RESPONSE TO REFEREES' E. A. PODOLSKIY COMMENTS

TO MANUSCRIPT TC-2015-158

Title: Tomography-based observation of sublimation and snow metamorphism under temperature gradient and advective flow

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We thank the referee E. A. Podolskiy for the constructive comments. All page and line numbers correspond to those of the Discussion Paper.

REVIEWER: E. A. Podolskiy

By employing time-lapse micro-CT approach, the work by Ebner et al. (2015) provides a fresh look onto old and important questions of sublimation and ice reposition within snow, which are indeed crucial for our understanding of snow physics and snow chemistry. Overall, I find the manuscript to be in a good shape, but nevertheless would like to suggest multiple points to consider for improving the readability and clarity of the paper.

Comment #1: p. 4846/14: *Consider giving a broader perspective: "… relevant for atmospheric chemistry." -> "… relevant for atmospheric chemistry {and isotope contents in snow}."*

Revision: Text changed in the revised paper:

> On page 4846, line 13: "However, the strong recrystallization of water molecules in snow may impact its isotopic or chemical content."

Comment #2: p. 4847/1: *In this introduction, I suggest to remind this well familiar effect before giving its name, say: "due to the Kelvin-effect" -> "due to higher vapor pressure over curved surfaces (Kelvin-effect)"*

Revision: Text changed in the revised paper:

> On page 4847, line 1: "… due to higher vapor pressure over convex surfaces and lower vapor pressure over concave surfaces, respectively (Kelvin-effect)."

Comment #3: p. 4847/10: *As non-native English speaker, I cannot correct language. Nevertheless, I note that I never saw "whistler-like crystals" in conventional snow classifications and could not get what was meant by it.*

Response: It is not mention in the conventional snow classifications but was observed in the Paper by Ebner et al. (2015b). However, there is spelling mistake, it is called "whisker-like crystals.

Revision: Text changed in the revised paper:

> On page 4847, line 10: "… whisker-like crystals (Ebner et al., 2015b). Whisker-like crystals are very small (~10-30 μm) elongated monocrystals."

Comment #4: p. 4847/10-11: *Please, reread this sentence, seems to be the opposite? You say the opposite (in p.4850, Lines 18-21).*

"The flow rate dependence on the deposition rate of water vapor on the ice matrix was observed, reaching asymptotically a maximum rate of … (Ebner et al., 2015b)." ->

"The {deposition rate of water vapor on the ice matrix} dependence on the {flow} rate was observed, reaching asymptotically a maximum rate of … (Ebner et al., 2015b)."

Revision: Text changed in the revised paper:

> On page 4847, line 10-11: "A deposition rate of water vapor on the ice matrix dependence on the flow rate was observed, reaching asymptotically a maximum rate of … (Ebner et al., 2015b)."

Comment #5: p. 4847/19: *In regard to ice-core interpretation I also suggest to add a reference to experimental study on isotopic content of snow driven by sublimation.*

Ekaykin, A. A., Hondoh, T., Lipenkov, V. Y. and A. Miyamoto (2009) Post-depositional changes in snow isotope content: preliminary results of laboratory experiments, Clim. Past Discuss., 5, 2239–2267.

Revision: Text changed in the revised paper:

> On page 4847, line 19: "… and thus is important for ice core interpretation (Stichler et al., 2001; Ekaykin et al., 2009)"

Reference added:

> Ekaykin, A. A., Hondoh, T., Lipenkov, V. Y. and A. Miyamoto (2009) Post-depositional changes in snow isotope content: preliminary results of laboratory experiments, Clim. Past Discuss., 5, 2239–2267.

Comment #6: p. 4847/22-23: *About Albert (2002) and his 2D finite-element model ->*

Please note that much more recently Slaughter and Zabaras (2012) took into account such effects on microstructure through a 3D FEM micro-structural model, which considers vapor

deposition and sublimation within the snow. The latter study is indeed very relevant reference here.

Slaughter AE and Zabaras N (2012), A phase-tracking snow microstructure model. In Proceedings of the International Snow Science Workshop, 16–21 September 2012, Anchorage, Alaska, 395–397 http://arc.lib.montana.edu/snow-science/item/1739

Response: It's correct that this study is a very relevant reference but, based on the Proceeding, this model is not yet finished and no results could be found in the literature. Therefore, it will not make sense to cite this. But we will cite Kaempfer and Plapp (2009)

Revision: Reference added in the revised manuscript

> Kaempfer, T. U. and Plapp, M. (2009) Phase-field modeling of dry snow metamorphism, Physical Review E, 79, http://dx.doi.org/10.1103/PhysRevE.79.031502.

Comment #7: p. 4848/8: *the warm site of the snow -> the warm si{d}e of the snow?*

Revision: Text changed in the revised paper:

> On page 4848, line 8: "… the warm side of the snow …"

Comment #8: p. 4848/10-11: *If available, a range of estimated naturally occurring flow rates would be useful to mention here for indicating how significant the used values were.*

Response: Unfortunately, no directly measured flow rates in a snowpack could be found in the literature, but based on numerical simulation in the PhD thesis of T. Neumann (2003) a maximum flow rate of 0.01 m s^{-1} inside the snow layer (close to the surface) was estimated for a high wind speed (≈10 m s^{-1}).

Comment #9: p. 4848/15-16: *"... and to evaluate the structural change in the earlier stage of metamorphism of new snow." -> I could not follow where in this paper this earlier stage had been discussed? Since it does not seem to be mentioned ever again in this particular manuscript, this phrase could be removed as irrelevant here.*

Revision: Text changed in the revised paper:

> On page 4848, line 15-16: "The snow was sieved with a mesh size of 1.4 mm into a box, and was sintered for 27 days at -5 ºC to increase the strength."

Comment #10: p.4848/19-20: *Please, state the resolution of taken projection X-ray images and reconstructed 3-D scenes.*

Revision: Text changed in the revised paper:

On page 4848, line 19-20: "The snow samples were measured with a voxel size of 18 μm over 108 h with time-lapse micro-CT measurements taken every 3 h, producing a sequence of 37 images."

Comment #11: p. 4849/3: *"is analyzed" -> "was analyzed" (for consistency with overall past tense) however, see also below where I suggest to remove this sentence.*

Revision: See Comment #15.

Comment #12: p. 4849/6: *pictures -> images*

Revision: Word changed in the revised paper:

Comment #13: p. 4849/7-8: *"was determined by direct pore-level simulations (DPLS) to determine" -> to avoid repetition of the same word, in one place it could be "was estimated by …"*

Revision: Text changed in the revised paper:

On page 4849, line 7-8: "… was estimated by direct pore-level simulations (DPLS) to determine …"

Comment #14: p. 4849/9: *a reference is made to Löwe et al. (2012), which does not appear in the references.*

Revision: It should mean: Löwe et al. (2013).

Comment #15: p. 4849/2-3 & 6-9: *These two sentences could be easily merged to avoid redundant text and repeating references:*

"As additional physical and structural parameter, the {effective} thermal conductivity{,k_e,} was estimated by direct pore-level simulations (DPLS) to determine the influence of changing microstructure (Kaempfer et al., 2005; Petrasch et al., 2008; Calonne et al., 2011; Löwe et al., 2012)."

Also, for someone who is not familiar with DPSL, an extra sentence introducing the main principle of this computational approach would be informative.

Revision: Text changed in the revised paper:

On page 4849, line 6-9: "As additional physical and structural parameter, the effective thermal conductivity, k_{cond}, was estimated by direct pore-level simulations (DPLS) to

determine the influence of changing microstructure. DPLS determined the effective thermal conductivity by solving the corresponding mass and momentum conservation equations (Kaempfer et al., 2005; Petrasch et al., 2008; Calonne et al., 2011; Löwe et al., 2013)."

Comment #16: p. 4849/19: *"were observed for example, Fig …" "were observed{,} for example, Fig …"*

Revision: Text changed in the revised paper:

On page 4849, line 19: "…were observed, for example, Fig …"

Comment #17: p. 4849/20: *since a reference to test names is made for the first time, it could be smoothed by adding: "for {tests} "ota3" and "ota4".*

Here I also note that Tables 1 and 2 use test names which are different from those mentioned in the main text, Figs. 2 & 4 or a caption of Fig. 3 (e.g., "ta1" vs. "ota1").

Response: The tests names in Tables 1 and 2 are incorrect, it should mean "ota". It is changed in the revised paper.

Revision: Text changed in the revised paper:

On page 4849, line 20: "Fig. 3 shows the locations of sublimation and deposition for tests "ota3" and "ota4"".

Comment #18: p. 4849/26: *What was an uncertainty for estimated pore size? Similar to the one stated for evaluating reposition?*

Response: It is not relevant to mention the uncertainty for the estimated pore size as the trend is relevant and not the absolute value. But the uncertainty for the estimated pore size is similar to the one stated evaluating reposition.

Revision: Text added in the revised manuscrip

On page 4849, line 25: "The pore size (uncertainty 6 %) increased …"

Comment #19: p. 4850/1-2: *Should be also mentioned that possible ice loss could not be detected due to limited accuracy (which is almost the same as provided values; p. 4849, Line 21)?*

Revision: Text changed in the revised paper:

On page 4850, line 1-2: “Loss of ice of the snow due to sublimation could not be detected by the micro-CT scans due to limited accuracy and no flow rate …”.

Comment #20: p. 4850/13: *What was the accuracy of estimated thermal conductivity (could be mentioned in DPSL part)?*

Response: It is not relevant to mention the accuracy of the estimated thermal conductivity as the trend is relevant and not the absolute value. However, the standard deviation is only 0.025 W m^{-1}. (Calonne et al., 2011)

Revision: Reference added in the revised manuscript.

Calonne, N., Flin, F., Morin, S., Lesaffre, B., Rolland du Roscoat, S., and Geindreau, C. (2011) Numerical and experimental investigations of the effective thermal conductivity of snow, Geophysical Research Letters, 38, doi:10.1029/2011GL049234.

Comment #21: p. 4850/21: *This phrase is slightly confusing: “In this study, changing flow direction lead …”, given that in methods Fig. 1 showed this direction as constant. Was it flipped instead of flipping your heating system? If so, please, check if it is clear in methods. Or if it refers to previously published paper, re-written -> “In the {latter} study, …”*

Revision: Text changed in the revised paper:

On page 4850, line 21: “In this study, changing the temperature gradient leads to a warming up of a cold saturated flow …”

Comment #22: p. 4851/5: *the analyze volume -> the analyzed volume?*

Revision: Word changed in the revised paper:

Comment #23: p. 4851/7: *due to the undersaturated airflow -> In methods, you indicated that the incoming advective flow was initially saturated. Here it is undersaturated. Due to warming during propagation? Perhaps, it could be mentioned here for a sake of clarity.*

Revision: Text changed in the revised paper:

On page 4851, line 7-9: “However, the uptake of water molecules and their transport due to warming during propagation was counteracted by diffusion of water molecules due to the temperature gradient.”

Comment #24: p. 4851/11-12: *Please, explain how mass transfer between diffusion and advection was measured? It remains not very clear from the context.*

Revision: Text changed in the revised paper:

> On page 4851, line 10-14: "The Peclet numbers ($Pe = u_D \cdot d_{mean}/D$ where D is the diffusion coefficient of water vapor in air), describing the ratio of mass transfer between advection and diffusion, measured during each experiment, showed that diffusion was still dominant (Table 1). Therefore, water molecules were diffused along the temperature gradient and advected along the flow direction leading to a back and forth transport of water molecules."

Comment #25: p. 4851/19-20: *Please, clarify here what causes the increased interaction between air and ice. Residence time? Or air pressure increase due to Bernoulli's principle?*

Revision: Text changed in the revised paper:

> On page 4851, line 19-20: "… and therefore the interaction of an air-parcel with the ice matrix in the pores increases due to higher residence time."

Comment #26: p. 4852/14: *I could not follow how Reynolds number was evaluated in the study, and in Table 1 in particular.*

Revision: Text changed in the revised paper:

> On page 4852, line 14: "Re (Re = $u_D \cdot d_{mean}/\nu$ where ν is the kinematic viscosity of the air) is the corresponding Reynolds-number of the flow."

Comment #27: p. 4852/22: *As the air in the pore spaces are always -> As the air in the pore spaces {is} always*

Revision: Text changed in the revised paper:

> On page 4852, line 22: "As the air in the pore space is always …"

Comment #28: p. 4855/14: *Similarly to Abstract: "… impact on atmospheric chemistry. -> "… impact on atmospheric chemistry {and isotope contents in snow}.*

Revision: Text changed in the revised paper:

> On page 4855, line 14: "… impact on atmospheric chemistry and isotope contents in snow."

Comment #29: Figure 3, caption: *108 h for (left panel) "ota3" and (right panel) "ota4". -> 108 h for "ota3" (left panel) and "ota4" (right panel).*

Revision: Caption changed in the revised paper:

> On page 4863, caption: "Superposition of vertical cross-section parallel to the flow direction at time 0 and 108 h for "ota3" (left panel) and "ota4" (right panel)."

Comment #30: Figure 4, caption: *In the caption a reference to (b) is missing. Also the label of y-axis for (d) uses different symbol from the one used in the main text. Should be consistent (k_e v.s. k_cond).*

Revision: Caption changed in the revised paper:

> On page 4864, caption: "Temporal evolution of (a) the mean pore size, d_{mean}, of the snow samples obtained by opening-size distribution, (b) the porosity, ε, obtained by triangulated structure surface method, (c) the specific surface area, SSA, of the ice matrix obtained by triangulated structure surface method, and (d) the effective thermal conductivity of the snow sample, k_{cond}, estimated by DPLS simulations."

Text changed in the revised manuscript:

> On page 4849, line 3: "… is analyzed by the effective thermal conductivity k_{cond} …"

Minor revisions were made throughout the revised manuscript.

We thank E. A. Podolskiy for his scrutiny and recommendations.

The authors