# Comment on “Sea Salt Aerosols from Blowing Snow: Contributions to Radiative Forcing” by Nordling et al.

This manuscript presents blowing snow’s contribution to aerosol, cloud condensation nuclei, and radiative forcing in polar regions based on a modelling study. The paper addresses an important gap in polar aerosol-climate feedback. However, the contribution to the scientific community is limited by the lack of comparison with existing studies and a failure to discuss the broader implications of the findings. I suggest a major revision before publishing on ACP.

Major Comments

1. The blowing snow mechanism is not fully clear yet. For example, the salinity, how many ice crystals are produced for each snowflake, sublimation rate under sub-/super-saturations are not clear yet. This paper lacks such information for the model simulation. More importantly, it would be even better to run the sensitivity test for each parameter.
2. The current results section is primarily descriptive (i.e., simply stating what is in the figures) rather than an in-depth analysis. The authors fail to place their findings in the context of existing literature. For instance, the simulated SSA AOD and its contribution to the total aerosol load should be compared with previous studies or satellite/AERONET observations. Without this, it is difficult to gauge the reliability of the parameterization.
3. Long-term blowing snow simulations lead to cooling at both poles, yet the net flux values provided are positive (Lines 203-204). This paradox requires a detailed explanation. Is this driven by the strong negative forcing during the summer compared to the winter (Fig. S2 & S3)? The efficiency of blowing-snow-SSA production in summer versus winter needs to be explicitly shown to support these conclusions. Furthermore, the lack of radiation simulation results for the Antarctic—where the impact is claimed to be more significant—is a major omission.
4. The figures require significant aesthetic and functional improvements. Some panels are cluttered (see Specific Comment Fig. 2), making them nearly unreadable. Data should be presented in a more intuitive manner (e.g., using dual axes or improved colorbar position) to help the reader distinguish between different variables and modes.
5. The manuscript concludes abruptly without discussing the broader implications of the work. How do these findings affect our understanding of polar climate sensitivity? What are the limitations of the current TM5 implementation, and what should be the focus of future research?

Specific Comments

Line 38: When mentioning specific CMIP6 models, please provide appropriate citations for the models discussed.

Line 53: Please clarify if the “2019-2020” period applies to both study locations or if there was a time distinction between the Arctic and Antarctic datasets.

Line 131: The correlation improvement here is relatively low. Please elaborate on the physical or numerical reasons behind this discrepancy, or can we only show the correlation during the blowing snow events?

Line 143 & Figure 4: The description and the figure show a different conclusion: as the figure shows, TM5 exhibits a slight decrease in Aitken mode particles when considering blowing snow instead of increasing. The following statement of results also indicates a decrease.

Figure 1:

The unit in the legend should use proper superscripts.

Add the specific locations of the three in-situ observation sites and the corresponding TM5 grid cells in Fig.1 Panel (a).

Standardize panel labels description as (a), (b), (c).

The colorbar is currently too large.

Figures 2 & 3: The readability is low.

Please specify the time resolution of the data.

Suggestion: Use a dual x-axis, moving the “added particle” data to the top x-axis to reduce overlap.

Figure 7: It should be “0.3% SS” instead of “0.3 supersaturation”. Additionally, the colorbar scale should be adjusted to clearly highlight the differences in CCN enhancement capabilities between the North and South Poles.