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The Cryosphere Discussions

Interactive comment on "Blowing snow detection from ground-based ceilometers: application to East Antarctica" *by* Alexandra Gossart et al.

Anonymous Referee #1

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Multi-year observations from ground-based ceilometers are used to study blowing snow at two coastal locations in East Antarctica. The blowing snow products include notably monthly and annual frequencies of occurrence, blowing snow layer heights and time elapsed since last precipitation event. For one location the frequencies are evaluated against visual observations for a 6-year period (2010-2015) and show general agreement.

General comments: The paper is generally well written, although a more rigorous attention is required in some parts when describing and discussing the blowing snow processes. The results are interesting and original and can be of great potential but a substantial revision is needed before the paper becomes acceptable for publication. More specifically, I have some reservations on the profile classification procedure in its cur-

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rent form. The distinction between precipitation and mixed blowing snow–precipitation events (Fig. 5) is not convincing. Information is lacking on how precipitation data are used to identify the occurrence of precipitation, as well as on the availability of data over the measurement period at PE. The (monthly and annual) frequency of occurrence is not studied at PE despite 7 years of measurements. Other potentially valuable information may be produced such as the inter-annual variability in blowing snow frequency (at both locations) or the relative proportion of mixed and pure blowing snow events (at least at PE). If you can use your profile classification to discriminate between blowing snow and mixed blowing snow events at Neumayer, this would be also of great interest. More generally, some parts need clarification and/or rerrangement, and the switching between different notions or locations make the manuscript sometimes difficult to read. Section 4.2 is not very useful. The conclusion, as well as the abstract, could contain more of the main (potential) results (annual and monthly frequencies, inter-annual variability, relative proportions of mixed blowing snow events, mean blowing snow layer heights). Indicate also in the abstract the respective locations (Neumayer/PE) and the time period to which your results correspond to. I recommend that all co-authors carry out thorough reading of the paper before resubmission.

Specific comments: 1. P2, L1-4: Despite the abundant literature on that topic, I recommend not to use wind speed ranges as a criterion to distinguish between drifting and blowing snow. As it is mentioned in the paper, the occurrence of drifting and blowing snow is strongly related to surface snow properties, which make the characterization of wind speed thresholds relative to the local climate conditions. For instance, low wind speeds can initiate erosion where loose snow is frequently brought by snowfall, while high wind speeds are needed to erode consolidated snow. The actual turbulent quantity involved in aerodynamic entrainment of surface snow particles is the friction velocity. Erosion starts when the actual friction velocity (depending on atmospheric flow conditions and surface aerodynamic properties) exceeds a threshold friction velocity (related to surface physical snow properties: density, cohesion, grain size, etc.). In the context of this paper, using a more general classification by mentioning just the height at which windborne snow is observed is a more convenient way to describe the drifting (< 2m) and blowing snow (> 2m) processes. Besides, it is not correct to discriminate between suspension and blowing snow. Suspension is a transport mode and refers to diffusion of snow particles in the atmosphere picked up at the top of the saltation layer by turbulent eddies. For a given erosion event, the maximum elevation reached by suspended particles in define the height of the blowing sow layer, which is thus not necessarily confined to a few meters above the surface. Saltation is the other main transport mode, and describes ballistic trajectories and periodic rebounds of particles at the surface. Drifting and blowing snow thus must be seen as differently balanced situations between these two transport modes: drifting snow more generally refers to a situation where saltation is the dominant transport mode, while blowing snow stands for the opposite.

2. P2, L10: Similarly, the threshold speed of 11 m s-1 given by Kodama et al. (1985) is relative to the measurement period and location in Adelie Land and should not be presented as a general threshold above which the influence of snowdrift sublimation on SMB become significant.

3. P2, L11: This is not always the case. Change for “can be more effective”.

4. P2, L17: “Affecting [. . .] the surface energy balance”, not “affect [. . .] on surface . . .”.

5. P2, L34: You should refer to Trouvilliez et al. (2014), who also report drifting snow statistics in East Antarctica from ground-based measurements with Flow-Capt instruments, instead of Trouvilliez et al. (2015) who present an evaluation of the Flow-Capt in the French Alps. The paper of Barral et al. has been published in 2014.

6. P5, L7 to P6, L2: These sentences belong to the methodology and should be moved in section 3.2.

7. P6, L3: How do you use this information in the study?

8. P7, L19: Distinguishing visually between pure blowing snow and mixed blowing

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snow-precipitation events seems far too subjective to me, even if “the blowing snow layer is not too dense”.

9. P8, L31: the first “of” has to be removed. Change “layer. E.g.” for “layer, e.g.”

10. P10, L5 and onwards: It is likely that I don’t understand correctly the detection principle, but in its current form I have some reservations about your classification procedure, especially about the distinction between precipitation with and without blowing snow, and the omission of strong precipitation associated with heavy blowing snow. I tried to list them below. It is difficult to relate the profile features described in the text using heights and bin numbers to the plotted profiles in Fig. 5. You could, for instance, clearly indicate the discontinuity between the 4th and 5th bins, and specify to which bin the lowermost backscatter intensity value reported on the graph correspond to. This would facilitate the understanding of the description of the detection algorithm.

- The increase in the backscatter signal between the first and the second bins in the mixed blowing snow profile in Fig. 5 is of vey small intensity compare to the one characterizing pure blowing snow. Except for this aspect, this profile seems very similar to the pure precipitation profile. Moreover, I suppose that a mixed profile should include both the signature of precipitation and blowing snow (strong signal close to the surface). Are you sure that this absence of the blowing snow signature does not simply imply that there is no blowing snow?

- L14: “between 40 and 50 m”: give the corresponding bin numbers.

- L17: I don’t understand why during strong precipitation associated with storms, the precipitation intensity might cover the blowing snow signal close to the ground. I’m wondering even further if the opposite would be true. The strong backscatter signal close to the surface in the typical blowing snow profile illustrates the influence of high particle density layers. This would be particularly amplified when abundant snowfalls provide a large supply of fresh snow that can be easily eroded by strong winds. By discarding these cases, you might omit an important part of the mixed blowing snow events, which can further affect all your statistics. This could be a major issue since you say latter in the paper that most of the blowing snow events occur simultaneously with precipitation. If the situation with strong precipitation and blowing snow is a clear limitation of your approach, you have to quantify it, especially since the occurrence of overcast conditions is also a limitation to satellite retrieval. You should give the relative proportion of each profile category (blowing snow, precipitation + blowing snow, precipitation, clear sky and omissions).

11. P11, Section 4.1: There is a temporal discordance between visual observations (performed 6 times a day) and ceilometer measurements (hourly means). Have you re-sampled the ceilometer dataset to match the frequency of visual observations, or do you compare the ceilometer hourly output corresponding to the time at which the visual observations were performed? Are the visual observations continuous over the measurement period (2010-2015)?

12. P12, Figure 6: Indicate N for each category.

13. P12, L16: Don't you think you could use the profile classification developed at PE (in terms of vertical variation in backscatter intensity) to discriminate the occurrence of precipitation at Neumayer?

14. P12, L19: An "r" is missing in the penultimate word.

15. P13, Fig. 7: How can you explain the apparently systematic discordance between visual observations and the detection algorithm in January?

16. P13, L7: Please indicate over which period of time the frequency is computed.

17. P13, Second paragraph: this paragraph is hard to follow and needs rearrangement:

- L9-11: You switch between annual and monthly time scales, and frequency and blowing snow rates. Move the sentence in which you describe the calculation of the frequencies at the beginning of the paragraph. Indicate the time period over which König-Langlo and Goose (2007) computed their frequencies. Remove "blowing snow rates" and stay focus on frequencies to compare apples and apples. Indicate also the measurement period for the frequency computed at PE (and for this you also need to discuss the representativeness of the winter data due to power supply issues).

- L13: See also Trouvilliez et al. (2014) and Amory et al. (2017) for similar statistics from ground-based measurements.

- L14: "Reasonable" is not rigorous. Please replace.

- L16: In the previous sentence you give the frequency for two locations (Neumayer: 28% and PE: 9%): which one do you compare with Palm's results? "Coherent" and "analogous" give no quantitative information, and are somewhat confusing when used together. Give directly the values from Palm et al. (2011) (and indicate the measurement period) and, then, discuss the particular geographical settings of PE to explain the contrast in wind speed and, ultimately, in blowing snow frequencies, with the other results/locations mentioned in the text. If the frequencies compare reasonably well with satellite measurements, does this mean that the hindering effect of clouds is not so influent? Again this appears contradictory with the apparently frequent occurrence of precipitation and overcast conditions during blowing snow events.

18. P14, Fig. 8 (legend): Non blowing snow (not "no")

19. P14, section 4.2: This section could have been more organized. You alternate between katabatic and synoptic conditions, blowing snow and non-blowing snow conditions, PE and Neumayer, and results and theory. Some sentences are ambiguous, others contain syntax errors, irrelevant or incomplete information, and some conclusions seem a bit early. I think you could remove this section entirely without disturbing your global analysis. Moreover, this would avoid redundant information with section 4.3, in which you actually refer to the work of Gorodetskaya et al. (2013) to define the two meteorological regimes. Find more detailed comments below:

- L5: "Fig. 8 and 10": an "s" is lacking

- L5-7: You only use a wind direction criterion to distinguish katabatic from synoptic conditions. What about a combined influence of katabatic and synoptic conditions? Is the deflection due to the Coriolis force also an influent factor accounting for the easterly component of the surface flow?

- L8-10: This sentence is ambiguous. Please rephrase.

- L11-13: Harsh construction. The colon (":") is misused. "wind speeds are high enough to be able to. . .and saltation" is clumsy: I guess "wind speeds are high enough to initiate snowdrift" is analogous but more concise.

- L12: The increase in RH is (partly) caused by blowing snow, not a cause of, so it doesn't "privilege" blowing snow.

- L13-15: Mentioning the self-limiting process of blowing snow sublimation and the increase in roughness due to windborne snow particles is not relevant since i) they are not a result here and ii) they don't explain any described feature.

- L15-16: This sentence needs rephrasing: "The increase in RH is both a result [. . .] and sublimation (not "due to") of precipitating and blowing snow particles."

- P15, L1: "Those also have an impact on the radiative budget": This is elusive. Illustrate and discussed further or remove.

- P15, L2: Turbulent mixing generally occurs during strong winds, whatever their origin (synoptic or katabatic). How do you distinguish between synoptic and katabatic conditions?

- P15, L4: "These variables": You mean "trends" (?)

20. P16, Fig. 10 (caption): Indicate the relative proportion of each category.

21. P16, L4: "as", not "although".

22. P16, L13: Remove "anymore".

23. P17, section 4.3.2: It is not clear how the depth of the blowing snow layer is determined.

24. P18, Fig. 11 (ordinate axis): Indicate the units.

25. P18, L10: If your algorithm is applied "successfully", then you consider the visual observations as ground truth. Compare favorably with or something like that, would be more appropriate. Idem for "proved the applicability".

26. P19, L15: Metamorphism does not impact the friction velocity, only the threshold friction velocity (see comment #1).

27. P19, L17: Can you give more examples of such (many) studies?

28. P19, L19: a "the" is redundant. The properties listed in brackets are not complementary information of "freshly fallen snow". Please rephrase.

29. P19, L29: Which role do you give to the turbulence during katabatic conditions in limiting the occurrence of blowing snow at PE?

30. P19, L29-31: Katabatic winds or conditions, not "katabatics". Please clarify where and how the effect of katabatic winds on the occurrence of blowing snow has been overestimated? Do you actually mean that katabatic winds are not the main driven force behind blowing snow at PE, as usually considered? If so, you should limit this conclusion to the particular geographical settings of PE, which are likely non-representative of the general conditions in coastal East Antarctica.

31. P20, L7: Specify that this conclusion is only valid for PE.

32. P20, L9: "mainly determines".

33. P20, L10-11: In which context this conclusion has been drawn?

34. P20, L12: "The availability": you mean erodibility (availability of fresh snow is not a snow property)?

35. P20, L15: Use “evaluate” rather than “validate”.

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