The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-165-AC2, 2019

Interactive comment on "Brief communication: Rare ambient saturation during drifting snow occurrences in coastal East Antarctica" *by* Charles Amory and Christoph Kittel

Charles Amory and Christoph Kittel

charles.amory@uliege.be

Received and published: 25 October 2019

Dear Michael,

We thank you for your insightful comments that will undoubtedly help to improve the quality of the manuscript. We have adapted the text in several places and included additional discussions to fit your suggestions. Our responses are reported hereafter.

General comments

The paper presents an observational study on moisture dynamics at one site in Antarctica with frequent events of strong katabatic winds and associated snow transport. The

paper is nicely formulated and timely as the role of sublimation during snow transport is a currently debated problem in meteorology and snow science.

Major comments

RC2: I asked myself whether this rather limited dataset is worth a separate publication but concluded that it helps to shape our understanding of what blowing snow sublimation may look like in these katabatic wind zones. However, I suggest that more complete context and discussion is provided. There have been recent LES simulations on drifting snow sublimation (e.g. Sharma et al., 2018; Huang et al., 2016), which claim that previous efforts in modelling sublimation may have started from wrong assumptions. It would add value and impact to the current paper if the authors could discuss whether their observations are consistent with the new model findings or not.

Authors: Thank you for this very relevant suggestion. We have completed the discussion with the following paragraph in which Fig. 2 is used to discuss our results from the perspective of the above-mentioned model findings: "In previous efforts dedicated to the modelling of windborne-snow sublimation, the saltation layer has been regarded as saturated at all times and emphasis was primarily placed on the sublimation of suspended snow particles. Recent large-eddy simulations (e.g., Huang et al., 2016; Sharma et al., 2018) have argued from reassessments of physical starting assumptions that sublimation within the saltation layer might be of significant importance in the surface and atmospheric water budgets. In some of these experiments, sublimation within the saltation layer can even exceed sublimation of suspended particles by several orders of magnitude when effective advective transport of moisture can sustain an undersaturated environment in the immediate vicinity of the surface (Huang et al., 2016). Assuming that saturation at the lowest measurement level of $\sim$0.4 m above ground (i.e., well above the saltation layer) also implies saturation down to the surface, Fig. 2 would indicate that saturation at saltation heights is observed in at least one third of the drifting snow occurrences, i.e. 4 times more frequently than saturated conditions at all measurement levels. In these cases, sublimation of saltating particles could indeed be ignored and only sublimation within the suspension layer would likely contribute to the surface-atmosphere moisture exchange. Similarly, limited moisture fluxes from sublimation of saltating snow could be expected in fully developed, deep blowing snow layers in which the contribution of suspension to the total column-integrated mass flux dominates over saltation.".

A brief mention of this new element of discussion is also made in the conclusion by completing the former sentence "Although saturation is preferentially confined below 1 metre, low-level atmospheric moistening by the sublimation of [. . .]" which thus becomes "Although saturation is preferentially confined below 1 metre likely involving saturation in the saltation layer, low-level atmospheric moistening by the sublimation of [. . .]".

RC2: One major comment I have is on the overall limitation of sublimation in snow transport clouds. The authors revisit the argument that with stronger wind and snow transport, total snow sublimation may be limited because saturation occurs. This argument has been formulated by Bintanja (2001) based on a model study, in which the author considers a model depth of 10 m to look of blowing snow sublimation. I have always been very skeptical about the conclusion of limited sublimation because I expect in these situations the level of maximum sublimation to be simply lifted to higher elevations such that it is not seen in the first 10 m. If I understand the Bintanja model study correctly, then he only sums up sublimation occurring in the lowest 10 m, which is of course only part of the total sublimation if high winds cause deep clouds of blowing snow. The authors are therefore encouraged to either present clear evidence of total sublimation reduction in stronger winds, or not conclude about this aspect.

Authors: We could not agree more with your comment. We have removed the conclusion about the original argumentation on the peak in sublimation for moderate winds in the Results section and added the following paragraph in the Discussion section"Figure 3 demonstrates that a layer of near-saturated air inevitably develops along the whole profile in the most extreme wind and drift conditions. Similar results obtained from

modelling experiments in which a blowing snow layer of 10 m depth is considered, has been used to hypothesise that total windborne-snow sublimation may be limited with strong winds and snow transport because low-level saturation occurs (Bintanja, 2001b). While both results do imply that further moisture release is inhibited in such conditions through the negative feedback of windborne-snow sublimation in a small portion of the near-surface atmosphere, it might also involve that the level of maximum sublimation is lifted to higher elevations when strong winds cause deep blowing snow layers. An important implication could thus be that, despite saturation conditions in the low-level atmosphere, the greatest contribution of windborne-snow sublimation to the total moisture flux might still occur at the highest wind speeds and associated snow transport, as near-surface sublimation is only part of the total atmospheric sublimation.".

As we believe that this reasoning can be of significant relevance for future blowing snow studies, it is also mentioned in the conclusion as: "[. . .] during those events for which the occurrence of drifting snow (i.e., < 2 m) and a saturated air layer of several metres is concurrently reported, the height reached by the suspended snow particles, or the depth of the transport layer, likely extends beyond the uppermost measurement level (i.e., > 5.5 m). As this possibly induces significant windborne-snow sublimation in higher atmospheric levels, contrary to previous considerations (Bintanja, 2001b) it is hypothesized that the presence of a low-level saturated air layer would not limit total atmospheric sublimation if the level of maximum sublimation in such conditions is simply moved upwards.".

Finally we have adapted our last paragraph in the conclusion as: "Combined dropsonde and satellite measurements have shown that blowing snow can frequently develop into deep layers of several hundreds of metres above the ice-sheet surface in which RH decreases with height through the depth of the layer (Palm et al., 2018), suggesting the existence of a potentially significant atmospheric mass sink through windborne-snow sublimation. However, satellite detection only captures blowing snow layers 30 m or greater in thickness during clear-sky or optically thin-cloud conditions and is limited by the satellite revisit frequency. Synergistic uses of remote sensing, modelling and observation products would help to assess the proportion of missed events when surveying snow transport from space and study the conditions that lead drifting and shallow (< 30 m) blowing snow events to evolve into deep blowing snow layers, to ultimately improve understanding of the links between snow transport and moisture dynamics and better quantify the influence of windborne-snow sublimation on the surface mass balance of the Antarctic ice sheet.".

Minor comments

RC2: In practical meteorological applications, drifting snow is below 2 m and blowing snow above. Snow scientists, however, would rather define drifting snow as saltation and blowing snow as suspended snow.

Authors: We agree that a standard definition for distinguishing between drifting and blowing snow is still lacking in the scientific community. The formulation of an official consensus may have been hindered by the use of analogous terminologies for the description of diverse meteorological conditions, (without systematic detailed explanations of the semantics used) to the extent that it would, maybe, ideally require a conciliation meeting some day. However, the definition using a standard level of 2 m in height as a distinction criteria has been widely employed over the recent years in publications dealing with snow transport in Antarctica (e.g., Leonard et al., 2011; Lenaerts et al., 2012; Gossart et al., 2017; Trouvilliez et al., 2014; Palm et al., 2017, 2018a,b) while we found no clear evidence of a distinction involving analogy between drifting and saltating snow from one hand and blowing and suspended snow on the other hand, even in the most recent publications dedicated to the characterization of specific aeolian processes (e.g., Aksamit and Pomeroy, 2017; Crivelli et al., 2016; Huang et al., 2016; Huang and Wang, 2016; Comola and Lehning, 2017; Comola et al., 2017; Paterna et al., 2016, 2017; Sharma et al., 2018). In addition, and from our point of view, one could argue that this requires the use of different words for referring to the same mechanism

(saltation/drifting and suspension/blowing) which could eventually be a bit confusing in some way. One option could be to mention the existence of the two definitions, and be explicit about the one we choose in the rest of the manuscript. In that case, could you indicate publications in which this definition clearly appears so we can refer to them and then support the proposed definition in the paper?

RC2: L30 (and first sentence in the abstract): I think that it is not proven yet or generally accepted that snow transport and sublimation is the main ablation process over the entire Antarctic ice sheet.

Authors: Yes you're right, this assertion actually relies only on a few modelling studies which have not resulted in a consensual acceptance. We changed the sentence to "Continent-wide modelling studies suggest that erosion through divergence of drifting and blowing snow transport and concurrent sublimation of particles during transport currently represent important ablation processes on the ice sheet.".

RC2: L50: Suggest to replace "verified" by "met".

Authors: Changed accordingly.

RC2: L75 vs. L80: You cannot say that D17 is in an accumulation zone and then that you achieve equilibrium horizontal mass flux. This is contradictory.

Authors: This clumsy part has been removed from the sentence.

RC2: L107: Whether or not the vapor pressure really increases towards the surface (or only most often) depends on the air temperature gradient (and the one in the surface snow to a lesser degree).

Authors: The influence of the air temperature gradient on the water vapour gradient decreases with the air temperature gradient itself. Active katabatic winds in the measurement area provide efficient turbulent mixing in the near-surface atmosphere. Figure R1 shows that the air temperature gradient (computed from the temperature difference between the highest and the lowest measurement level) decreases as wind speed increases, and is in most cases below the actual instrumental accuracy of 0.4 °C at -20 °C (see Table 1 in the original manuscript). We can thus expect the actual temperature gradients in the thin portion of the atmospheric boundary layer covered by the meteorological profiles (the first 6 meters above the surface) to be very small (actually below instrumental accuracy), even more in case of moderate to strong winds so we can reasonably make the assumption of the existence of a nearly-isothermal layer. Figure 3 in the original manuscript evidences however the existence of RH gradients above instrumental accuracy for every range of wind speeds that cannot be entirely explained by possible residual temperature gradients; explanations for this gradients have then been proposed from L107 to L110 (in the original version) and used as arguments to justify the importance of relatively constant measurement heights in the statistical analysis of relative humidity. Nevertheless, we have modified slightly the sentence in the revised version of the manuscript and mention instead the proximity with the surface as a influencing factor for RH as :"This is important for consistent time statistics of relative humidity since (i) the proximity with the snow surface, which acts as a moisture source through sublimation influences the vapour pressure of the air and (ii) the additional moisture loading and atmospheric cooling through windborne-snow sublimation at a given elevation above the snow surface partly depends on the snow mass concentration which is a strongly decreasing function of height.".

RC2: L161 ff: Mark this as hypothesis/discussion and mention (again) temperature gradients, which are also able to produce moisture gradients.

Authors: This paragraph now includes temperature gradients as a possible explanation for the RH gradients as "Only surface sublimation, and possibly residual temperature gradients in that case contribute to the vertical moisture gradient and leads RH to increase when approaching the surface.".

RC2: L175: See major comment above on moving the elevation of maximum sublimation upwards in higher winds. This is what I expect to occur.

Authors: See our response to major comment #2.

RC2: L212: Check wording "preferably".

Authors: We changed to "preferentially".

Aksamit, N. O. and Pomeroy, J. W.: The Effect of Coherent Structures in the Atmospheric Surface Layer on Blowing-Snow Transport, Boundary-Layer Meteorol., doi:10.1007/s10546-017-0318-2, 2017.

Bintanja, R.: Modelling snowdrift sublimation and its effect on the moisture budget of the atmospheric boundary layer, Tellus A, 53(2), 215–232, doi:10.1034/j.1600-0870.2001.00173.x, 2001.

Comola, F. and Lehning, M.: EnergyâĂŘ and momentumâĂŘconserving model of splash entrainment in sand and snow saltation, Geophys. Res. Lett., 44(3), 1601–1609, doi:10.1002/2016GL071822, 2017.

Comola, F., Kok, J. F., Gaume, J., Paterna, E. and Lehning, M.: Fragmentation of wind-blown snow crystals, Geophys. Res. Lett., 44(9), 4195–4203, doi:10.1002/2017GL073039, 2017.

Crivelli, P., Paterna, E., Horender, S. and Lehning, M.: Quantifying Particle Numbers and Mass Flux in Drifting Snow, Boundary-Layer Meteorol., 161(3), 519–542, doi:10.1007/s10546-016-0170-9, 2016.

Gossart, A., Souverijns, N., Gorodetskaya, I. V., Lhermitte, S., Lenaerts, J. T. M., Schween, J. H., Mangold, A., Laffineur, Q. and van Lipzig, N. P. M.: Blowing snow detection from ground-based ceilometers: application to East Antarctica, The Cryosphere, 11(6), 2755–2772, doi:10.5194/tc-11-2755-2017, 2017.

Huang, N. and Wang, Z.-S.: The formation of snow streamers in the turbulent atmosphere boundary layer, Aeolian Research, 23, 1–10, doi:10.1016/j.aeolia.2016.09.002, 2016.

Huang, N., Dai, X. and Zhang, J.: The impacts of moisture transport on drifting snow sublimation in the saltation layer, Atmos. Chem. Phys., 16(12), 7523–7529, doi:10.5194/acp-16-7523-2016, 2016.

Lenaerts, J. T. M., van den Broeke, M. R., Déry, S. J., van Meijgaard, E., van de Berg, W. J., Palm, S. P. and Sanz Rodrigo, J.: Modeling drifting snow in Antarctica with a regional climate model: 1. Methods and model evaluation, J. Geophys. Res., 117(D5), n/a-n/a, doi:10.1029/2011JD016145, 2012.

Leonard, K. C., Tremblay, L.-B., Thom, J. E. and MacAyeal, D. R.: Drifting snow threshold measurements near McMurdo station, Antarctica: A sensor comparison study, Cold Regions Science and Technology, 70, 71–80, doi:10.1016/j.coldregions.2011.08.001, 2011.

Palm, S. P., Kayetha, V., Yang, Y. and Pauly, R.: Blowing snow sublimation and transport over Antarctica from 11 years of CALIPSO observations, The Cryosphere, 11(6), 2555–2569, doi:10.5194/tc-11-2555-2017, 2017.

Palm, S. P., Yang, Y., Kayetha, V. and Nicolas, J. P.: Insight into the Thermodynamic Structure of Blowing-Snow Layers in Antarctica from Dropsonde and CALIPSO Measurements, J. Appl. Meteor. Climatol., 57(12), 2733–2748, doi:10.1175/JAMC-D-18-0082.1, 2018a.

Palm, S. P., Kayetha, V. and Yang, Y.: Toward a Satellite-Derived Climatology of Blowing Snow Over Antarctica, J. Geophys. Res. Atmos., 123(18), 10,301-10,313, doi:10.1029/2018JD028632, 2018b.

Paterna, E., Crivelli, P. and Lehning, M.: Decoupling of mass flux and turbulent wind fluctuations in drifting snow, Geophys. Res. Lett., 43(9), 4441–4447, doi:10.1002/2016GL068171, 2016.

Paterna, E., Crivelli, P. and Lehning, M.: Wind tunnel observations of weak and strong snow saltation dynamics, J. Geophys. Res. Earth Surf., 122(9), 1589–1604,

doi:10.1002/2016JF004111, 2017.

Sharma, V., Comola, F. and Lehning, M.: On the suitability of the Thorpe–Mason model for calculating sublimation of saltating snow, The Cryosphere, 12(11), 3499–3509, doi:10.5194/tc-12-3499-2018, 2018.

Trouvilliez, A., Naaim-Bouvet, F., Genthon, C., Piard, L., Favier, V., Bellot, H., Agosta, C., Palerme, C., Amory, C. and Gallée, H.: A novel experimental study of aeolian snow transport in Adelie Land (Antarctica), Cold Regions Science and Technology, 108, 125–138, doi:10.1016/j.coldregions.2014.09.005, 2014.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-165, 2019.

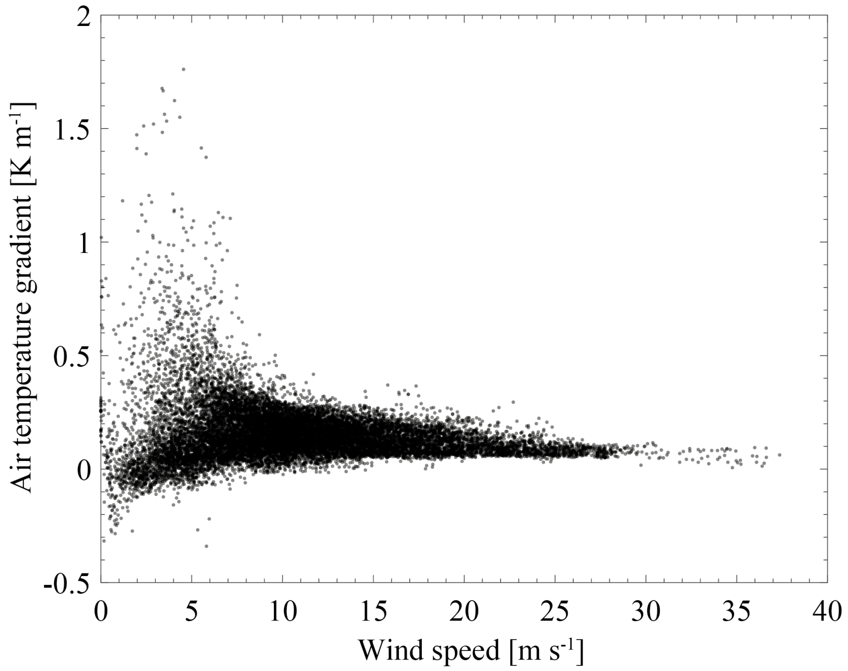

Fig. 1. Figure R1. Air temperature gradient (computed from the temperature difference between the highest and the lowest measurement level) as a function of wind speed.