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*

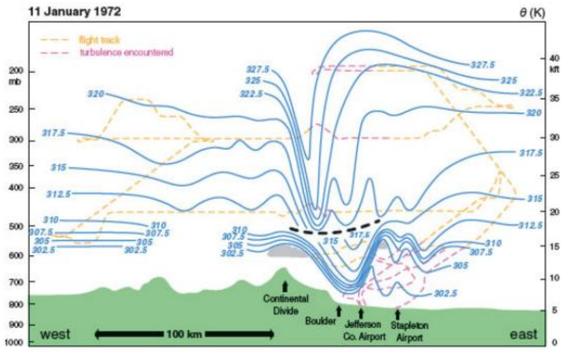
    Re: Done, as well as the other two similar places.

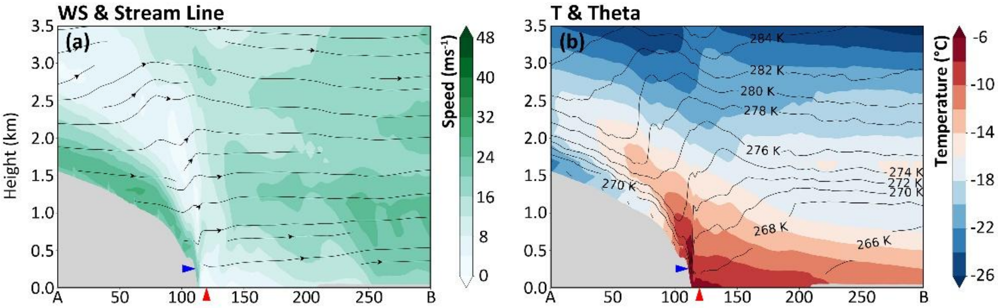
13. *l. 437: The fine-scale structure of BS mixing ratio in Fig. 7d looks suspicious. It almost appears that y and x axis are exchanged. Otherwise, the repeated pattern of high BS declining with distance from left to right and then jumping again to a high value is not realistic. It may simply be a problem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

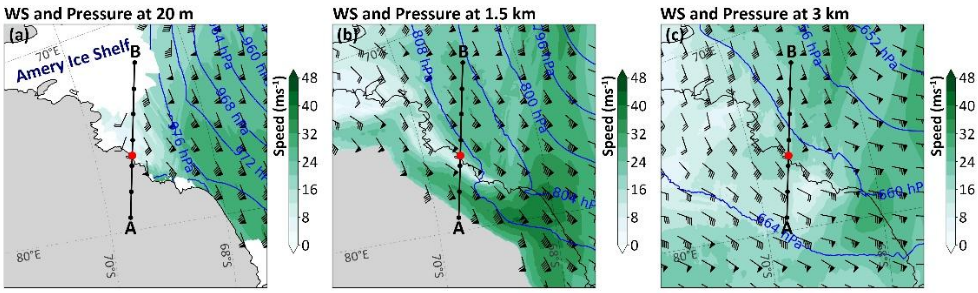
14. *l. 472 ff: It is true that turbulence dissipates energy but the primary reason for low wind speed should be the pressure gradient across the hydraulic jump with the turbulence then a consequence of high shear and accelerations, right?*

    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.

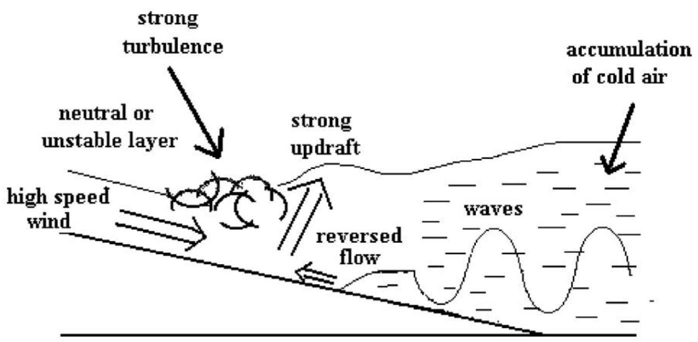


Figure Conceptual model of a katabatic flow jump based on NWP (Yu and Cai, 2006)

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.