**Dear Reviewer#1,**

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.*

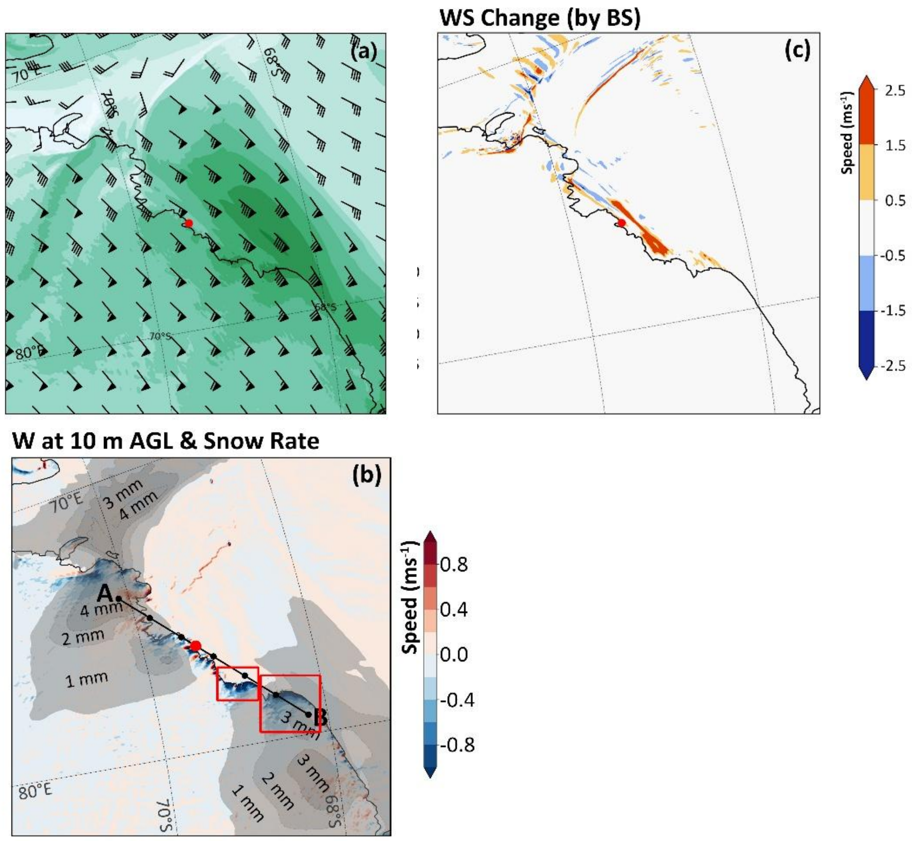
1. *Discussion section reads like a repetition of the results section which is already quite extensive.*

   **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.

**Dear Reviewer#2,**

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:**

*This paper presents a high-resolution numerical simulation of a severe blowing-snow event near Zhongshan Station, East Antarctica, using the CRYOWRF coupled atmosphere–snow model. The study aims to investigate the interactions between katabatic winds, cyclonic forcing, and snow transport processes, and compares model results with ground-based and satellite observations. The manuscript addresses an important topic for Antarctic meteorology and surface mass balance studies, and it uses a promising modelling framework. Several of the reported results, especially those concerning the sensitivity of local meteorological processes to the activation of the blowing snow module, are potentially very valuable. However, the manuscript in its current form does not yet realize this potential.*

*Substantial work is required to (i) redefine and clarify the study objectives, (ii) streamline the presentation, and (iii) strengthen the scientific argumentation connecting the experiments to the broader knowledge gaps outlined in the introduction. The paper is overly long and sometimes loses focus through extensive, loosely connected descriptive passages. The introduction in particular reads as a dense compilation of facts, often lacking clear logical transitions or explicit linkage to the scientific questions addressed later. Many sentences remain vague or imprecise, weakening the overall clarity. A major review of redundant material would significantly improve readability. Despite these issues, the dataset and modelling framework are of clear scientific interest, and I would be pleased to read a revised version once these structural and conceptual improvements are made.*

**Re:** We would like to express our sincere gratitude for your highly constructive comments on this paper. In response to your feedback, we have optimized the overall structure of the paper and strengthened the research focus. Specifically, we have made substantial revisions to the introduction, refined the conclusions, deleted non-essential figures in the main text, and adjusted the structure to highlight the core theme of blowing snow. Through these revisions, we aim to enhance the focus and readability of the paper. Thank you again for your valuable insights.

**Specific comments:**

1. *L43–44: "Most direct compared to what?" The meaning of direct is unclear here. Please reformulate precisely. L45: "The main way for the redistribution of surface snow": it is indeed the main way, because it is also the only one. Please rephrase or qualify. L45–46: "Adjustment of surface mass balance": the term adjustment seems meaningless in this context.*

   **Re:** We have revised the above two sentences to: "Blowing snow is a very common phenomenon in Antarctica, which has a major impact on mass balance primarily through redistribution of snow and enhanced sublimation". Additionally, the use of "only way" may lead to ambiguity, as the redistribution of surface snow should also include surface melt and runoff, precipitation (accumulation), and ice dynamics (glacier flow and calving). Thanks for your suggestion.

2. *L47: Sentence too generic and vague, please remove.*

   **Re:** Done. Thanks.

3. *L58: "strong wind duration, etc.": "etc." should be deleted; the phrase as written is nonsensical.*

   **Re:** This sentence has been removed in the revised version of the introduction. Thanks.

4. *L59–61: Missing reference.*

   **Re:** This sentence has been adjusted in the revised version of the introduction, and the relevant references have been listed. Thanks.

5. *L67: Define the acronym WRF at first use and remove "etc."*

   **Re:** Done. Thanks.

6. *L66–70: Add an appropriate reference.*

   **Re:** Sorry, due to the excessive length of the original sentence, it might have led to the misunderstanding that no literature was cited. The original text has been condensed, with the cited literature being "Vignon et al., 2020".

7. *L104: "snow quality": define this expression or link it to a measurable physical property related to snow erodibility (e.g., snow cohesion).*

   **Re:** Done. The Sentence has been modified as: "Additionally, the physical properties (such as dendricity, density, sphericity, and particle sizes) of the snowpack surface layer undergo dynamic changes due to wind erosion and compaction, which in turn affects the accurate assessment of blowing snow flux (Lehning et al., 2000; Gallée et al., 2001; Clifton et al., 2006)". Thanks.

8. *L105–109; L112–114: Add supporting references.*
   **Re:** Done. The previous version was overly verbose, with one piece of work content split into two sentences, which made the citation appear missing, but it has now been revised. Thanks.

9. *Introductory structure: The introduction lists results and ideas densely, often without clear connection. It would benefit from restructuring with one explicit objective per section and by adjusting the level of detail to match the research question. Many ideas appear at once, making the text hard to follow.*
   **Re:** Thank you very much for your suggestion. We have made substantial revisions to the introduction to enhance its logical flow. Additionally, as the framework of the main text has also been adjusted, the introduction has been logically aligned with this revised main text structure. Thank you again.

10. *Referencing: References should appear immediately with the first sentence that cites their results.*
    **Re:** Done. Thanks

11. *L127: "surface mass balance budgets": redundant. Choose either balance or budget and use it consistently throughout the paper.*
    **Re:** Done. Thanks

12. *L128–129: Too vague. Specify which aspects remain uncertain and what knowledge gaps are being targeted. The discussion should also include previous modelling work on blowing snow to contextualize the study (e.g., Lenaerts and van den Broeke 2012; Gerber et al. 2023; Amory et al. 2021) and highlight the complementarity of the present approach.*
    **Re:** Done. Given the substantial revisions made to the introduction section of the manuscript, we have incorporated the references provided by you in the following positions to support the introduction. (1) Both Amory et al. (2021) and Lenaerts and van den Broeke (2012) noted that regional climate models may have uncertainties in simulations under rugged terrain, and we have used this as a basis for introducing the high-resolution non-hydrostatic CRYOWRF model adopted in this study, which can be found in Introduction. (2) The evaluation by Amory et al. (2021) also pointed out that blowing snow assessments are usually focused near the surface. This is relevant to our finding that blowing snow can be transported over long distances in this case study, so we have added relevant descriptions in the discussion section, located in Discussion. (3) Gerber et al. (2023) evaluated the accuracy of CRYOWRF and noted that there are uncertainties in the saltation parameterization scheme. We have included in Discussion.

13. *L130–133: The phrase "inaccurately characterizing" is ambiguous: by whom or by what? Albedo and thermal conductivity are physical properties, not processes. Clarify the logical link between these properties and the blowing-snow process introduced earlier. L133–139: Only a small fraction of CMIP6 ESMs implement multi-layer snow schemes; please nuance and rephrase accordingly.*

    **Re:** Given that the relevance of this part of the introduction to the current study is not particularly strong, we have removed the relevant statements and citations. Thanks.

**Dear Professor Michael Lehning,**

It is a great honor to receive your review comments. Without the dedication of you and your team to model development, we would hardly have the opportunity to use this new model for relevant research, and we would like to express our sincere gratitude here. In this document, we have addressed your comments point by point. Your original comments are presented in blue italics, 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:**

*The paper is a very detailed meteorological analysis of a sequence of an interesting weather event in East Antarctica, namely the propagation of a cyclone from the mid-latitudes. The paper is generally well written and the analysis easy to follow. Figures are of high quality. My main comment is on the descriptive nature of the presentation, which reads as a weather protocol rather than a journal paper. The main result section is too long and detailed and would profit from shortening. The discussion section is very good and an example of the style that could also be applied to the main result section to achieve conciseness. It is further suggested that the authors try to focus on some of the more novel observations such as the height of the blowing snow cloud in the diverse stages from the hydraulic jump to the passing of the cold front. It would also be interesting to analyse the total mass balance of snow during the event and name contributions from precipitation to transport and sublimation.*

*One major comment is that I did not understand how the authors distinguish between blowing snow and snowfall in their measurement data. Please clarify.*

**Re:** We have conducted comprehensive revisions to the entire manuscript, including streamlining the introduction, main text, and conclusions, adjusting the overall structure to highlight the theme of blowing snow, and addressing all issues or redundancies in response to the comments from all reviewers. We hope this revised version will offer a better reading experience.

Regarding your suggestion on “the height of the blowing snow cloud”, we have appropriately supplemented the description of the event process, as shown in Fig. 10. We have also considered this issue: the ground-based laser ceilometer we used cannot penetrate the blowing snow layer, so direct observation cannot provide information about its top height. Furthermore, since blowing snow and snowfall occurred simultaneously during the event, it is difficult to observe the top of the blowing snow even with Calipso data (obscured by clouds). According to the results from the numerical model, the top of the blowing snow cloud can reach 500 meters (transported horizontally from upstream). Theoretically, these particles can release more water vapor into the environment through sublimation (e.g., Luo et al., 2021 has discussed this issue). In addition, it is also a noteworthy question whether smaller blowing snow particles, when transported to higher altitudes, can further form condensation nuclei and thereby affect cloud formation. Relevant research is highly necessary, as the vast majority of existing studies have focused on surface saltation, while blowing snow particles in the air may also produce significant environmental effects. This problem must be solved through the integration of multiple observation methods. A potential approach we have considered is to use a multi-frequency millimeter-wave cloud radar to further distinguish the preferential deposition fraction and suspension fraction of snow particles in the air. However, the current observation site lacks the necessary equipment for such work, and we hope to have more opportunities to conduct in-depth relevant observation research in the future.

In addition, we have discussed the balance changes caused by snowfall and blowing snow in the new Fig. 14. Regarding the sublimation effect you mentioned, we believe it would introduce substantial uncertainties because it affects water vapor and thus snowfall. Moreover, the contribution of blowing snow to the mass balance was relatively small in this case, so we have not provided a separate description of it temporarily.

Thanks for your suggestions.

*Luo L , Zhang J , Hock R ,et al. Case Study of Blowing Snow Impacts on the Antarctic Peninsula Lower Atmosphere and Surface Simulated With a Snow/Ice Enhanced WRF Model[J].Journal of Geophysical Research: Atmospheres, 2021, 126.DOI:10.1029/2020JD033936.*

**Specific comments:**

1. *l. 47: Make the statement more specific*

   **Re:** This sentence has been deleted. Thanks.

2. *l. 66: "A study" please give the reference*

   **Re:** We regret that the original sentence was too long, leading to the misunderstanding that no literature was cited. The text has been abbreviated, with the corresponding reference being "Vignon et al., 2020".

3. *l. 98: "Another study" please give the reference*

   **Re:** This sentence has been deleted.

4. *l. 105: "A study" please give the reference*

   **Re:** Reference "Souverijnset al., 2018" has been added. Thanks.

5. *l. 112 – 114: This is very old and almost general knowledge, please reformulate and add references if needed*

**Re:** This sentence has been revised to "Additionally, the physical properties (such as dendricity, density, sphericity, and particle sizes) of the snowpack surface layer undergo dynamic changes due to wind erosion and compaction, which in turn affects the accurate assessment of blowing snow flux (Lehning et al., 2000; Gallée et al., 2001; Clifton et al., 2006)". Thanks.

6. *l. 139 – 145: Sentence too long, complicated and probably grammatically wrong*
**Re:** This sentence has been removed due to the adjustment of introduction.

7. *l. 183: Why "Prize" now?*
**Re:** Sorry for the mistake, it's should be Prydz Bay.

8. *l. 191: Analog OR digital?*
**Re:** It should be analog-digital. Thanks.

9. *l. 142: How did you initialize the snow for the SNOWPACK module? This is quite important as it may help to explain the high threshold friction velocities you find later.*
**Re:** We adopted the RACMO 2.3 data provided by CRYOWRF to initialize SNOWPACK, with its configuration consistent with Case Ia in Sharma et al. (2023). Relevant descriptions have been added to the model specification section: "The detailed configuration of the SNOWPACK model in this study strictly follows Case Ia as described in Sharma et al. (2023). The simulation was initiated at 12:00 UTC on 14 July 2022, running for 60 hours".
Regarding the issue of high threshold friction velocities, please refer to the response in reply 20.

10. *l. 294 – 295: Would give the wind speed range in numbers.*
**Re:** Done. "…and there was also a gentle breeze (wind speed < 5 m/s) that persisted for several hours, dominated by easterly to east-southeasterly winds with obvious directional oscillation (90–110 degrees)". Thanks.

11. *l. 322 ff: Shouldn't blowing snow almost always have a distribution with maximal values close to the ground, if the wind erodes particles or under sustained saltation, the maximum concentrations are found close to the ground and if deposition dominates, it also reaches the ground. Please see also a glossary type of definitions in the appendix.*
**Re:** We fully agree with your comment; our original description was insufficiently precise. We have revised this sentence to: "About an hour later, the bottom of the high-value zone of the backward scattering signal quickly transitioned to ground contact, indicating the onset of the blowing snow". Additionally, we would like to express our sincere gratitude again for the terminologies provided in your reference

appendix.

*l. 373 ff: Can you discuss the role of grid resolution in potentially explaining discrepancies?*

**Re:** We supplemented the wind speed simulation results (Model d01) in Figure 3a and found that model resolution indeed exerts a significant impact on wind speed. For instance, in the coarse-resolution grid, the calm wind conditions caused by hydraulic jumps are difficult to simulate, which confirms the importance of adopting a non-hydrostatic model for simulating local circulations. We have incorporated relevant discussions into the manuscript, thanks for your suggestions.

12. *l. 414: Specify the moment*

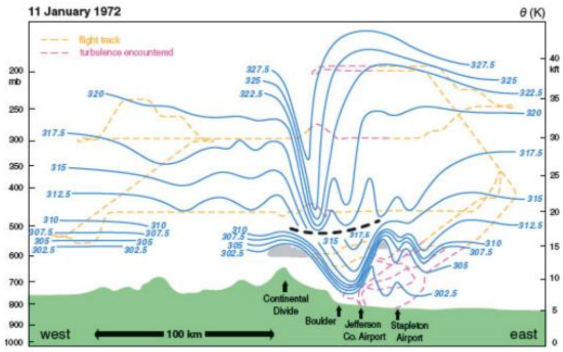
    **Re:** Done, as well as the other two similar places.

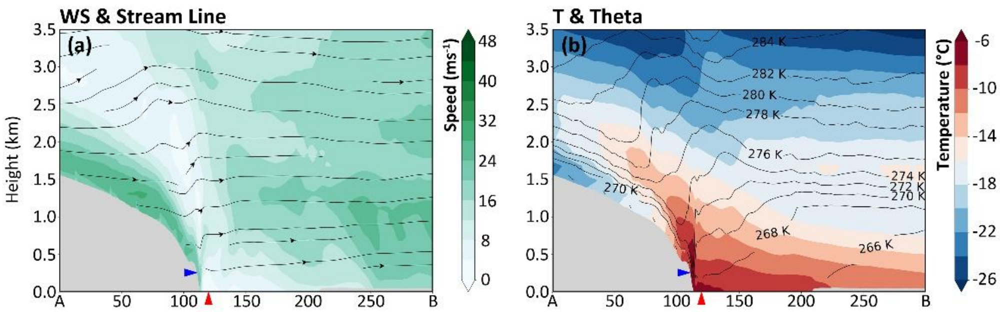
13. *l. 437: The fine-scale structure of BS mixing ratio in Fig. 7d looks suspicious. It almost appears that y and x axis are exchanged. Otherwise, the repeated pattern of high BS declining with distance from left to right and then jumping again to a high value is not realistic. It may simply be a problem of the plotting but needs to be looked into to make sure this is not a model problem*

    **Re:** After verification, this issue was caused by a plotting program error. We have re-plotted Figures 6d, 8d, and 11d, and we sincerely appreciate your reminder.

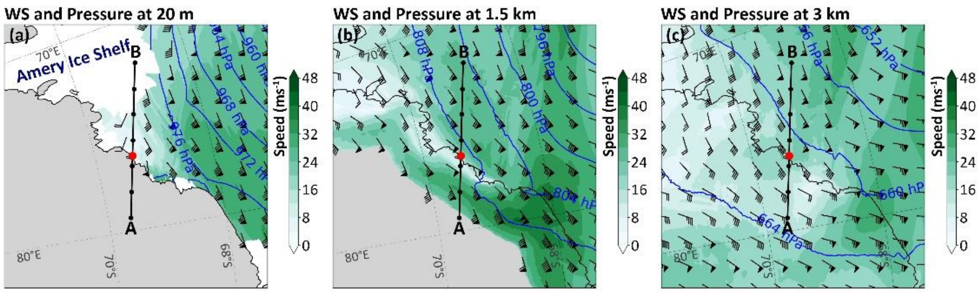
14. *l. 472 ff: It is true that turbulence dissipates energy but the primary reason for low wind speed should be the pressure gradient across the hydraulic jump with the turbulence then a consequence of high shear and accelerations, right?*

    **Re:** Due to the streamlining of the manuscript, the relevant descriptions have been removed, but I would still like to provide a further explanation regarding your question. In a classic hydraulic jump phenomenon caused by downslope winds (as illustrated in the figure below; Klemp and Lilly, 1975), when the airflow transitions from subcritical flow (typically characterized by a Froude number $Fr < 1$) to supercritical flow ($Fr > 1$), the wind speed on the leeward slope continues to increase, the airflow thickness decreases continuously along the entire path, ultimately resulting in a hydraulic jump (By further reducing the velocity, the thickness of the air flow is increased). Such phenomenon usually occurs under conditions such as strong airflow, an angle greater than approximately 60 degrees between the airflow and the mountain ridge, and a steep leeward slope. In our study, the airflow along the profile exhibits the similar characteristics.




This process is not necessarily driven by the reverse pressure gradient force. In fact, due to fluid continuity, the acceleration and high compression of the fluid in the hydraulic jump zone may instead lead to changes in the horizontal pressure gradient. However, in this case, the intense pressure gradient between the plateau and the cyclone remained the dominant factor shaping the pressure field when the hydraulic jump occurred (blue lines on the following figure).




Some studies (e.g., Yu and Cai, 2006) have specifically examined the hydraulic jump phenomenon in Antarctica. They found that the pressure-gradient force ahead of the air flow, associated with the cold air pool, facilitates the initiation of hydraulic jumps (represented by the conceptual figure below). However, there is a substantial discrepancy in scale between this study (with a horizontal scale of less than 10 km) and the present research — the topographic gradient in the former is significantly gentler than that in the latter. Nevertheless, this also indicates that the environmental pressure gradient can influence the occurrence of hydraulic jumps.

Essentially, hydraulic jumps are still characterized by extreme turbulence, resulting in significant energy dissipation.

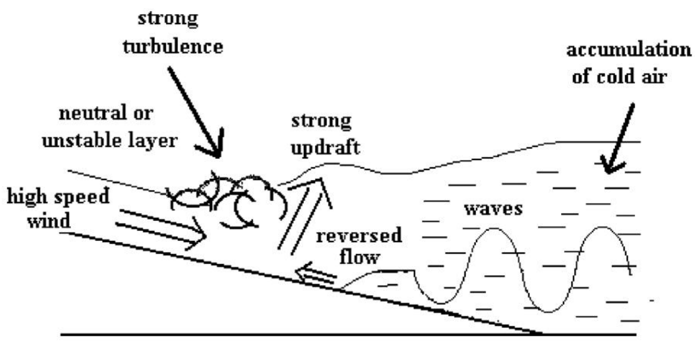



Figure Conceptual model of a katabatic flow jump based on NWP (Yu and Cai, 2006)

*Klemp, J. B., and D. R. Lilly, 1975: The Dynamics of Wave-Induced Downslope Winds. J. Atmos. Sci., 32, 320–339*

*Yu, Y., Cai, XM. Structure and Dynamics of Katabatic Flow Jumps: Idealised Simulations. Boundary-Layer Meteorol 118, 527–555 (2006).*

15. *l. 539: Maybe better to say "erosion" here instead of "saltation"?*

    **Re:** Done. Thanks.

16. *l. 553: Not clear, do you mean the saltation parameterization?*

    **Re:** These sentences are changed to "For a single station, the simulated blowing snow mixing ratio mainly originates from sub-grid blowing snow parameterization processes (primarily snow particle saltation, sedimentation, diffusion, turbulent mixing, and phase change) and grid-scale processes (including horizontal and vertical advection). In this case, the positive contribution of parameterization is mainly in the surface layer approximately 50-60 meters above the ground (Fig. 10b), which is directly linked to the SNOWPACK model's real-time saltation flux output……". Here, we further illustrate that the results in Fig. 10 are derived from the parameterization scheme, and the external source of the blowing snow mixing ratio ($q_{bs}$) in this scheme is the saltation flux output by the SNOWPACK model.

17. *l. 577: Figure 12a: How can it be that the first layer above ground has significantly lower BS over the whole profile? This is not realistic*

    **Re:** We sincerely appreciate your careful review. Similar to the 13th comment, we encountered a minor issue with the plotting, which has now been resolved by re-drawing the figures using contour fill plots. As you pointed out, the blowing snow mixing ratio reaches its maximum near the surface layer. Compared with the previous version, the main conclusions remain unaffected, and corresponding revisions have been made in the main text. Thank you again for your patience.

18. *l. 601: Do you really mean rain or just precipitation, please clarify*

    **Re:** It's precipitation, thanks a lot.

19. *l. 618: Specify the moment*

**Re:** Done.

*l. 640: Consider introducing a new sub-title as this section includes results from all stages*

**Re:** Your comments are very insightful. We have separately included this part in Section 4.2: Impact of snow layer properties on blowing snow considering simulation-observation discrepancy. Thanks a lot.

20. *l. 697: These threshold wind speeds are unrealistically high. Can you explore whether they have to do with your snow initialization or find out why they are so high otherwise?*

**Re:** We sincerely apologize for using incorrect symbols, which has led to your misunderstanding. Our calculation process is as follows: first, we calculate the critical friction velocity $u_{*,t}$, and then invert it to the threshold wind speed at 10 m height ($u_{th}$) using the logarithmic wind profile. What we plotted on the original figure is the $u_{th}$, but we incorrectly labeled it as $u_{*}$, resulting in values that are far beyond common sense (we checked the program and found that the friction velocity in the control experiment is approximately 0.78 $m \cdot s^{-1}$). The symbols on the figure and in-text descriptions have now been corrected. Thanks a lot.

21. *l. 732: Nice figure, which I would place much earlier in the paper to introduce the three stages*

**Re:** Thank you for your affirmation. We have moved this figure forward to Section 3 (the chapter introducing the synoptic background) and added introductory statements in the main text.

22. *l. 736: Can you also comment on the direction of the katabatic wind since in East Antarctica katabatic winds are typically also influenced by Coriolis because of their long running distances*

**Re:** We mention it in the discussion: under the influence of intense weather systems, both the pressure gradient force and the Coriolis force undergo dynamic changes; this causes air currents to flow through steep terrain at varying speeds and directions, easily triggering typical non-hydrostatic processes such as hydraulic jumps and downslope storms.

23. *l. 739: See comment on TKE above*

**Re:** Currently, the relevant description has been streamlined in the discussion section. Thank you.

24. *Appendix: Clarifying terms in snow transport. Drifting snow is preferably used synonymously with saltation and blowing snow with suspension, potentially*

*including preferential deposition during precipitation. Preferential deposition (as introduced in my 2008 paper) is strictly only deposition of precipitation, albeit it becomes a bit philosophical if a short rebound at the surface is then counted as erosion / deposition or still preferential deposition. Airborne snow is everything that is in the air regardless of the process behind and I would use it synonymously with aeolian snow. Snow transport is everything that moves snow not only in the air.*

**Re:** Thank you very much for your detailed explanation. We have used the term "preferential deposition" in the discussion section as follows: "Meanwhile, when strong winds and snowfall occur simultaneously, the actual object measured by the instruments is airborne snow, which includes the deposition of precipitation (preferential deposition, Lehning et al., 2008), and this also results in the overestimation of observations…". We would like to express our gratitude again.