

# REVIEW COMMENTS

This manuscript investigates the role of sea salt aerosol (SSA) emissions from blowing snow and their impact on aerosol properties, cloud condensation nuclei (CCN), and radiative forcing in polar regions. The authors implement a blowing snow SSA parameterisation in both the TM5 chemical transport model and the EC-Earth3 Earth system model, and evaluate the results against MOSAiC observations and Arctic ground stations. The study is timely and relevant, as blowing snow has emerged as an important yet still uncertain aerosol source in polar environments. The combination of CTM simulations, climate model experiments, and observational comparisons represents a strong modelling framework, and the manuscript aligns well with the scope of ACP.

The results indicate that including blowing snow enhances SSA concentrations, improves the seasonal cycle, and increases CCN, with associated impacts on radiative fluxes. However, several aspects of the study require further clarification and strengthening. In particular, the treatment of uncertainties in the blowing snow parameterisation is limited, the improvement in model–observation agreement is marginal, and key aspects of aerosol microphysics and radiative forcing interpretation are not fully resolved. Some conclusions, especially regarding temperature responses, are not sufficiently supported by the analysis.

I recommend this manuscript for publication in ACP after major revisions addressing the comments below.

## Major Comments

### 1. Abstract

The abstract clearly outlines the study’s scope and highlights the importance of blowing snow as a source of sea salt aerosols in polar regions. However, several aspects could be improved to better reflect the strength and limitations of the results. The abstract does not explain why it is important to quantify the role of sea salt aerosols from blowing snow.

First, the abstract is somewhat method-focused, with detailed descriptions of model configurations that could be streamlined in favor of emphasizing the key scientific findings. Second, the statement that blowing snow emissions lead to “improved agreement with observations” appears overstated, given that the reported improvement is small and not statistically significant. This should be phrased more cautiously.

The description of radiative forcing is simplified and does not adequately reflect the complexity of the results, particularly the differing shortwave and longwave responses and regional contrasts between the Arctic and Antarctic. Finally, the concluding statement that blowing snow emissions are “essential” for realistic model representation is too strong given the uncertainties in the parameterisation and the modest improvements in model performance.

### 2. Introduction

The introduction is well-structured and clearly justifies the study's relevance, effectively bridging the gap between recent observational breakthroughs and current modeling limitations.

While it mentions that SSA impacts CCN, it could be more explicit about why blowing snow is physically different from standard sea spray (e.g., its dominance during winter/spring when open water is scarce).

The transition from discussing "Arctic amplification" to "Antarctic" enhancements is a bit abrupt. Ensuring the global scope is established even earlier might help, as the results later show the Antarctic actually has the more "pronounced" sea salt fluxes.

### **3. Methods**

The Methods section is technically sound but needs more transparency regarding assumptions and sensitivity.

The paper notes that the blowing snow parameterization is "most sensitive to changes in relative humidity". More detail on how well-represented RH is in the ERA5 and EC-Earth3 datasets over sea ice is required, as any bias there will propagate directly into the SSA results. The authors should clarify whether a sensitivity test was performed to see how a change in humidity affects the total SSA flux.

The authors should clarify critical wind speed thresholds for blowing snow verified against the MOSAiC observational data.

The authors should explain why the Yang et al. (2019) parameterization was chosen over more recent alternatives, and explain any known limitations regarding particle size distribution.

The model uses fixed snow salinity per size bin. In reality, snow salinity on sea ice is highly variable. The authors should justify why a fixed value is sufficient or discuss the potential error this introduces.

The study assumes a set number of SSA particles are produced per sublimating snow particle. This is a major source of uncertainty that needs a more thorough sensitivity discussion in the Results section.

There is a notable difference in resolution between the atmospheric model and the TM5 component. The authors should clarify if this "coarser" chemistry resolution captures the "localized" nature of Arctic emissions mentioned in Section 3.

### **4. Results**

While the inclusion of blowing snow captures "observed sea salt aerosol peaks" better than the baseline, the correlation increase is marginal (0.37 to 0.39) and statistically insignificant. The authors should explicitly discuss why the model still underestimates or misses certain events—is it due to the resolution being too coarse for coastal topography, or errors in the ERA5 wind fields?

There is a significant conflict between the TM5 and GEOS-Chem models. In TM5, adding blowing snow decreases Aitken mode particles (due to coagulation), while in GEOS-Chem, they increase. The authors attribute this to "aggressive transfer" in TM5. The authors should discuss which response is more consistent with available observations and how this model dependence affects confidence in the CCN and CDNC conclusions. If the Aitken mode response is model-dependent, the resulting CCN and cloud droplet number concentrations (CDNC) estimates are highly uncertain.

The most controversial finding is that EC-Earth3 shows Arctic cooling, whereas previous literature shows warming. The authors suspect this is "internal variability" but do not prove it. Ideally, this would be addressed with a larger ensemble or longer simulations; if this is not feasible, the authors should substantially strengthen the caveat and avoid interpreting the temperature response as a robust forced signal. Does the cooling stem from the Direct Radiative Effect (DRE) of sea salt or an overestimation of the Twomey effect (cloud brightening)? The text mentions a drier lower troposphere in the experiment; this dehydration effect needs to be quantified as it could be a major driver of the cooling.

The results show that Antarctic SSA enhancements are much more "pronounced" and "zonally uniform" than in the Arctic. The authors should discuss if this is purely due to the greater expanse of sea ice and higher wind speeds in the Southern Ocean, or if the model's fixed snow salinity assumption is more representative of the Antarctic than the Arctic.

## 5. Conclusions

The results showed a surprising regional cooling in the Arctic, yet the net radiative flux was positive. The authors should mention this cooling in the conclusion but add a caveat about internal climate variability.

The authors mention that including these emissions is "essential" in the abstract, but they don't explain how this changes our understanding of Arctic Amplification or future climate sensitivity in the conclusion.

The authors mention that TM5 and GEOS-Chem show different responses in the Aitken mode. A stronger conclusion would suggest which model representation (aggressive vs. less aggressive coagulation) likely reflects reality better based on the MOSAiC observations.

## 6. Figures

**Figure 1:** The caption should be more specific. Please state the averaging period, the model experiment, the flux units, and whether panels c–d show percentage changes in total particle number or only sea-salt aerosol particles. The color scale should also be clarified, since the text reports maximum Antarctic fluxes larger than the apparent colorbar range. Please also improve the unit formatting for panels a–b.

**Figure 2:** The caption and legend should be made consistent. The black curve is described as the "estimated number of particles added", whereas the axis suggests a percentage change. Please clarify whether it represents an absolute concentration difference or a relative increase. The title and y-axis label should also clearly state the plotted quantity, particle size range, and concentration units. Please correct the misspelling "consentration" to "concentration".

**Figure 3:** The caption should specify that CCN are shown at 0.3% supersaturation and avoid the term "sea salt CCN", since the plotted quantity appears to be total CCN concentration influenced by sea-salt emissions. Please also state the temporal resolution of the observations and model output, define how the grey-shaded blowing-snow events were identified, specify the spatial range, and improve the concentration unit formatting.

**Figure 4:** The caption needs clarification. The text refers to monthly median size distributions, whereas the caption states monthly mean distributions; these should be made consistent. The caption should also define the green shaded observational range and explain the use of different y-axes for TM5 versus observations/GEOS-Chem. Please use consistent terminology for GEOS/GEOS-Chem throughout.

**Figure 5:** The caption should state the observational period, whether model values are sampled at the nearest grid point, and whether the reported mass is total sea salt or sodium-equivalent

sea salt. Please also provide the latitude and longitude of each station and adjust the x-axis tick labels, as months 11 and 12 overlap.

**Figure 6:** The vertical coordinate “lev” is not sufficiently informative. Please use pressure or altitude, or clearly define the model levels in the caption. The caption should also state that differences are calculated as “with blowing snow minus baseline” and indicate whether FDR correction was applied to the significance masking. Please improve the formatting of the colorbar units.

**Figure 7:** The caption is too brief. Please state that the figure shows annual zonal-mean differences in CCN at 0.3% supersaturation, in  $\text{cm}^{-3}$ , calculated as “with blowing snow minus baseline”. As in Figure 6, the vertical coordinate should be converted to pressure or altitude, or clearly defined. Please also improve the colorbar unit formatting and make the x-axis tick labels more publication-ready, for example by including latitude units/degrees.

**Figure 8:** The sign convention should be stated explicitly. Please clarify whether positive values indicate increased downward surface flux and whether all curves show “with blowing snow minus baseline”. The x-axes should be labelled with appropriate units, and the y-axes should include both the variable name and units, not only the units. The caption should also explain how statistical significance is shown. Please correct “new downward solar” to “net downward solar”.

**Figure 9:** The caption should specify that the plotted fields are zonal-mean differences between the blowing-snow and baseline transient EC-Earth3 simulations. Please state the averaging interval, sign convention, and how statistical significance is indicated. Given the apparent role of internal variability, please also clarify whether these are single-realization differences or ensemble-mean responses. The y-axis labelling should be made more publication-ready.

## Minor Comments

**Line 4:** Please define the abbreviation AMIP at first use.

**Lines 26–29:** This sentence could be improved for clarity. Suggested wording: “Sea salt aerosols (SSA) are an important source of cloud condensation nuclei (CCN) in the polar regions. Model estimates suggest that SSA account for more than one quarter of Arctic aerosol number concentrations. As Arctic sea ice declines, increased open-ocean area and more frequent sea-ice leads are expected to enhance SSA emissions, thereby increasing Arctic CCN concentrations (Emme and Horowitz, 2025).”

**Line 30:** Please add a space between “ice” and the citation “(Lapere et al., 2024)”.

**Lines 31–32:** Please briefly explain why blowing-snow SSA emissions are especially important during winter and spring.

**Lines 99–100:** The statement that each M7 mode is further divided into soluble and insoluble fractions may be misleading, since the nucleation mode does not have an insoluble counterpart. Please revise this sentence for accuracy.

**Line 227:** The phrase “most important finding” is subjective and should be avoided. Consider using more neutral wording, such as “A notable finding” or “One key result”.