total flux)? If the SPC systematically overestimates this class, this could partially explain the discrepancies between measurements and simulations.

L154: Please add units for the bulk settling velocities (e.g., m s^{-1}) for consistency.

L325: The method of Van den Broeke et al. (2006) and Kuipers Munneke et al. (2011) for estimating cloud cover from observations deserves a brief summary (e.g., based on net radiation? thresholds on SW↓/LW↓?). This will help readers understand your data selection without referring to external sources.

L342–348: Your hypothesis that "inaccuracies in the surface snow compaction model" may translate into an overestimated threshold friction velocity (u_*t) is relevant, but could you specify the physical mechanisms at play? For example domething like “Rapid surface snow compaction could reduce the availability of loose snow, thereby increasing u_*t”. Similalry, Could you sopecify what do you have in mind when

mentioning the interactions between blowing snow and precipitation particles: riming of fresh snow particles reducing their availability for transport? What about surface crust formation limiting erosion? Please be more explicit about the suspected physical processes.

Moreover, why not mention the leads already identified by Lenaerts et al. (2014) (e.g., saltation particle size, gamma distribution parameters)? Even if that version of the model at that time suffered from overestimation (unlike R24), these parameters could also explain your underestimations. A comparative discussion would be valuable.

Similarly, I agree that more observations elesewhere on the ice sheet and on longer time periods could be highly beneficial, but I encourage you to suggest concrete avenues for model improvement, this is always useful for your readers. For example : calibrate the gamma distribution parameters (α, β) using in situ measurements (e.g., SPC), test different snow compaction schemes or hypothesis using specific observations of surface snow properties, focus on saltation layer, evaluate the impact of vertical resolution (is your 8-level grid between 0.1 m and 10 m sufficient to capture near-surface gradients?).

Lines 363–364: A brief discussion on potential biases linked to cloud microphysics in R24 would be valuable. For instance, an underestimation of low-level cloud mass mixing ratios and effective radii could explain part of the residual biases in downwelling longwave radiation. This would help readers better understand the limitations of the results and identify future research directions.

Lines 375–376: Add a brief comment on the implications for coastal Greenland, where cloudy conditions are more frequent. Would the impact of blowing snow on radiation be less pronounced, or conversely amplified by interactions with low-level clouds?

Typos

Figure 3 is referenced (twice in Section 2.1.1) before Figure 2. Please correct the order to avoid confusion.

Line 258: -0.39 m s−1 (units after the value).

Best wishes for the final version, you’re almost there

Charles Amory