RESPONSE TO REFEREES' F. FLIN COMMENTS

TO MANUSCRIPT TC-2015-158

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

Authors: P. P. Ebner, M. Schneebeli, and A. Steinfeld

We thank the referee F. Flin for the constructive comments. All page and line numbers correspond to those of the Discussion Paper.

We reply to the general questions of the reviewer specifically in the section "Specific comments", as all general questions of the review are given with more details in this section.

<u>REVIEWER: F. Flin</u>

This work deals with the effect of saturated air (T_{air} = T_d = -14°C?) warming up progressively (typically from -14°C to -12.5°C) into snow samples submitted to an imposed temperature gradient (TG) of about 50 K/m. 4 samples were submitted to different flow velocities (from 0 to 3 L min^{-1}) and the evolution of their inner parts (probably 7x7x7 mm3 volumes – this is to be confirmed by the authors) was monitored by X-ray tomography with a pixel size of probably 20 micrometer over 108 h. The evolutions of density, specific surface area (SSA), mean pore diameter and vertical component of thermal effective conductivity were computed from the 3D images obtained. Based on these observations, the authors conclude that the circulation of air, as defined in their settings, impacts the microstructure but not the overall ice mass at the scale of the observed volume. They compare this result to those of a previous paper (Ebner et al, 2015b), where the saturated air is progressively cooled down and propose physical explanations for the differences observed.

This is potentially a good paper that proposes, as the previous papers of Ebner et al, (2015a) and Ebner et al, (2015b) an interesting way to study the impact of air circulation in snow. This time, the case of a saturated air flow progressively warming up in a snow sample is addressed, which consists in one of the possible cases of air advection in snow.

Among several topics, this paper has possible implications for the study of snow subjected to wind (e.g. Seligman 1936; Champollion et al 2013), permeability measurements (e.g. Jordan et al, 1999; Arakawa et al, 2009; Domine et al, 2013) and a better understanding of the matter redistribution mechanisms occurring in TG snow metamorphism (e.g. Flin and Brzoska, 2008; Kaempfer and Plapp, 2009; Pinzer et al, 2012; Calonne et al, 2014; Wang et al, 2014; Krol and Löwe, in press).

This paper needs, however, improvements and clarifications before publication. Here are my main concerns, with some suggestions (see “specific comments” for more details):

1. Missing information and unclear definition of the physical problem:

1.1 The presentation of the physical problem needs to be improved. In particular, the concept of "saturated air", which is extensively used by the authors, is very ambiguous as soon as the snow sample is submitted to a TG. The authors should state more clearly the experimental conditions, and define precisely to which temperature the air is saturated (using the Td notation for the dew point might help). From a strict presentation point of view, such crucial information should appear as soon as possible in the paper.

1.2 Other important information such as voxel size and the size of the region of interest (ROI) used for the computations should be mentioned in the paper.

1.3 Also, no information about the vertical position of the ROI is available. This is, however, an important parameter that should be taken into account in the physical interpretation of the experiment. See e.g. comment 4848/section 2.

2. Problems with the presentation of the physical analysis: Logical links are sometimes difficult to follow and some explanations about the involved physical mechanisms should be improved. See e.g. comments 4845/l6-7 and 4851/21-23.

3. Data post-processing and reliability: The experiment is really interesting, but some "unusual" results obtained need to be checked, clarified or discussed. It is in particular the case of:

- Some erratic translations or changes that can be observed in the image series (see the enlarged version of Fig. 2 in supplementary materials). Has each image that constitutes Fig. 2 been spatially repositioned thanks to adequate references? The beginning of the series ota3 is especially problematic. For most series, some slight but persistent downward translations are observed and should also be commented. See e.g. comment 4862/Fig 2.

- Fig. 3, which is a bit difficult to "read" (no classical rounding or TG effect) and exhibits some post-processing artefacts. See comments 4863/Fig. 3 for suggestions.

- The ota1 series, which does not show any increase of the vertical component of its conductivity when submitted to a TG only. At least a comment should be written on this topic. See comment 4852/27-4853/2.

- Ideally, a characterization of the structural anisotropy of the snow samples would be appropriate. See comment e.g. 4850/7-8.

4. Abstract would benefit from a reformulation: it lacks basic, but important experimental information and do not give a sufficiently clear summary of the physical process occurring in the samples.

5. The title does not describe precisely the experiment and could be improved. Here is a suggestion: "Tomography-based observation of snow metamorphism under a saturated air flow progressively warming up inside the snow sample".

Specific comments:

Comment #1: 4845/Title: *Tomography-based observation of sublimation and snow metamorphism under temperature gradient and advective flow*

1) "temperature gradient and advective conditions": this formulation does not really allow the reader to distinguish the present title from the title of Ebner et al, 2015b.

From the experimental conditions, it appears that snow is always placed in slight but undersaturated air flow, as the incoming air is saturated with respect to the colder temperature of the sample. Mentioning this important fact in the title would be pertinent. At least, from a purely didactic point of view, this would help the reader to understand quickly why sublimation occurs (abstract l. 6).

2) This is a minor point but the wording "sublimation and snow metamorphism" seems unusual as local sublimation is generally assumed (with condensation) as being a part of the metamorphism process itself. Maybe replace the title with: "sublimation during snow metamorphism"?

See also main comment #5.

Response: We reformulated the title

“Metamorphism during temperature gradient with undersaturated advective airflow in a snow sample.**“**

Comment #2: 4846/Abstract: Clear explanations about the direction and intensity of the TG and of the air flow are missing here. Are the air flow velocity and TG collinear? Are they pointing toward the same direction? Which TG and air velocity ranges are concerned? Which part of the sample was observed (top, middle-height, etc.)? These pieces of information are very basic, but mandatory to understand the exact topic of the paper.

Revision: Text added in the revised paper:

> On page 4846, line 6: “Cold saturated air at the inlet was blown into the snow samples and warmed up while flowing across the sample. The temperature gradient in the sample was around 50 K m^{-1} at maximum airflow velocity.”

Text changed in the revised paper:

> On page 4846, line 6: “The sublimation of ice for saturated air flowing across the snow sample was experimentally determined via changes of the porous ice structure in the middle-height of the snow sample.”

Comment #3: 4846/l6-7: *The sublimation of water vapor for saturated air flowing across the snow sample was…*

1) As we are in TG conditions, please define to which temperature the air is saturated (top, middle or base of the snow sample?), i.e., give the dew point of the incoming air.

2) "sublimation of ice" would be preferable to "sublimation of water vapour"

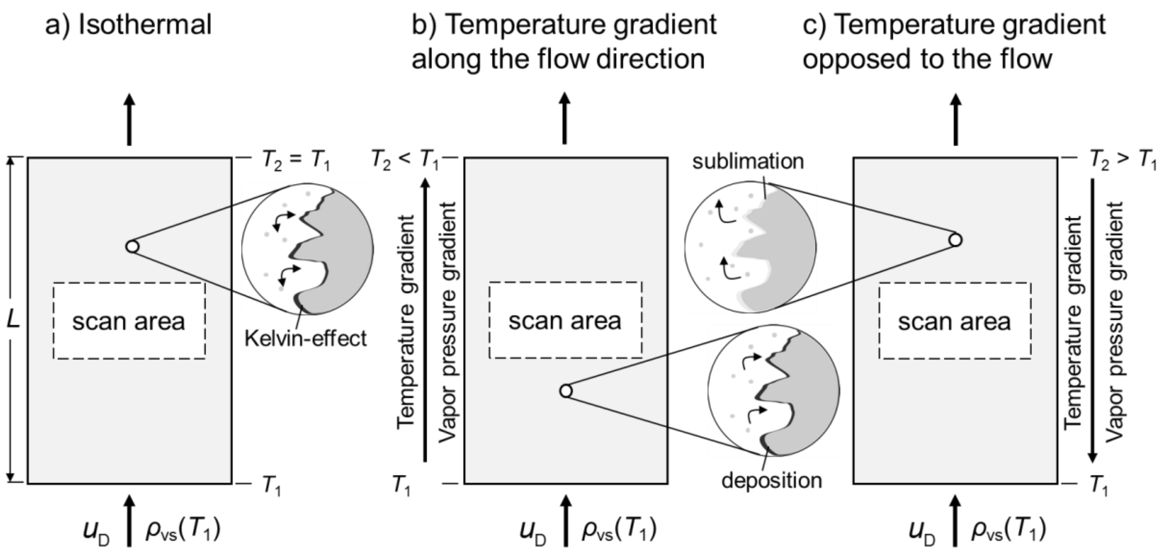
Revision: Text added in the revised paper:

> On page 4845, line 6: “Cold saturated air at the inlet was blown into the snow samples and warmed up while flowing across the sample. The temperature gradient in the sample was around 50 K m^{-1} at maximum airflow velocity.”

Text changed in the revised paper:

> On page 4845, line 6: “The sublimation of ice for saturated air flowing across the snow sample was experimentally determined via changes of the porous ice structure in the middle-height of the snow sample.”

Figure 1 changed in the revised paper:



<u>Comment #4:</u> 4845/8-10: *The results showed that the exothermic gas-to-solid phase change is favorable vis-a-vis the endothermic solid-to-gas phase change, thus leading to more ice deposition than ice sublimation.*

I can hardly understand the meaning of this sentence in the context of the current problem, where all the processes seem to be rather well explained with combined TG and undersaturation effects. Do the authors suspect latent heat effects to take a significant role in the process described? If so, additional explanations should be added to the text. Otherwise, removing this sentence would clarify the conclusion of the paper.

Response: Text removed in the revised paper.

<u>Comment #5:</u> 4846/13-14: *However, the strong reposition process of water molecules on the ice grains is relevant for atmospheric chemistry.*

The sentence seems a bit strange. Why is this process relevant to atmospheric chemistry? No additional element is available in the paper. A possible alternative would be "However, the strong reposition process of water molecules in snow may impact its isotopic or chemical content."

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."

<u>Comment #6:</u> 4847/26-28: *In the present work, we studied the surface dynamic of snow metamorphism under an induced temperature gradient and saturated airflow in a controlled laboratory experiments.*

Again, please define more explicitly if the airflow is saturated with respect to the top, the middle or the base of the sample. Giving the typical temperatures of the sample (T_top = -12.5°C and T_base = -14°C) and the temperature and dew point of the incoming air (T_air = - 14°C and Td_air = -14°C?) as soon as possible in the paper would really help the reader.

Revision: Text added in the revised paper:

> On page 4847, line 28: "Cold saturated air at around -14 ºC was blown into the snow samples and warmed up to around -12.5 ºC while flowing across the sample."

<u>Comment #7:</u> 4848/2: What is the meaning of "discrete-scale geometry"? Do you just mean "discrete geometry" (or "digitized geometry") or something more specific?

Response: We use the term "discrete-scale geometry" in contrast to "continuum-scale geometry".

Revision: Text added in the revised paper:

> On page 4848, line 2: "By using discrete-scale geometry, all structures are resolved with a finite resolution corresponding to the voxel size."

<u>Comment #8:</u> 4848/section 2: no information about the voxel size of the images is clearly available. It is however a very crucial point when considering the changes occurring in a metamorphosing snow microstructure (especially for a rather short time period of 108 h at about -14°C).

Important information such as the selected ROI in the sample (size and position inside the sample) needs also to be added. As the authors know, the vertical position of the ROI is especially important as soon as TG metamorphism is concerned. For example, if a snow sample is submitted to vertical gradient (Tbase=-12.5°C, Ttop=-14°C) with no air flow, the base is known to undergo strong sublimation while the top undergoes condensation: people observing only the upper part of the sample would conclude into an ice mass increase, while people looking at the base part would reach exactly opposite conclusions. The central part is however known to be constant in density (Schneebeli and Sokratov, 2004; Srivastava et al, 2009; Calonne et al 2014), which is consistent with what is observed in this experiment. It should however be noticed that additional air flow makes the problem much more complex, exhibiting supplementary reasons for vertical variations depending on air flow velocity (see e.g. Fig. 5 of Calonne et al, 2015).

Revision: Text changed in the revised paper:

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

Text added in the revised paper:

> On page 4848, line 20: "The innermost 36.9 mm of the total 53 mm diameter were scanned, and subsamples with a dimension of 7.2 mm × 7.2 mm × 7.2 mm were extracted for further processing. The imaged volume was in the center of the sample (Fig. 1)"

Comment #9: 4848/11-13: *Natural identical snow produced in a cold laboratory [Schleef et al., 2014] was used for the snow sample preparation (water temperature: 30 ºC; air temperature: −20 ºC).*

Such information on water and air temperatures seems particularly system-dependent. Would it be possible for the authors to indicate temperature and humidity (Td?) in the nucleation chamber? Are there specific reasons for the choice of snow type C1f/G6 ("hollow columns with germs" as described by Schleef et al, 2014)?

Response: We agree that this information is system dependent. Schleef et al. (2014) estimated the corresponding temperature and humidity in the chamber. As the snow was isothermally stored during more than 27 days before use (-5 °C), the original snow type was not any more recognizable in the used samples

Comment #10: 4848/25-27: *The segmented data were used to calculate a triangulated ice matrix surface and tetrahedrons inscribed into the ice structure. Morphological parameters such as porosity (epsilon) and specific surface area (SSA) were then calculated.*

As stressed in preceding reviews, several mesh methods are known to provide biased estimations of SSA (see e.g. Flin et al, 2011).

Response: We agree that the calculation of the SSA is not trivial. In our case, we used the algorithms of Haussener et al (2012), which are validated (Zermatten et al., 2014). In addition, we use here mainly the ratio between SSA/SSA_0 to investigate the trend and the evolution of this parameter to see the influence of advective airflow and metamorphism.

Comment #11: 4849/1: *Opening size distribution was applied…* -> "An opening-based morphological operation was applied…"

Revision: Text changed in the revised paper:

<u>Comment #12:</u> 4849/18: *The morphological evolution was similar between all four experiments and only a slight rounding and coarsening was visually observed, shown in Fig. 2, indicating that the initial ice grain did not change with time. Only coarsening processes of the ice grain were observed for example, Fig. 3 shows the locations of sublimation and deposition for "ota3" and "ota4".*

These sentences are not very clear and some assertions seem to contradict each other.

Revision: Text added in the revised paper:

> On page 4849, line 18: "The morphological evolution was similar between all four experiments and only a slight rounding and coarsening was visually observed, shown in Fig. 2. The initial ice grain did not change with time and the locations of sublimation and deposition for "ota3" and "ota4" is shown in Fig. 3."

<u>Comment #13:</u> 4850/3: *the temporal porosity distribution* -> "the temporal evolution of the porosity"

Revision: Text changed in the revised paper:

<u>Comment #14:</u> 4850/7-8: *A coarsening was observed for each experiment but the influence of changing airflow was not visible, confirmed by the temporal SSA evolution, shown in Fig. 4c.*

Have the authors tried to quantify snow structural anisotropy? From detailed observation of the final images of Fig. 2 (see supplementary material), it seems the higher the velocity, the less horizontally-layered the structure.

Response: Although a change in the anisotropy was observed in previous TG experiments we didn't quantify the snow structural anisotropy in our observations.

<u>Comment #15:</u> 4850/9-11: *Although the repositioning of water molecules led to a smoothing of the ice grains, it did not affect the heat transfer in the snow. The thermal conductivity slightly increased after applying airflow to the temperature gradient…*

Heat transfer is actually made of different contributions (conduction, convection, radiation, latent heat…) but the authors limited their computations to the determination of the vertical component of the effective conductivity from the obtained tomographic images. This formulation might be more adequate: "The repositioning of water molecules led to a smoothing of the ice grains, but did not affect the conductivity of snow. This quantity slightly increased after applying airflow to the temperature gradient."

Revision: Text changed in the revised paper:

> On page 4850, line 9-11: “The repositioning of water molecules led to a smoothing of the ice grains, but did not affect the thermal conductivity of snow. This quantity slightly increased after applying airflow to the temperature gradient ...”

<u>Comment #16:</u> 4850/16-18: *The kinetic phase-change from gas to solid is preferable over solid to gas as energy is released rather than consumed leading to more ice deposition rather than ice sublimation.*

Like in the abstract, this sentence sounds very strange to me, and is neither supported by what follows in the discussion nor by the reported experiment. See also lines 4854/21/22 in the text.

Response: Text removed in the revised paper.

<u>Comment #17:</u> 4851/3-4: *The superposition of vertical cross-section in Fig. 3 shows a big effect on reposition of water molecules on the ice structure.*

Due to the acquisition process (slight variability of the X-ray source leading to small differences in the reconstruction parameters, e.g.), the 3D images can generally undergo tiny translations and rotations with time. Has each image that constitutes Fig. 3 been spatially repositioned thanks to adequate references? How was it done? See also comment 4862/Fig. 2.

Response: Yes, we repositioned the images to adequately reference. We used a linear encoder with a resolution of less than 1 voxel to verify that the scans were taken at the same position. We added a sentence to the description of the methods.

Revision: Text added in the revised paper:

> On page 4848, line 20: “A linear encoder with a resolution of less than 1 voxel was used to verify that the scans were taken at the same position.”

<u>Comment #18:</u> 4851/5-6: *Continued sublimation and deposition of water molecules due the Kelvin-effect led to a saturation of the pore space.*

Does this sentence concern the snow samples before starting the experiment? In the described experiment, we have:

1) a significant temperature gradient;

2) an air advection effect, which brings cold and dry air onto slightly warmer ice surfaces; Is really Kelvin effect (also known as curvature effect) occurring in this case? What about Clausius-Clapeyron equation? Some physical explanations of the whole paragraph are rather difficult to follow and should be clarified.

Response: Yes, this sentence concern the snow samples before starting the experiments.

Revision: Text added in the revised paper:

> On page 4851, line 5-6: "The vapor pressure of the air in the pore was in equilibrium with the water pressure of the ice, given by the local temperature."

Comment #19: 4851/15-18: *...sublimation inside a snowpack has a significant influence not on the total net mass change but on the structural orientation of the ice grains due to redistribution of water vapor on the ice matrix*.

1) What does "sublimation inside a snowpack" actually mean? It seems it would be better to replace this wording by "advection of cold saturated air into a slightly warmer snowpack".

2) It is also important to notice that the experiment presented in the paper does not allow drawing any conclusion on potential "skin effects", i.e. on what happens near the interfaces (snow-air interface, interface between 2 distinct layers in the snowpack…). However, "skin effects" are particularly important as far as snowpack, TG and undersaturated flow are considered.

3) Also, what is actually meant by "structural orientation"? I there a way to quantify this impact? See also comment 4850/7-8. 4851/21-23: *Our results support the hypothesis of Neumann et al. (2009) that sublimation is limited by vapor diffusion into the pore space rather than sublimation at crystals faces.*

I could not understand the logical process by which the author reached this conclusion. Would it be possible to improve the related explanations?

Response: Optically we can see a change of the structural orientation. However, as we didn't calculate the anisotropy we will reformulate the sentence.

Revision: Text changed in the revised paper:

> On page 4851, line 15-18: "…advection of cold saturated air into a slightly warmer snowpack has a significant influence not on the total net mass change but on the

structural change of the ice grains due to redistribution of water vapor on the ice matrix."

<u>Comment #20:</u> 4853/27-4853/2: *Thermal conductivity changed insignificantly in these experiments of short duration. This indicates that advective cold airflow opposite to a temperature gradient reduces or suppresses the increase in thermal conductivity usually observed by temperature gradient metamorphism (Riche and Schneebeli, 2013).*

It should be noticed that this stable conductivity is also true for experiment ota1, which is a pure TG experiment as it occurs without any air advection: why an increase in the vertical component of the thermal conductivity is not observed in this particular case? Are the results sufficiently reproducible (difficulty of precise temperature control on a small snow sample, problem of representative elementary volume for the computation of thermal conductivity, dependence of the morphology with the vertical position of the investigated sample, etc.) to draw conclusions on this topic? Are there other reasons to explain the conductivity evolution of ota1?

Response: An increase of the vertical component of the thermal conductivity is not observed for 'ot1' because one reason could be that we had an open TG experiment system. Compared to closed TG experiment, the temperature gradient induce an air movement and therefore has an additional influence on the thermal conductivity.

Revision: Text changed in the revised paper:

> On page 4853, line 27 – page 4853, line 2: "Thermal conductivity changed insignificantly in these experiments. This indicates that advective cold airflow opposite to a temperature gradient and an open system reduces or suppresses the increase in thermal conductivity usually observed by temperature gradient metamorphism (Riche and Schneebeli, 2013)."

<u>Comment #21:</u> 4855/8-10: *Conditions (1) and (3) showed that they have a negligible effect on the structural changes of the ice matrix and can be neglected to improve models for firn compaction and evolution.*

For conditions (3), this sentence seems in contradiction with some parts of the paper, where the morphological changes are considered as significant in 108 h of experiment (see e.g Fig. 3 and 4851/3-4).

Response: Correct, but looking at the temporal porosity evolution there is no change observable and therefore it can be neglected to improve models for snow compaction and evolution at the surface.

Revision: Text changed in the revised paper:

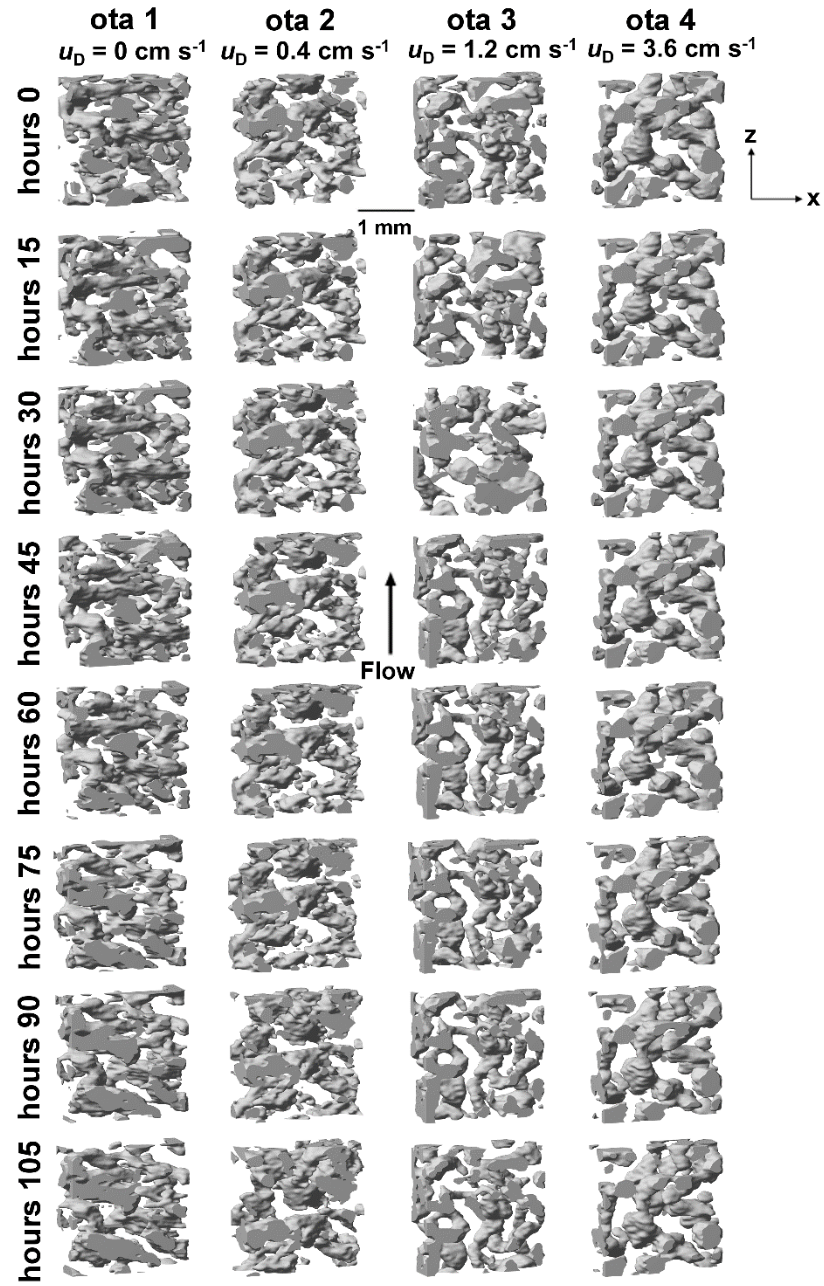
> On page 4855, line 8-10: “Conditions (1) and (3) showed that they have a negligible effect on the porosity evolution of the ice matrix and can be neglected to improve models for snow compaction and evolution at the surface.”

Comment #22: 4862/Fig. 2: Please add flow velocities in the figure and voxel size in the caption. From the enlarged view (supplement file) it can be noticed that microstructures show erratic translations with time: it is the case for ota1 (between 0 and 15 h) and ota2 (between 75 and 90 h) where fast downward translations occur. Is it an artefact of the image acquisition process or is it linked to a physical process (snow settling under its own weight (Schleef et al, 2014a), which is generally considered as unlikely under significant TG, sublimation of underlying snow, etc.)? Artefacts should be corrected and physical processes explicitly mentioned in the article's text.

For ota3, the series show clearly erratic vertical translations between 0 and 30 h, with a completely different structure at 30 h, before returning to something more like the original image after 45 h. Could these positioning problems be related to the strong variations observed in conductivity (Fig. 4 – ota3) between 0 and 30 h? Please check.

See also comment of 4850/7-8.

Revision: Figure 2 and caption changed:



Caption Figure 2:

> "Evolution of the 3-D structure of the ice matrix with applied temperature gradient and advective conditions. Experimental conditions (from left to right) at different measurements times from beginning to the end (top to bottom) of the experiment. The shown cubes are 110 × 40 × 110 voxels (2 mm × 0.7 mm × 2 mm) large with 18 µm voxel size (a high resolution figure can be found in Supplement)."

Text added in the revised paper:

> On page 4849, line 19: "The change of structural change "ota 3" at 30 h is due to an error in the scan."

<u>Comment #23:</u> 4863/Fig. 3: is a bit deceiving: in particular, it exhibits a voxel-size horizontal layering, which is typically obtained when image processing algorithms (median and Gaussian filters, threshold…) are only applied in 2D on horizontal cross-sections without considering the vertical direction. I suggest really using a 3D threshold method to improve the quality and reliability of the images. If necessary, some indications and references can be found in e.g., Hagenmuller et al, 2013 (p. 862-863).

It seems also difficult to deduce a general trend for sublimation and deposition sites. Maybe a 3D view with a color code proportional to the measured interface speed or an adequate graph (see e.g. Krol and Löwe, in press) would help.

Response: For the μ-CT80 (Scanco Medical) it would not make sense to use a 3D threshold method. The device scans the sample stack by stack having a slightly difference in the intensity for each stack. Therefore, we decided to segment each 2D section separately using the Otsu method.

A measured interface speed graph is out of the scope for this paper as we are not interested in the interface speed and if the editor agrees, we suggest not to include such graphs.

<u>Comment #24:</u> 4864/Fig. 4: please specify (caption and/or text) the sizes of the samples on which these properties have been obtained. Are volumes representative for the considered properties?

Revision: Text added in the revised paper:

> On page 4848, line 20: "The innermost 36.9 mm of the total 53 mm diameter were scanned, and subsamples with a dimension of 7.2 mm × 7.2 mm × 7.2 mm were extracted for further processing."

<u>Technical comments:</u>

<u>Comment #1:</u> 4847/28: *experiments* -> "experiment"

Revision: Text changed in the revised paper:

<u>Comment #2:</u> 4847/10 *whistler-like crystals* -> "whisker-like crystals"

Revision: Text changed in the revised paper:

<u>Comment #3:</u> 4848/6: *in a cold laboratory temperature* -> “at a cold laboratory temperature”

Revision: Text changed in the revised paper:

<u>Comment #4:</u> 4848/11: *Natural identical snow* -> “Nature identical snow”

Revision: Text changed in the revised paper:

<u>Comment #5:</u> 4849/18: *initial ice grain* -> initial ice grains

Revision: Text changed in the revised paper:

<u>Comment #6:</u> 4849/19: *the ice grain* -> the ice grains

Revision: Text changed in the revised paper:

<u>Comment #7:</u> 4851/5: *analyze volume* -> analyzed volume

Revision: Text changed in the revised paper:

<u>Comment #8:</u> 4855/11: *seem* -> “seems”

Revision: Text changed in the revised paper:

<u>Comment #9:</u> 4864/Fig. 4: A "(b)" is missing in the caption.

Revision: Text insert in the revised paper:

Minor revisions were made throughout the revised manuscript.

We thank F. Flin for his scrutiny and recommendations.

The authors