Reply to RC2

We would like to thank Charles Amory for taking the time to review our manuscript and for his positive feedback and useful comments. We respond point by point to the comments and provide the proposed changes to the manuscript in the text below. The referee comments are written in black, our response is written in blue, and the proposed changes to the original manuscript are highlighted in red.

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.

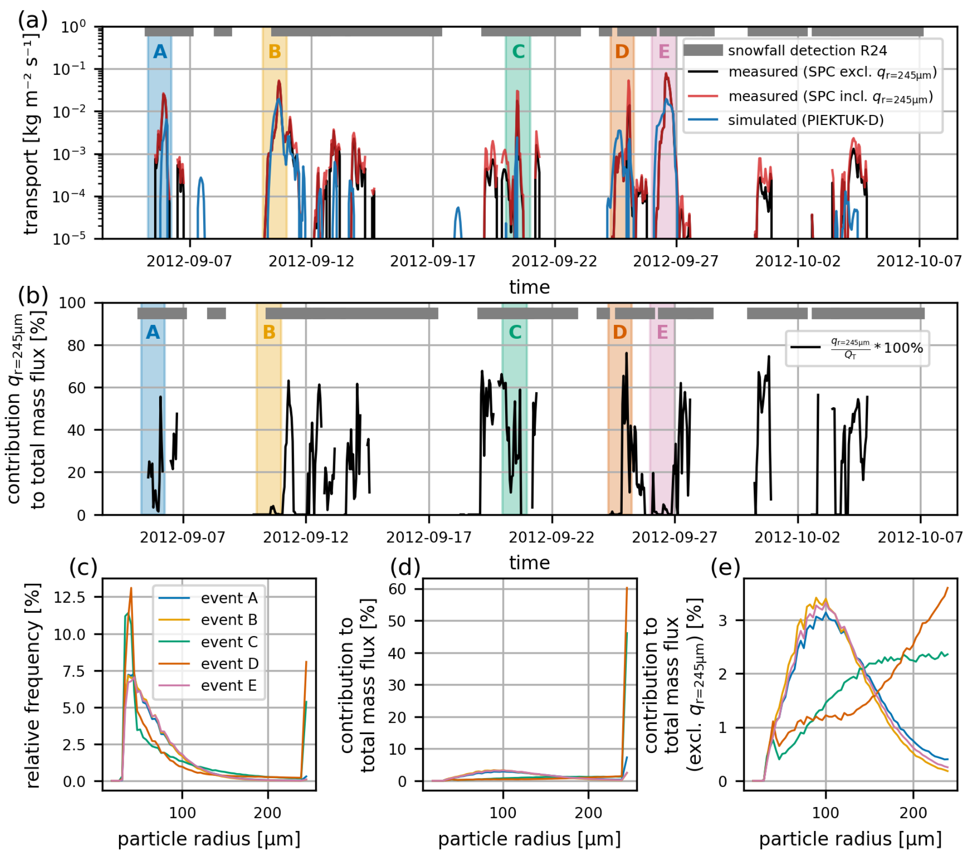
Thank you for pointing this out. We propose the following clarification:

Line 75:

Blowing snow occurs across the entire GrIS (Fig. 1a), with the highest frequencies (percentage of days with a daily mean blowing snow transport $Q_T > 10^{-3}$ kg m^{-1} s^{-1}) near the ice sheet (...)

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.

Thank you for your question. The largest particle radius class is excluded from the total blowing snow transport flux of the Snow Particle Counter (SPC), which is plotted in Figure 3a in the manuscript. Including it in the total blowing snow transport flux would further increase the discrepancies between measurements and simulations. In the figure below, the contribution of the largest particle class to the total measured blowing snow mass flux is investigated, which will hopefully clarify why we chose to exclude the largest particle class from our analysis.



The measured snow particle size distribution during the five largest blowing snow events (panel c) is approximately gamma distributed, as would be expected from theory, except for the peak in the largest particle radius class (245 µm). Moreover, during blowing snow events with little influence of snowfall (A, B & E), the mean contribution of each particle radius class to the total mass flux generally decreases for particle radius classes above 100 µm (panels d & e), with the second-largest particle radius class (239 µm) contributing <1% to the total mass flux (excluding the largest particle class). The contribution of the largest particle radius class is an exception, especially during blowing snow events impacted by snowfall (C & D), where the contribution can reach 76% of the total mass flux (panel b). As we could not explain why this peak in the largest particles is observed, and because it makes a major contribution to the total blowing snow mass flux, while theory would expect its contribution to the total mass flux to be small (i.e., similar to the other large particle radius classes), we decided to exclude this particle class from our analysis. To clarify our motivation for excluding this particle class within the manuscript, we propose to change the following sentences:

Line 105:

(...) method of Naaim-Bouvet et al. (2014). The measured snow particle size distributions during the five largest blowing snow events are approximately gamma distributed, as expected from theory (Budd et al., 1966; Schmidt, 1981), except for an anomalous peak in the largest particle radius class (245 μm, not shown). This particle radius class therefore makes a major contribution to the total blowing snow mass transport flux (up to 76 %), whereas contributions from other large particle radius classes were generally below 1 % during blowing snow events without concurrent snowfall. As we could not identify an alternative physical explanation for this observed peak, we surmise that these large particles are precipitating snow and exclude this largest particle radius class from the analysis, following Sugiara et al. (1998).

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

Thank you. We will add the units.

Line 154:

(...) the bulk settling velocities weighted by the third and first moment respectively [m s^{-1}], (...)

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.

Thank you for your feedback. We propose to add the following sentences to summarise the method briefly:

Line 326:

The method assumes that the clear-sky (cloud fraction = 0) and fully overcast (cloud fraction = 1) conditions are represented by a second-order polynomial fitted to the 5th and 95th percentile levels of $LW_{\downarrow}$ plotted against 2 m temperature. Cloud fractions in between these lower and upper bounds are obtained by a linear interpolation between the fitted polynomials.

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, something like "Rapid surface snow compaction could reduce the availability of loose snow, thereby increasing u_*t". Similarly, could you specify what 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 elsewhere 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 hypotheses using specific observations of surface snow properties, focus on the 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?).

Thank you for your feedback. We agree that this paragraph would benefit from elaborating on the specific physical processes at play, a comparative discussion with the findings of Lenaerts et al. (2014), and concrete avenues for model improvement. Therefore, we propose to adapt the paragraph in the following way:

Line 342:

The underestimation of simulated peak horizontal blowing snow transport fluxes by PIEKTUK-D during summer is also reported by Gadde and Van De Berg (2024), who evaluate the same model using data from acoustic blowing snow sensors over Antarctica (Amory, 2020). Note that the difference between the horizontal blowing snow transport fluxes of the SPC at S10 in this study and those reported by Lenaerts et al. (2014) could not be explained. While this underestimation can be partially explained by the underestimation of horizontal wind speeds at S10, it could also be related to an underestimated availability of loose surface snow due to inaccuracies in the surface snow compaction model (Gadde and Van De Berg, 2024). For example, overly rapid surface snow compaction in summer could reduce the availability of loose snow and consequently increase the threshold friction velocity (Gallée et al., 2001). Also, R24 does not resolve direct interactions between precipitation and blowing snow, such as precipitating snow acting as an additional source of blowing snow before it reaches the ground (Gadde and Van De Berg, 2024). The modelled blowing snow transport flux is highly sensitive to the surface snow density and particle size distribution within the saltation layer (Lenaerts et al., 2024; Gadde and Van De Berg, 2024). Therefore, future research should focus on improving the spatiotemporal variability of the modelled characteristics of surface snow in R24, such as density, dendricity, and sphericity, and on constraining the particle size distribution across Greenland using in situ measurements. Additional observations, both at other locations and over longer time periods, are required to evaluate PIEKTUK-D's ability to capture the spatial and seasonal dynamics of blowing snow transport and sublimation over the GrIS.

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.

Thank you for your suggestion. We propose to add the following sentence discussing the potential causes of the residual bias in downwelling longwave radiation in R24:

Line 352

Including the radiative effects of blowing snow reduces biases in downwelling longwave radiation in R24, particularly under clear-sky conditions, as well as in other RCMs (Hofer et al., 2021; Le Toumelin et al., 2021). Part of the residual bias in R24 might be related to deficiencies in cloud representation, such as an underestimated occurrence of thin, high-altitude clouds, an underestimation of cloud mixing ratios, and biases in cloud phase partitioning (Feenstra et al., 2026). The underestimation of downwelling shortwave fluxes (...)

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?

Thank you for your feedback. In the case study (Section 3.2.1), we observed that the direct impact of blowing snow on the surface radiation budget is most pronounced during clear-sky conditions at S10 when $SW_{\downarrow}$ is large, and atmospheric emissivity is near its lower limit, allowing blowing snow to both substantially lower $SW_{\downarrow}$ and increase $LW_{\downarrow}$. However, interactions between blowing snow sublimation and low-level clouds, like moisture release due to sublimation, could indeed lead to secondary radiative effects of blowing snow in coastal regions. Therefore, we propose to add the following sentence:

Line 376:

In coastal Greenland, the direct radiative impacts of blowing snow will likely be less pronounced due to more frequent cloudy conditions. Secondary radiative effects could potentially be amplified due to the interactions between blowing snow and (low-level) clouds discussed above.

Typos

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

Thank you for finding this inconsistency. We believe that Figure 2 should be displayed in the method section and Figure 3 should be displayed in the results section based on the content of the figures. Therefore, we propose deleting the references to Figure 3 in Section 2.1.1 to avoid confusion, as the references are not essential for the clarity of the argumentation in the text here.

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

Thank you for finding this typo. We will change this accordingly in the revised manuscript.