Report on the revised version of paper tc-2015-18: Tomography-based monitoring of isothermal snow metamorphism under advective conditions
P. P. Ebner, M. Schneebeli, and A. Steinfeld

The authors have significantly improved the scientific consistency of the manuscript. I recommend publication after the following comments have been taken into account.

Specific comments:

11-13: *The effect of diffusion and advection across the snow pores on the snow microstructure were analysed in controlled laboratory experiments and further elaborated on natural snowpacks.*
I suggest removing ñand further elaborated on natural snowpacks.ò as it does not seem realistic to extrapolate the results obtained on 4 samples (2 snow types) under very specific conditions to all natural snowpacks.

13-15: *The 3D digital geometry obtained by tomographic scans was used in direct pore-level numerical simulations to determine the effective transport properties.*
Actually, only permeability has been computed (no Dupuit-Forchheimer coefficient, no vapor diffusivity, no thermal conductivityé). Replacing ñtransport propertiesò by ñpermeabilityò would be more informative and accurate.

17-19: *Isothermal snow metamorphism is driven by evaporation-deposition caused by the Kelvin effect and is the limiting factor independently of the transport regime in the pores.*
This is just a suggestion, but the most appropriate terminology would probably be ñsublimation-depositionò. Please consider using it more systematically throughout the paper.

81-82: *Two different snow types with high specific surface area were considered to evaluate the structural change in the earlier stage of isothermal metamorphism of new snow.*
This sentence is unclear. Do you just refer to sa1 and sa2 series or to all series? The sentence is ambiguous because sa1 and sa2 have probably the same snow type while sa3 and sa4 have quite moderate SSAs and cannot be considered as new snow samples. Please clarify these points in the text.

79-87: More generally, please use these lines to clearly define all the experiments sa1, sa2, sa3 and sa4 (and their corresponding snow types according to the international Classification).

99-108: *Assuming an isothermal snowpack, Pe > 1 is unlikely in nature because of: 1) low density snow, which has always a very low strength, will be destroyed due to the high airflow velocity; 2) Pe > 1 would be possible for depth hoar, but this snow type is typically found at depth and rarely exposed to high windspeed (Colbeck, 1997); 3) Pe depends on the temperature due to changing diffusivity. Seasonal temperature fluctuations of -60 ºC to -30 º C are typical for surface snow layer in Antarctic regions, and lead to Pe variations of up to 25%. Theoretically, Pe* **å** *1.2 could be realistic at -60 ºC for* **ó***a4***ô** *However, simulations by Neumann (2003) showed a rapid decrease of the airflow velocity inside the snow layer (***Ò** *0.01 m s-1) for a high wind speed (***å** *10 m s-1) above the snow surface (pore size* **å** *1 mm). This leads to a maximum Pe* **å** *0.8.*
I agree with these explanations, but they are still not perfectly convincing and need to be moderated by the following facts:
-the upper part of an ñisothermal snowpackò is generally constituted of moderate density snow (RG, typically), which may have, at the same time, relatively high strength and high pore size -and would consequently allow much higher Pe numbers (see my comment 1024/14-15 and the related table of my previous report on this paper).

-depth hoar structures can also appear close to the surface (Alley et al, 1990; Gallet et al, 2013; Adams and Walters, 2014é).

-depth hoar structures that formed under TG can then undergo isothermal conditions: the snowpack would then be isothermal, while its inherent structure (and Pe) would be much closer to that of depth hoar.

-The pore diameter dp, which is used in Pe estimations, is often estimated qualitatively and can easily change the Pe number by a factor 2 (again, see comment 1024/14-15 of my previous report). Consequently, distinguishing values of Pe between 0.8 and 1.2 may seem quite useless in this context. By the way, the authors still do not give clear information in the paper about the method they used to compute dp (see also comment of line 398).

I agree that generally Pe < 1, but there can be some occurrences for which Pe would be higher in natural snowpacks. As the authors have just investigated the case were Pe < 1, I think it is easier to state it explicitly (e.g., in the title, abstract and conclusions of the paper) rather than trying to demonstrate Pe > 1 is impossible.

106: sa4 has not been defined previously.

165-168: *The mass sublimated preferentially at locations of the ice grain with low radii due to Kelvin-effect and was relocated on the grain leading to a smoothing of the ice grain.*

1) The sentence does not seem perfectly clear to me. Here is a suggestion: ñDue to Kelvin effect, the mass sublimated preferentially from convexities of the ice matrix to be relocated on its concavitiesò

2) Actually, this observation is not really obvious from fig 6, which is quite deceiving and cumulates 2 problems:

-1st, it exhibits a voxel-size horizontal layering, which is typically obtained when image processing algorithms (median and Gaussian filters, thresholdé) 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).

-2nd, the evolutions shown are not really typical from isothermal metamorphism since deposition often seems to occur on convexities rather than on concavities (see e.g. fig 6b as compared to fig 2. of Brzoska et al, 2008, and figs of Calonne et al, 2013). Please check once the vertical cross sections have been re-processed.

210-213: *Experiment ósa1ô and ósa2ôhad a higher value of n, indicating a strong coarsening process due to sintering and that surface processes were rate limiting (Legagneux et al., 2004; Legagneux and Domine, 2005).*

This sentence is unclear and seems inconsistent with the previous sentence. Do these experiments have higher value than 2 or than 3? Higher value than sa3 and sa4?

225-227 (and below): *The effect of decreasing SSA on the permeability was not elucidated in our experiments. A SSA decrease of at least 5% in the experiments could not be reproduced in the permeability.*

Based on the fact that, at constant density, permeability is very well known to increase with decreasing SSA (see e.g. Shimizu, 1970; Courville et al, 2010; Calonne et al, 2012; Calonne et al, 2014), these 2 sentences seem strange. From a presentation point of view, I suggest slightly reformulating this subsection by:

-1) describing the results that have been obtained by the authors (with reference to fig. 11)

-2) commenting the relationships with density, by referring briefly to the results of the literature.

-3) commenting the relationship with SSA, by referring briefly to the results of the literature. It has no need to be long, but some basic references should be present.

229-230: *The effect of increasing airflow velocity had no influence on the flow characteristics.*
Maybe replace by ñThis means that the effect of increasing airflow velocity had no influence on the flow characteristics.ò

238-239: *This difference could therefore be due to an error during the measurement.*
I am still not convinced by this point and suspect other reasons than errors in experimental setup or tomographic acquisitions (see my previous report).

243-244: *The two main transport processes, diffusion and advection, were analysed inside the pore space*
Actually, the processes themselves were not analyzed. Maybe replace by: The effects of the main transport processes, diffusion and advection, were analyzed inside the pore space.

245-246: *Pe > 0.85 were not possible due to the destruction of the snow structure*
I am still not convinced, as some typical snow types would probably allow higher Pe numbers without damage. Have the authors really tried ñoldò RG, DH or MF samples in their experiments? See table below:

Maximal velocities and Pe numbers using eq. 2 of Ebner et al, (2014) for images s2 and s4, as well as for other data available from Calonne et al, (2012) (http://www.the-cryosphere.net/6/939/2012/tc-6-939-2012-supplement.pdf):

type	name	density	vol. frac. (x)	K	A	V	SSA	dg	dp	u_max	Pe_max
								6/(SSA*917)	dg*(1-x)/x	eq 2	
	sa2	186,1	0,202944384	3,15E-09	0,00175845	5,27535E-05	43,7	1,50E-04	5,88E-04	**6,48E-02**	**1,87268512**
	sa4	264,93	0,288909487	2,20E-09	0,0015688	4,70639E-05	28	2,34E-04	5,75E-04	**9,18E-02**	**2,59251523**
PP	Fr	120,49	0,131395856	3,69E-09	0,0019163	5,7489E-05	55,3	1,18E-04	7,82E-04	**3,18E-02**	**1,22313609**
PP	I01	102,9	0,11221374	3,33E-09	0,00195862	5,87586E-05	55,79	1,17E-04	9,28E-04	**2,10E-02**	**0,95501965**
RG	I23	256	0,27917121	2,47E-09	0,00159028	4,77084E-05	17,24	3,80E-04	9,80E-04	**9,62E-02**	**4,63054836**
RG	NH0	431,36	0,47040349	4,30E-10	0,00116839	3,50516E-05	17,34	3,77E-04	4,25E-04	**4,76E-02**	**0,99221318**
DH	Grad3	369	0,402399128	1,06E-09	0,00131842	3,95525E-05	21,84	3,00E-04	4,45E-04	**8,58E-02**	**1,87452598**
DH	7A-G	311,23	0,339400218	4,84E-09	0,0014574	4,37221E-05	13,42	4,88E-04	9,49E-04	**2,79E-01**	**12,9871354**
MF	H03	498,11	0,543195202	1,73E-09	0,0010078	3,02339E-05	5,25	1,25E-03	1,05E-03	**2,55E-01**	**13,1323776**
MF	H05-G	471	0,513631407	4,87E-09	0,00107302	3,21906E-05	3,78	1,73E-03	1,64E-03	**6,42E-01**	**51,692107**

D	l	mu	g	r
2,04E-05	0,03	1,80E-05	9,81	0,0265

All quantities are given in SI units.

NB: for dp, an estimation using SSA was used in the table above. While this estimation is very rough, it is consistent with the fact that:
-dg is much smaller than dp for recent snow
-dg is nearly equal to dp for MF snow samples grown in saturated water.
-dp values for sa2 and sa4 are about the same order of magnitude (2 times higher, actually) that those given by the authors

246: *and is not realistic in natural snowpacks*
→ and is not frequent in natural snowpacks

398: Table 1: the method used to compute dp (initial pore diameter) is still not explained in this version of the manuscript: was dp estimated according to Dullien (1992)? If so, please specify the formula in the text. Depending on the estimation used, the pore diameter may be 2 times higher than the presently given values (see comment 1024/14-15 of my previous report): this would then result in an increase of all the computed Pe numbers by a factor 2.

425-427: *The accurateness of the isothermal conditions between the top and base of the sample throughout the experiment is less than 0.2 ºC.*

It is to be noticed that such a temperature difference between the top and bottom induces a temperature gradient of about 6.7 K/m for the used samples (3 cm high snow samples), which can be considered as already far from perfect isothermal conditions.

458-460: fig 2
Presently, only the meshes of the inlet and outlet parts are visible. I suggest providing an enlarged view of the mesh in the snow sample area, which would really allow the reader for estimating the mesh quality.

Technical comments or suggestions:

38-41: maybe suppress ñin isothermal metamorphismò

44 and 51: *snow pack*
→ snowpack

51-52: *It is suggested that advective flow of air has a direct effect on snow-air exchange processes related to atmospheric chemistryé*
→ Advective flow of air may have a direct effect on snow-air exchange processes related to atmospheric chemistryé

79-81: please move the reference (Ebner et al, 2014) just after ñexperimental setupò or change the sentence. Otherwise the referencing seems ambiguous.

83-84: *Natural identical snow*
→ Nature identical snow

91: uD → u_D

98: *simulation*
→ simulations

152-153: *A longer sintering duration at higher temperature*
→ A longer sintering duration at -5°C

161: *The initial ice grain didnôt change with time.* → The initial ice matrix didnôt change significantly with time.

186: suppress ñheightòbetween ñsnowòand ñforò

195: *(Legagneux and Domine, 2005*)
→ Legagneux and Domine (2005)

203: *real mechanism*
→ real mechanisms

251-252: *The experimental observations supported the hypothesis that further densification was limited by coarsening kinetics and further confirmed a constant porosity evolution*
Maybe change ñfurther densificationòinto just ñdensificationò

253-254: *small surface radii*
→ small curvature radii

References

Adams E. E., and D. J. Walters, 2014. Fine structure layering in radiation recrystallized snow, ISSW Proceedings.

Alley, R. B., Saltzman, E. S., Cuffey, K. M., Fitzpatrick, J. J., 1990: Summertime formation of Depth Hoar in central Greenland, Geophysical Research Letters, 17(13), 2393-2396.

Brzoska, J.-B., F. Flin and J. Barckicke, 2008. Explicit iterative computation of diffusive vapour field in the 3-D snow matrix : preliminary results for low flux metamorphism, Ann. Glaciol., 48, 13-18.

Calonne, N., F. Flin, C. Geindreau, B. Lesaffre and S. Rolland du Roscoat, 2014. Study of a temperature gradient metamorphism of snow from 3-D images: time evolution of microstructures, physical properties and their associated anisotropy, The Cryosphere, 8, 2255-2274, doi: 10.5194/tc-8-2255-2014.

Calonne, N., F. Flin, B. Lesaffre, A. Dufour, J. Roulle, P. Puglièse, A. Philip, F. Lahoucine, S. Rolland du Roscoat and C. Geindreau. A room temperature operating cryogenic cell for in vivo monitoring of dry snow metamorphism by X-ray microtomography, American Geophysical Union (AGU) Fall Meeting, San Francisco, USA, 9 - 13 December, 2013, poster.

Calonne, N., C. Geindreau, F. Flin, S. Morin, B. Lesaffre, S. Rolland du Roscoat and P. Charrier, 2012: 3-D image-based numerical computations of snow permeability: links to specific surface area, density, and microstructural anisotropy, The Cryosphere, 6, 939-951, doi: 10.5194/tc-6-939-2012.

Courville, Z., Hoerhold, M., Hopkins, M., and Albert, M., 2010: Lattice-Boltzmann modeling of the air permeability of polar firn, J. Geophys. Res., 115, F04032, doi: 10.1029/2009JF001549.

Dullien, F.A.L., 1992. Porous Media: Fluid Transport and Pore Structure. Academic Press.

Gallet, J.-C., Domine, F., Savarino, J., Dumont, M., and Brun, E., 2014: The growth of sublimation crystals and surface hoar on the Antarctic plateau, The Cryosphere, 8, 1205-1215, doi:10.5194/tc-8-1205-2014.

Hagenmuller P., G. Chambon, B. Lesaffre, F. Flin and M. Naaim, 2013: Energy-based binary segmentation of snow microtomographic images, J. Glaciol., 59(217), 859-873, doi: 10.3189/2013JoG13J035.

Shimizu, H., 1970: Air permeability of deposited snow, Contributions from the Institute of Low Temperature Science, A22, 1ï 32.