Comment on paper tc-2015-18: Tomography-based monitoring of isothermal snow metamorphism under advective conditions
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This work deals with the effect of saturated air circulation in isothermal snow. 4 samples were submitted to different flow velocities and the evolution of their inner parts (about 7x7x7 mm^3 volumes) was monitored by X-ray tomography with a pixel size of 18 micrometer during 4 days. The evolution of density, SSA and intrinsic permeability were computed from the 3D images obtained. Based on 3D local observations and the analysis of SSA evolution over this 4 day period, the authors conclude that the circulation of saturated air in isothermal snow does not impact the snow metamorphism. They finally extrapolate their results to all isothermal snowpacks, concluding that diffusion is always the main transport mechanism.

This is potentially an excellent paper that proposes an original way to study the impact of air circulation in snow. The fact that the authors restrict to saturated air and isothermal snow conditions, which is a very specific case of air advection in snow, might appear a bit frustrating. However, as a pioneering tomographic approach, it is an important and wise step toward more complicated experiments. Furthermore, this work has also direct implications for a better understanding of the matter redistribution mechanisms occurring in isothermal snow metamorphism (e.g. Kaempfer et al, 2007; Brzoska et al, 2008; Vetter et al, 2010) and potential applications for permeability measurement methods (e.g. Jordan, 1999; Arakawa et al, 2009; Domine et al, 2013).

That said, this paper needs major improvements before publication. Here are my main concerns, with some suggestions (see ñspecific commentsò for more details):

1. Diffusion vs. advection: in their analysis, the authors based on the fact that all Pe numbers are below 0.85 to claim that diffusion is the main transport mechanism. This approach is not rigorous enough. When deciding which phenomena are dominant and which ones can be neglected, characteristic numbers should be analyzed depending on the separation of scales of the problem. Using such an approach will show that a Pe number of 0.85 is conceptually not different from 1 (or from 0.08, e.g.) and that experiments sa3 and sa4 at least are in a regime where diffusion and advection contributions are both significant and cannot be neglected (see e.g. Auriault et al, 2009; Calonne, 2014 - chapter 3). It should also be noted that conditions where Pe is ñsignificantly largerò than 1 actually correspond to situations where transfers are driven by advection and dispersion (Bear, 1972 ï chapter 10), which is a distinct regime than that of the presently considered regime of diffusion-advection (i.e., for Pe > 1, Darcyôs Law is no more valid and inertial effects should be accounted for).
→ The authors should analyze the considered problem more carefully and adapt their conclusions accordingly.

2. There are some inconsistencies concerning the mechanism that is supposed to govern isothermal metamorphism: from their SSA analysis, the authors conclude that surface processes are limiting the metamorphism independently of the transport regime in snow (diffusion or advection). However, they finally conclude that diffusion is the main mechanism at the origin of snow metamorphism.
→ The authors should clarify which mechanisms (diffusion or surface processes) are occurring under the different advection conditions, or at least should present their results in a more consistent way.

3. Based on 4 samples submitted to saturated air, the authors try to generalize their experimental results to any isothermal snowpack. For this, they argue that Pe numbers higher than 1 are impossible in isothermal snow. Such a claim sounds exaggerated. For instance, Depth hoar can form close to the surface (Alley et al, 1990; Gallet et al, 2013;

Adams and Walters, 2014) and then undergo equi-temperature metamorphism due to a change of weather conditions, leading to potentially high Pe numbers, while their microstructures slowly evolve toward more rounded structures.
→ It seems more realistic to restrict to the evidence shown by the experiments and not to extrapolate to conditions that have not yet been properly investigated.

4. Some inconsistencies also appear throughout the paper concerning the settling of snow samples. From the literature (e.g. Schleef et al., 2014), it is obvious that the samples should exhibit at least moderate settling effects. This seems to be confirmed by most of the views of the samples (Fig. 3 and 4) where at least slight translations in the vertical direction are detectable. However, the authors give sometimes contradictory information on this topic.
→ The authors should clarify this point. A possible approach would be to acknowledge settling effects in all experiments but to consider that these effects have negligible impact on their study.

5. Many appropriate references are missing and several works are inadequately introduced. The paper lacks also some comparisons with previous results of the literature and their in-depth discussion.
→ See detailed comments (and point 6. below, e.g.).

6. Permeability computations show evolutions that are not really consistent with those of the density and SSA. This might be due to the inherent difficulty of computing very precise estimations of this property using direct numerical simulations.
→ Comparisons of the results to existing relationships of the literature and a discussion on this topic would strengthen the paper.

7. A meshing approach has been used to compute most of quantitative parameters (density, SSA, permeability) but very little information is given on the mesh quality. It has, however, a very strong impact on the numerical computations.
→ A graph showing the influence of the mesh quality on the computed properties, and the pertinence of the chosen mesh would be appropriate. At least, for one sample, a figure (3D view) of the mesh used is needed.

8. The local observations of snow structure are a bit deceiving. In particular, Fig. 4 is difficult to read and does not allow really checking the typical nature of Kelvin effect. Vertical displacements of the structure are detectable, but not commented by the authors.
→ The authors should consider replacing Fig. 4 by the superposition of cross-sections between 2 given time steps and discussing it in more details.

9. From a presentation point of view, the fact that only cases with Pe <= 1 have been investigated should appear in the important parts of the paper (title, abstract, introduction, etc). In addition, the discussion concerning the Pe numbers appears probably too late in the text.
→ An option would be to discuss and restrict the problem to Pe <= 1 much earlier in the paper.

10. Some basic but important information are missing (height of the samples, anodic current, computation method used for dp, etc.).
→ To be added to the text.

Specific comments:
All page and line numbers correspond to those of the Discussion Paper.

-1022/2-3: *Diffusion and advection across the snow pores were analysed in controlled laboratory experiments.*
It is difficult to understand to which part of the work this sentence refers. Does it refer to the theoretical interpretation of Pe numbers or just to the fact that several experiments with different regimes where experimented? Furthermore, the authors do not have access to the diffusion nor advection fields, but just to their effect on the snow microstructure. A slight reformulation of the sentence would clarify these points.

-1022/8-10: *Diffusion originating in the Kelvin effect between snow structures dominates and is the main transport process in isothermal snow packs.*
Can we still talk about transport by diffusion when the Peclet number Pe is so close to one? Do the results not tend to show that the isothermal metamorphism is rather driven by evaporation-deposition phenomena (i.e., probably what the authors also call ñsurface processesò), independently of the transport process actually used (diffusion for low Pe, advection for Pe closer to 1) ï see the n-exponent analysis. In addition, the generalisation of the experiments done on 4 specific samples in saturation conditions to all isothermal snowpacks seems clearly exaggerated. I suggest reformulating this last sentence.

-1022/24-25: *The energy reduction is achieved by mass transport processes such as vapour diffusion (Neumann et al., 2009), surface diffusion (Kingery, 1960b), volume diffusion (Kuroiwa, 1961), and grain boundary diffusion (Colbeck, 1997a, 1998, 2001; Kaempfer and Schneebeli, 2007). Viscous or plastic flow (Kingery, 1960a), evaporation-condensation with vapour transport (German, 1996; Hobbs and Mason, 1963; Legagneux and Domine, 2005; Maeno and Ebinuma, 1983), and the Kelvin effect (Bader, 1939; Colbeck, 1980) are also suggested to play an important role.*
These sentences sound strange for several reasons:

-the Kelvin effect is not really a mechanism for isothermal metamorphism but the ñdriving forceò of this metamorphism. It is consensually known to be the cause of isothermal metamorphism and it should not be confused with the way (transport phenomena or other mechanisms) by which the mass redistribution occurs (see e.g. Flin et al 2003, Vetter et al 2010).

-to my knowledge, there is a general agreement to consider vapour diffusion, evaporation-condensation and surface diffusion as the most probable dominant mechanisms depending on temperature conditions (see e.g. Hobbs, 1974; Maeno and Ebinuma, 1983; Brzoska et al, 2008; Vetter et al, 2010)

-1022/26-1023/2: *Recent studies indicate that vapour transport caused by the Kelvin effect is most important in isothermal metamorphism (Vetter et al., 2010).*
It seems that this is not exactly what is written in Vetter et al, 2010. Please check.

-1023/14-17: *However, no prior studies have described the effect of airflow on the vapour transport and the recrystallization of the snow crystals.*
As far as tomography is concerned, this subject seems clearly new, indeed. However, several studies have been devoted to air flow effects on vapour transport and recrystallization (actually, phase changes) using other approaches (e.g., Neumann et al, 2009; Albert, 2002; Albert and Schultz, 2002; Calonne, 2014). Please correct the sentence accordingly.

-1023/17-20: *However, saturation vapour density of the air is reached in the pore space within the first 1 mm of the snow sample, regardless of temperature or flow rate (Neumann et al., 2009; Ebner et al., 2014).*
From Neumann et al, 2009 (conclusions), it actually appears that this length is not 1 mm but 1 cm.

-1024/10:
Please give the numerical value used for D.

-1024/26-30: *We designed experiments in a controlled refrigerated laboratory and used time-lapse computed tomography (micro-CT) to obtain the discrete-scale geometry of snow (Schneebeli and Sokratov, 2004; Kaempfer and Schneebeli, 2007; Pinzer and Schneebeli, 2009; Pinzer et al., 2012; Ebner et al., 2014).*
Please consider adding some appropriate references to non-SLF studies (e.g., Chen and Baker, 2010). This would help to situate the present work in the international contest.

-1024/14-15: *Higher Pe numbers were experimentally not possible, as the shear stress by air flow would destroy the snow structure*.
Maybe I missed something important, but this assertion does not seem convincing: refrozen MF samples, DH samples, or even ŏldòRG samples would probably have allowed higher Pe numbers without any significant problem. See table below, were I computed some 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

-1024/23-24: *The acceleration voltage in the X-ray tube was 70 keV with a nominal resolution of 18* ***ε****m.*
Please change 70 keV into 70 kV and add information about current in **ε**A.

-1024/24-25: *The samples were scanned with 1000 projections per 180 degree, with an integration time of 200ms per projection*.
Does it mean 2000 projections were done per sample, or that half of a rotation was used to scan the specimen? In the latter case, please specify if there are specific reasons for this choice (360° rotations are much more common, as they better allow checking the consistency of the image reconstruction).

-1024/25-27: *The innermost 36.9mm of the total 53mm diameter were scanned and subsamples with a dimension of 7.2mm×7.2mm×7.2mm were extracted for further processing.*
Please add information about the total height of the snow sample (and that of the snow sample holder) in the text.

-1026/1-3: *It showed no significant change in the grain shape, even for different airflow velocities, and only a slight rounding and coarsening was seen for experiments* ***ñ****sa1***ò** *and* ***ñ****sa2***ò**
Please comment in the paper on the strong translation effect (settling or sublimation of the sublayering snow?) that is obviously visible for sa1 and sa2 (Fig. 3).

-1026/5-6: *The sublimated mass was relocated to bigger grains but the airflow velocity did not affect this relocation process.*
Was the mass preferentially relocated to bigger grains or to concavities? Which kind of vapour transport is actually occurring? Where are the vapour sources and the corresponding sinks? Could not directional effects that are due to the flow direction be observed on the microstructure? Cross sections (residence time graphs of Fig. 4 are poorly informative).

-1026/9-10: *after sintering, further densification is limited by coarsening kinetics.*
This sentence seems strange. To my understanding, sintering and densification are inherently coupled in metamorphism processes (see Flin et al, 2003; Vetter et al, 2010; Schleef et al, 2014).

-1026/11-12: *Thus, spatial change in the flow field due to different interfacial velocities can be neglected.*
This is true for the imaged volume, but what about the base, top and lateral boundaries of the overall sample, particularly prone to flow changes and heterogeneities?

-1026/13-14: *Consequently, Pe was constant with time, and diffusion was still the dominant mass transfer mechanism.*
The relationship with the preceding sentences is not obvious for me. Concerning diffusion as a dominant mechanism, it seems the authors need to check and clarify this point throughout the paper: is really diffusion the dominant vapor transport mechanism? Is advection really negligible? Are these two phenomena not strongly coupled for Pe approaching 1 (sa3 and sa4)?

-1026/18: *no settling and densification occurred*
Please add at least ñin the investigated volumeò
This assertion seems quite questionable as far as the whole sample is concerned. Recent snow undergoing isothermal metamorphism, such as sa1 and sa2, are known to undergo settling and densification due to their own weight. See here for instance: Calonne et al 2013, Schleef et al 2014. At least, strong translation effects can be seen on Fig. 3 (sa1 and sa2) and are also detectable on Fig. 4 (s3 and s4).

-1027/eq 1:
This formula results from a very basic mean field approach. In particular, it considers disconnected grains that do not undergo settling. Consequently, equation (1) may give very qualitative estimation on the real mechanisms occurring in snow (for a discussion on some of these aspects, see e.g. Legagneux et al 2004, who mention different non-integer exponents for several experiments and the introduction of Taillandier et al, 2007).
It is also known to be extremely dependent to the initial state, which is well illustrated by the high difference obtained for n values of sa3 and sa4 between tables 2 and 3.
At least, a small comment on these topics seems relevant as far as the determination of mechanisms is concerned.

-1027/16-17: *Theoretically, the growth exponent n is approximately 2 when surface processes are rate limiting*
What does ñsurface processesò stand for? Is it sublimation-deposition, surface diffusion, or both of them?

-1027/20-21: *Experiment* ***ñ****sa3****ò*** *and* ***ñ****sa4****ò*** *had similar fitting parameters and a lower value of n, suggesting that surface effects were rate limiting.*
Why a lower value of n, namely 0, suggests surface effects are rate limiting?

-1028/1-4: *The effect of decreasing SSA on the permeability was not elucidated in our experiments. [****é*** *] The value of the effective permeability was higher than the one determined in a previous study (Zermatten et al., 2011, 2014), although, our measured SSA was higher by a factor of at least 2.4. The temporal evolution of permeability for experiment* ***ñ****sa2****ò*** *showed a decrease of 8% for the first 40 h and remained constant afterwards.*
An SSA decrease at a constant density would result in an increase of permeability (see e.g. Calonne et al 2014). This does not seem to be in accordance with the results of Fig. 9.
As the authors have access to both SSA and density, they could plot permeability estimations using existing relationships from the literature (e.g. Shimizu (1970), Carman-Kozeny formula, etc - see e.g. Courville et al (2010), Calonne et al (2012) or Domine et al (2013)) and discuss how these estimations compare with their numerical results.

-1028/13-14: *This difference could therefore be due to an error during the measurement.*
Please clarify this point. What kind or measurement error? Is this inconsistency not rather due to problems in permeability computations (meshing, impact of the borders of the image file, choice of boundary conditions, REV)?

-1028/15-16: *As Pe < 1, diffusion was consequently the dominant component.*
The interpretation of Pe numbers in terms of transport mechanisms seems biased. I agree that a Pe value of 0.85 is smaller than 1, but 0.85 can be seen also as nearly equal to 1 depending on the separation of scales of the problem (typically, here: pores of 0.3 mm in a sample of size 50 mm). This means that for sa3 and sa4 experiments, diffusion and advection, which are concurrent mechanisms, both play a non-negligible role in vapour transport. Actually, given the scale separation of the problem (about 1/100), it seems Pe should be of the order of 10^{-4} to neglect advection effects in the transport phenomena (see e.g. Auriault et al, 2009; Calonne, 2014).

-1028/21-23: *(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, 1997b)*
This is not really true: depth hoar often forms close to the surface and could then be exposed to air advection (Alley et al 1990). See also Gallet et al (2013) and Adams and Walter (2014) concerning radiation recrystallized snow.
Also, refrozen wet snow or ñoldò rounded grains may be suitable to Pe > 1. See comment 1024/14-15.

-1029/7-8: *Pe > 0.85 were not possible due to the destruction of the snow structure.*
See comment 1024/14-15. In any case, such information as well as the discussion concerning the Pe number could appear explicitly earlier in the paper (title, abstract and introduction). For the title, replacing « advective conditions » by « moderate advective conditions » could be an option.

-1029/12-13: *after sintering, further densification was limited by coarsening kinetics.*
See comment 1026/9-10

-1029/16-18: *no enhancement of mass transfer inside the pores was observed and diffusion through the pores was the main driving force.*
Is really diffusion the ñdriving forceò of the metamorphism? What about Kelvin effect? What about the role of « surface processes » mentioned in the paper (see 1027/25-27)?

-1029/18-19: *Curvature caused sublimation of small ice grains leading to a slight decrease in SSA*
What about concave shapes?

-1029/19-20: *In isothermal snow packs, diffusion through the pores is the dominating part and advective transport processes on the structural dynamics can be neglected.*
Is this sentence really deduced from the experimental work done? Based on the results obtained for 4 samples where Pe was always below 0.85 and the air was always saturated, this assertion seems a bit exaggerated. Using under or over-saturated air (or larger Pe, which is not impossible depending on snow types) may lead to different results.

-1034: Table 1.
How was dp estimated? Using an estimation based on SSA would give a pore diameter 2 times higher than the presently given values (see comment 1024/14-15): this would then result in an increase of the computed Pe numbers by a factor 2. At least, some information should appear in the text of the paper about the methodology used.

-1038/Fig 2:
This graph does not seem mandatory to me. The authors can just write instead that the temperatures were -7.5+-0.5°C and -14.5+-0.5°C at the top and base of the sample throughout the experiments. If they want to keep this graph, it could be worth plotting both of the NTC measurements to better show the accurateness of the isothermal conditions.

-1039/Fig 3:
It could be worth to recall on the figure:
- -the direction of the air flow
- -the Pe numbers for each experiment

Adding a figure with a full-size view of the surface mesh of a sample would help the reader to be convinced of the meshing accuracy and of those of subsequent computations (density, SSA, permeability, etc.). A graph showing the pertinence of the chosen mesh (e.g. permeability = f (cell number)) should also be considered by the authors.

-1040/Fig 4:
Residence time of ice particles within in a slice → ñwithin a sliceò
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 rotation with time. Has each image that constitutes the figures been spatially repositioned thanks to adequate references?
Residence time views are interesting, but do not show really how the snow evolves over time (i.e., what parts are growing, what parts are shrinking, and in which directions they are

moving). This is, however, of primary importance, as it allows understanding the nature of the driving forces (Kelvin effect) and mechanisms in process.
Please consider replacing these graphs with the superposition of vertical cross-sections at time 0 and 96 hours (or another time). See e.g. Calonne et al 2013.
From the present graphs, a vertical displacement can be noticed. Is it due (1) to minor settling effects in the snow sample, (2) to the effect of the vertical air flux, or (3) to a combination of these phenomena?
Adding the direction of the air flux would also be useful.

-1042/Fig 6 + 1043/Fig 7 + 1044/Fig 8:
How were the errors on density and SSA actually estimated?
The authors refer to Zermatten et al 2014, but it seems the method used in the work of Zermatten (two-point correlation function) was significantly different from that used in the present paper (triangulation). Note also that the error given by Zermatten et al was estimated based on the comparison with stereological estimations from horizontal cross-sections.
Another point to consider is that triangulation methods are potentially prone to systematic overestimations for SSA. At least, this is the case for simple Marching Cubes estimations (see e.g. Flin et al, 2011; Hagenmuller, 2014).

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