Dear Reviewer,

We would like to express our sincere gratitude to you for dedicating your time and effort to provide valuable comments. In this document, we have addressed your comments point by point. Your original comments are presented in *blue italic*, while our responses are in regular black text.

Based on a comprehensive consideration of the comments from the three reviewers, we have made the following major revisions to the manuscript:

1. Significantly condensed the introduction and conclusion;
2. Appropriately adjusted the structure of the manuscript to highlight the theme;
3. Added discussions on the impact of blowing snow on wind speed;
4. Corrected the drawing errors of the blowing snow cross-sections and unified the drawing style for blowing snow.

Major comments:

My major comment is that while the authors mention that the focus of the paper is on the blowing snow, the paper is actually more about the cyclonic event but rather blowing snow feels like a small part of the paper.

1. *Discussion section reads like a repetition of the results section which is already quite extensive.*

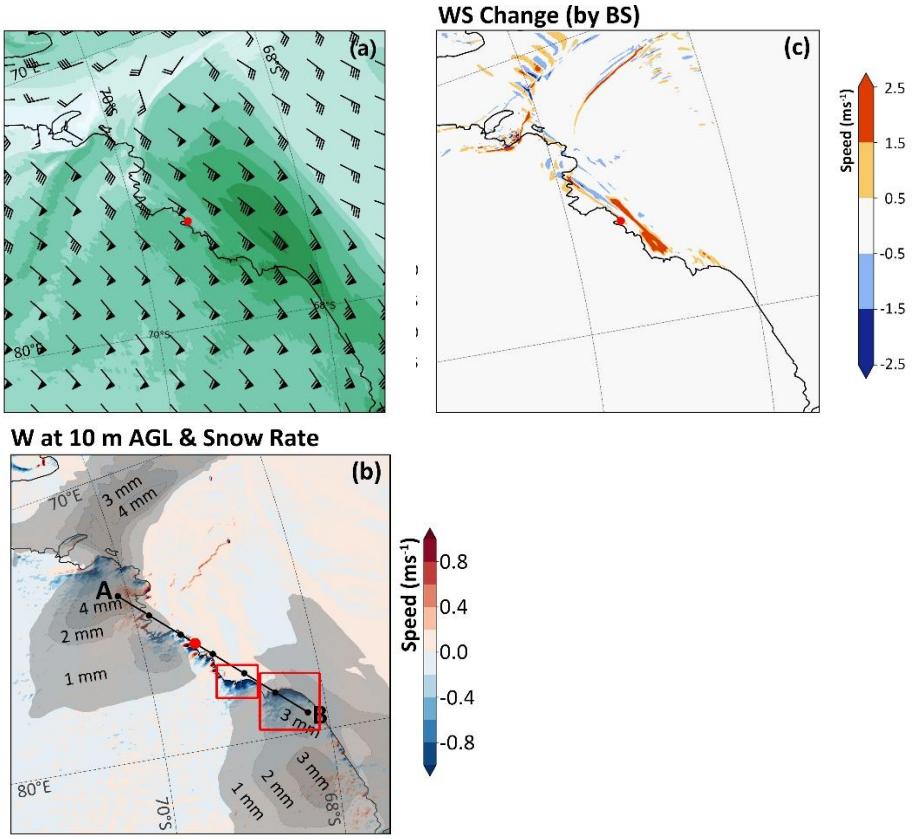
   Re: We have reorganized this article to enhance its logical structure, focusing on the impacts of topographic and snowpack characteristics on blowing snow, condensing the description of weather processes, and substantially streamlining the introduction and discussion sections. Thanks for your suggestion.

2. *The authors only briefly touch upon different sensitivity studies (with and without blowing snow) (lines 660 – 711), but do not discuss in detail what is the effect on local meteorology. What would actually make the paper more interesting and more relevant to understanding blowing snow, and also increase the value of the paper is if the authors focused more on the effect of blowing snow on katabatic wind, previous studies (e.g. Kodama et al. (1985)) indicate that blowing snow increases the Katabatic force. With previous RCM simulations it has been difficult to resolve this phenomenon, with the current setup used by the authors it is possible to have a look at the effect of blowing snow on the katabatic force and other meteorological variables. Adding this analysis would actually improve the quality of the paper and also allows us to generalize some ideas.*

   Re: Your suggestions are immensely valuable. We have also studied Kodama's paper and agree that this phenomenon is well worth exploring, even though there have been relatively few relevant studies in this field in recent years. According to Kodama's conclusions, when the wind speed exceeds a specific threshold, the acceleration of katabatic winds significantly outpaces the increase in katabatic force (KF_a) calculated without considering blowing snow. This

indicates that blowing snow provides an additional driving force for katabatic winds.

We conducted a set of comparative experiments and present here the wind speed simulation results at 18:00 UTC on July 15, 2022 (when blowing snow was most intense). The left panel shows the control experiment, which incorporates the wind field of blowing snow; the right panel displays the difference between the control experiment and the sensitivity experiment without considering blowing snow, i.e., it reflects the impact of blowing snow on wind speed (only wind speed distribution is presented, as wind direction is barely affected). It can be observed that downstream of the intense downward motion (marked with a red box on the vertical velocity distribution map), wind speed is indeed significantly enhanced, which appears to validate Kodma's perspective.



Upon examining the model's source code, we found that the existing CRYOWRF model already includes the blowing snow particle mixing ratio (q_{bs}) as a hydrometeor component, which participates in the calculation of fluid density (specific volume). The calculation formula is as follows: $\alpha = \alpha_d(1 + q_v + q_c + q_r + q_i + q_{bs})^{-1}$, where $\alpha_d = (1/\rho_d)$ represents the specific volume of dry air. This specific volume is integrated into the entire dynamic-thermodynamic framework of the model for calculations. Therefore, the incorporation of blowing snow q_{bs} obviously exerts an impact on the wind speed simulation results. However, we believe the conclusions of this sensitivity

experiment may not be reliable, for the following reasons:

(1) Kodama's study focused on an idealized scenario where katabatic winds induce blowing snow. Its core consideration was the katabatic force caused by the near-surface stratification, without accounting for horizontal gradient forces from cyclones or other weather systems. This differs significantly from the scenario in our current study.

(2) The inclusion of blowing snow in the model not only changes density but also triggers processes such as phase transitions; Meanwhile, due to the model's long-term integration, cumulative effects may also arise from the continuous amplification of initial discrepancies. Therefore, even if our sensitivity experiment showed that wind speed increased in some areas when q_{bs} was considered, we cannot conclude that this increase was caused only by the density change from blowing snow. This makes it difficult to identify which process is the direct driver of the observed changes.

Considering the above factors, we believe that using idealized simulations to isolate the interactions between different factors would be a more effective approach in future research. We have included these results and discussion in the current manuscript. Thank you again for your valuable suggestions.

3. *The paper in the current state is too long and the sensitivity studies with respect to blowing snow is overlooked which could add important insights and of possible value to the community.*

   Re: In accordance with your comments, we have made certain adjustments to the article structure to highlight the theme of blowing snow. Thanks.

Minor comments:

1. *Line 47 – It profoundly affects almost every 'link' in the Antarctic cryosphere? What do you mean by 'link'?*

   Re: Thank you for pointing out the ambiguity in the semantics here, we have deleted the original sentence to streamline the expression.

2. *Line 66 – Which study? Rephrase this sentence.*

   Re: The sentence is rephrased as "Using ground observations, satellite data, and Weather Research and Forecasting (WRF) simulations, Vignon et al. (2020) found that katabatic winds crossing abrupt topographic transitions can trigger gravity waves, lifting snow to ~1 km and forming a blowing snow wall."

3. *Introduction could be shortened and more streamlined.*

   Re: Done. Thanks.

4. *Line 186: What is etc? Please mention all the variables relevant to the paper*

that are measured.

Re: Thank you for your rigor. This study mainly uses wind (including wind speed and direction), temperature, and air pressure data from automatic weather stations; other variables (including humidity and instrument parameters) are not used, so "etc" in the original text has been deleted.

5. *Line 181: Which automatic weather station is this? What are the specifications? Please include some details.*

   Re: We have added some details: "The AWS is included in the World Meteorological Organization (WMO)'s Antarctic Basic Synoptic Network (ABSN) and Antarctic Basic Climatological Network (ABCN), and also integrated into the Global Climate Observing System Surface Network (GSN); it has an international meteorological station number of 89573 and is operationally maintained by the China Meteorological Administration."

6. *Line 211: Please add appropriate reference*

   Re: We have added the reference, "The CRYOWRF model (Sharma et al., 2023) is …". Thanks.

7. *Line 231: Typically WRF nested domain grid ratios are either 3 or 5. So in this case it should have been: 12, 4, and 1.3 km. Is there any effect of choice of 1km on the results?*

   Re: We recognize the standard WRF guidance recommending odd-integer ratios (3:1 or 5:1) for nested domains to ensure optimal two-way feedback and minimize numerical damping and interpolation errors. On the one hand, we aim for the higher resolution in the innermost domain to allow more detailed topographic representation. On the other hand, since we did not enable inter-domain feedback (feedback=0) in the CRYOWRF configuration, the impact of the 4:1 ratio on simulation stability may be insignificant. Furthermore, we have actually used this configuration to simulate several other cases, and it has proven to be relatively stable. Therefore, we have retained this configuration, which is not strictly in line with the recommended guidelines. Thanks a lot for your suggestion.

8. *Fig 2: Latitude and Longitude replot them in white color so they are visible. It is very difficult to find the coordinates with the current figure*

   Re: Thanks for your suggestions. Figure 2 has been replotted by using the black color for latitude and longitude in the revised version, and we also tired the white color firstly which might be similar to the coastlines, so we finally choose the black one.

9. *Fig 2: Please include a separator between date and time to avoid confusion*

Re: Done. Thanks.

10. *Line 307: Please include statistical values when mentioning 'good agreement' with the observations, a visual match is not enough. From Figure 3(a) it seems the observed windspeed during the peak of the event could be more than 10 m/s compared to simulations*

    Re: Done. We have calculated the BIAS between the simulations and observations. Thanks.

11. *Line 375: Fig 5d*

    Re: Done. Thanks.