Author comment to Anonymous Referee #1

We thank Reviewer #1 for his/her evaluation of our work. We have substantially modified our manuscript and improved the parts that Reviewer #1 identified to be weakest. We hope that our revised manuscript will be acceptable for final publication in *The Cryosphere*.

GENERAL COMMENTS

The paper by Calonne et al. is continuing the trend in determining physical properties of snow and firn by direct numerical simulation. They use a fair number of snow samples (35) digitized by micro-tomography, a commercial software for the simulations, and calculate a relation between density, specific surface area and (scaled) permeability (which makes much nicer plots). What is new in this paper is the "measurement" of small scale anisotropy, which is not easily accessible by direct methods.

We thank Reviewer #1 for acknowledging the interest of our approach.

The paper is an interesting example of the application of direct numerical simulation, but gives relatively little new scientific insight beyond well known facts about permeability in porous media (N.B. there a dozens of paper in the geophysical literature on direct numerical simulation of permeability, most notably by Adler, Arns and Knackstedt, which are not even mentioned).

We thank Reviewer #1 for providing references to permeability of porous media, although we note that there are relatively few studies dealing with direct numerical simulation of snow permeability. Note that Reviewer #2 identified our work as a useful contribution to this effort. We have added the following references, page 2, line 44: “More recently, the availability of 3-D images of snow and firn from X-ray tomography (Brzoska et al., 1999; Coléou et al., 2001; Freitag et al., 2004; Schneebeli and Sokratov, 2004; Kaempfer et al., 2005; Chen and Baker, 2010) opened the way to numerical simulations, applying to snow the methods developed for porous media in general (Spanne et al., 1994; Ferreol and Rothman, 1995; Martys and Chen, 1996; Arns et al., 2001, 2004).”

It remains also rather unclear what is the wider objective, beyond the numerical exercise.

The main final objectives of the paper are (1) to be able to estimate permeability values from field measurements (specific surface area and density) and (2) to evaluate the anisotropy of permeability depending on snow type. We have better clarified the objective of our paper in the abstract and introduction as follows:

Page 1, line 1: « We used three-dimensional (3-D) images of snow microstructure to carry out numerical estimations of the full tensor of the intrinsic permeability of snow (**K**). This study was performed on 35 snow samples, spanning a wide range of seasonal snow types. For several snow samples, a significant anisotropy of permeability was detected and is consistent with that observed for the effective thermal conductivity obtained from the same samples. The anisotropy coefficient, defined as the ratio of the vertical over the horizontal components of **K**, ranges from 0.74 for a sample of decomposing precipitation particles collected in the field to 1.66 for a depth hoar specimen. Because the permeability is related to a characteristic length, we introduced a dimensionless tensor $\mathbf{K}^* = \mathbf{K} / r_{es}^{2}$, where the equivalent sphere radius of ice grains (r_{es}) is computed from the specific surface area of snow (SSA) and the ice density (ρ_i) as follows: $r_{es} = 3 / (SSA \times \rho_i)$. We define K and K^* as the average of the diagonal components of **K** and $\mathbf{K}^*$, respectively. The 35 values of K^* were fitted to snow density (ρ_s) and provide the following regression: $K = (3.0 +/- 0.3) r_{es}^{2} \exp((-0.0130 +/- 0.0003) \rho_s)$. We noted that the anisotropy of permeability does not affect significantly the proposed equation. This regression curve was applied to several independent datasets from the literature and compared to other existing regression curves or analytical models. The results show that it is probably the best currently available simple relationship linking the average value of permeability, *K*, to snow density and specific surface area. »

Page 2, line 57: « We carried out numerical estimations of the full 3-D tensor of intrinsic permeability (K) on 35 3-D images of seasonal snow obtained from microtomography. Computations were performed with the software Geodict (Thoemen et al., 2008; Koivu et al., 2009; Calonne et al., 2011) and were based on the periodic homogenization method (Auriault et al., 2009). The main objective of this paper was to elaborate a parameterization of the snow permeability from other variables measurable in the field. For this purpose, we studied the relationship between the computed permeability, snow density and grain size, defined here as the equivalent sphere radius, at the scale of the representative elementary volume (REV). This relationship obtained using our data was compared to existing literature dataset as well as other equations from theoretical models and regressions. In addition, we focused on the anisotropy of permeability, available from the computed 3-D tensor of permeability. »

It remains especially unclear how the important issue of anisotropy can be determined except by direct numerical simulation. The descriptive methodological part in this paper is dominating. In addition, there are no direct physical measurements done and directly compared to their numerical solutions, a point which can also be criticized in earlier papers, but which should not be repeated ad infinitum.

We agree with the Reviewer #1 and have two lines to address this issue:

- first of all, we do not currently possess an instrument capable of measuring snow permeability. Only a few of these instruments exist worldwide (most notably at CRREL in the USA, Japan).
- second, even if we had a field or laboratory permeameter, it would be extremely challenging to compare measurements from such an instrument operating on samples on the order of 10^{-3} m^3, to be compared to micro-tomographic images used for this study, whose volume in on the order of 10^{-7} m^3. In particular, the anisotropy potentially observed with a field or laboratory permeameter on large samples (dm-scale) would not be comparable to the microscopic anisotropy, which is relevant at the cm-scale.

It would have been interesting to see, finally (after Freitag, Courville and Zermatten) a direct comparison of these numerical techniques.

We take good note of the reviewer's suggestion and are open to such a comparison. Such an assessment would require an international consortium to be established, to openly compare results from different numerical techniques on the same snow samples (as it has been done for fibrous materials in Koivu et al., 2009). Although potentially interesting and thus highly desirable, this goal appears beyond the scope of our paper, which aims at studying the links between different physical properties of snow (namely, permeability, density and specific surface area) more than comparing different methods.

They are all within the range of parameterisations proposed - but the gain in knowledge is rather small. The (log) scaled permeability shows a good correlation with density, and this is indeed a nice results, as SSA_v and density are sufficient to estimate permeability within about a factor of 3. I consider this paper as an interesting methodological development, but not a paper to be considered to appear in a very high-ranking cryosphere journal.

Our mains results can be summarised as follows:

(1) Our proposed regression allows estimating an average value of snow permeability from specific surface area and density, both measurable in the field.
(2) The anisotropy coefficient of permeability (vertical / horizontal value) ranges from 0.74 to 1.66 depending on the snow microstructure.

We believe that these results may interest the snow community, as expressed by the Reviewer #2, and we think this paper is particularly suited to The Cryosphere journal.

SPECIFIC COMMENTS: The introduction starts after a very non-specific sentence with theory. Introduce the reader first to the subject (I think you can assume for given that readers of The Cryosphere now what are the constituents of snow), explain your hypotheses and objectives.

The beginning of the introduction has been reformulated and goes now straight to the point. Page 1, line 20: "The intrinsic permeability (**K**) is an important physical property of snow. Defined in a tensorial way, **K** (m^2) links the pressure gradient …".

Present the theory in the "Methods". The discussion of the literature is not complete, especially there is no inclusion of the direct numerical simulation of permeability in the larger field of porous media (where the French researcher P.M. Adler plays a leading role). The discussion of permeability in snow and firn is incomplete. Bader (1939) did much more than a few measurements of permeability, he actually showed some deep insight in the process. Please read his original contribution. He realized dozens of precise measurements which would well compare to the data presented here. He was also the first to propose permeability as a classificatory variable, almost identically as later done by Arakawa et al. (2009) (which was not aware of the work by Bader). The pioneering work by M. Albert et al. is also discussed superficially, as they made many detailed and precise measurements with a technically improved method.

We have improved the literature review of snow permeability, but chose to keep it concise in order to not distract the reader from the main results exposed here. We have added the reference of Bader (1939), page 2, line 29: "Permeability has also been proposed as a means of characterization for quantitative snow classification (Bader, 1939; Jordan et al., 1999; Arakawa et al., 2009)." Nevertheless, it is not the purpose of our study to carry out an extensive review on snow permeability measurements.

Is direct numerical simulation of permeability really able to deal with spatial variability typical for this property? How about thin layers of a few mm thickness (or even thinner)?

Computations are performed on representative elementary volume (REV), meaning that the results are representative of the entire sample. We assume that the sample is homogeneous at this scale. The edge size of our volumes of computation range between 2.5 and 9.5 mm.

The samples investigated are mostly from cold-room experiments: do they really cover typical natural conditions, and to which degree? In addition, only one climatological type of snow (maritime alpine) was sampled - missing all tundra, taiga, and polar snow types.

Cold room experiments cover 65 % of the total samples investigated here. We acknowledge that not all snowpack types are represented here, but claim that many snow types (as described in the International Classification of Seasonal Snow on the Ground, Fierz et al., 2009) are indeed represented in our dataset. We thus feel that the obtained regression and conclusions are applicable to most snow conditions worldwide.

The explanation of the numerical technique is very short, and possibly not easy to follow for readers of TCD. How is the periodic BC handled in the code? Are the samples mirrored at the BC?

We have expanded the description of the method used. Please find below the additional information put in the manuscript. Note, however, that our manuscripts quote the reference technical publications (including one book) which the reader can read to access detailed information about the method employed. Page 4, line 124: "The components of the permeability tensor **K** were estimated by solving the above boundary value problem (3) on a REV extracted from tomographic images (see Sect. 3.6), using the software Geodict (http://www.geodict.de). The boundary value problem is solved by using the finite difference method. Within this method a staggered grid (voxel) is used. The values of velocity and pressure are defined at center points of the faces and volume of the cubic unit cells, respectively. The partial differential equations for incompressible Stokes flow (3) are solved by using the FFF-Stokes solver based on Fast Fourier Transform. Periodic boundary conditions are applied on the external boundaries of each volume (see Wiegman (2007) for more details)."

Results and discussions are mixed in one section: it would result in a better structured and readable manuscript if these were separated. It would then also become more clear what the hypothesis of this paper is.

As we have a thematic approach, we think that discussing the results just after their presentation in a same section is more suitable. As Review #2 does not comment about this issue and finds our manuscript clear, we thus kept the results and discussions in a same section. Nevertheless, we have restructured sections of the paper in order to improve its readability.

Concerning the correlation of K*: while it is always possible to calculate Rˆ2, the interpretation of Rˆ2 for log-transformed data as a measure of quality is not as easy, as the original values are clearly not normally distributed, but belong to a log-normal distribution. A confidence interval would require a bootstrap analysis, and this should be done here.

We acknowledge that the presentation of the statistical significance of our regression curve was not satisfying and we have added a special section 3.3 named "Regression analysis" (page 8, line 230) to address this issue in details.

I also missed an error analysis: As K* is scaled by SSA_v, both errors on SSA and density contribute to uncertainty. It would be interesting to read more about this issue.

We added details on this issue in the paper. Page 9, line 288: "Based on the mathematical equation of our proposed regression curve (Eq. (6)), accounting for a 10 % uncertainty on the parameter *a* of the equation (here we neglect the uncertainty on the parameter *b* of the equation - see Table 1), and assuming that r_{es} and ρ_s both carry a measurement uncertainty on the order of 10 % (Matzl and Schneebeli, 2006; Painter et al., 2006; Gallet et al., 2009; Conger and McClung, 2009; Arnaud et al., 2011), the propagation of these relative errors in terms of *K* adds up to about 50 %. This experimental error is of the same order of magnitude as the minimum and maximum deviation found when applying the regression curve to independent data (black points in Fig. 5)."

The conclusion that simple cubic packing of (equivalent?) spheres is well suited to come close to the permeability of snow is surprising. How do you built in anisotropy? How did you deal with a volume density below 0.5?

The permeability tensor of simple cubic packing of spheres is isotropic due to the different planes of symmetry of the microstructure. The microstructure consists in a square array of non-overlapping (respectively overlapping) spheres with the same diameter for a volume density smaller (respectively larger) than 0.5. We have added more details about this issue in section 3.5, page 10, line 320: "Numerical values of permeability were computed using the periodic homogenization method and a finite element software, for a simple cubic packing of spheres (Boutin and Geindreau, 2010). Note that at high density, where spheres interpenetrate, the SSA is computed from the effective surface formed by the sphere's assembly.", and also page 11, line 351: "The predicted values using a simple packing of spheres are also consistent with experimental and numerical estimates from snow samples. In particular, this latter model behaves better than the CK and SC approach for the MF samples, where these two models fail to reflect the snow microstructure and overestimate the permeability, as shown in Fig. 1. These results can be explained by the fact that at low density the airflow around a snow particle is little affected by flow around its neighbors. In contrast, at higher density, snow particles are close together and the flow around one of them disturbs the flow around the others. This last phenomenon is not captured by analytical models."

Concerning anisotropy, Luciano and Albert measured much larger samples. Although they point out that their sample were "visually homogeneous", this is unlikely to be the case for Summit firn, as shown by Dadic, R. et al. (2008),JGR, 113(D14), doi:10.1029/2007JD009562. So what they measured, is actually a "macroscale anisotropy" and not the microscale anisotropy discussed here.

For a homogeneous sample, the anisotropy coefficient measured at the macroscale and computed from a representative microscopic sub-volume of this sample will show a similar value. Again, if computations are performed on REV, the result will be similar to the macroscopic result. So, if we assume that the samples of Luciano and Albert are homogeneous, which is explicitly stated in their paper and confirmed by figures of vertical cross sections of samples, we believe both of these anisotropies are comparable.

Finally, I found two conclusions confusing. First, the major conclusion "... the the permeability of seasonal snow, if assumed isotropic, can be reasonably inferred ...": but you should in the entire paper that the permeability IS anisotropic. So what do you conclude really? That is can be approximately estimated (we know that), but you have no handle on anisotropy. Second, the authors suggest that permeability may by useful for classification (as suggested by Bader and Arakawa). But on the next sentence it is written "... the high coefficient of correlation between K, SSA and rho ...", so then SSA and rho are AS GOOD as K, and there is no advantage of K?

We agree that our conclusion needed clarification. We have reformulated it and it now reads as:
Page 12, line 375: "The intrinsic permeability tensor K was computed on 35 tomographic images of various snow types by solving numerically a specific boundary value problem arising from the homogenization process on a representative elementary volume (REV). The equivalent sphere radius (r_{es}) was used as a characteristic length of the microstructure to reduce **K** to a dimensionless tensor **K***. A regression equation using the 35 computed values of mean permeability (K), density (ρ_s)and equivalent sphere radius was proposed, such as: K =(3 +/- 0.3)r_{es}^2 exp((-0.0130 +/- 0:0003) ρ_s) and compared to existing literature data. Our main conclusions are summarized below:

1. The intrinsic permeability of snow can be anisotropic depending on the snow microstructure. The anisotropy coefficients of permeability range from 0.74 for a sample of decomposing precipitation particles collected in the field to 1.66 for a particularly evolved depth hoar specimen, and are consistent with the anisotropy coefficient of the effective thermal conductivity. It appears that the use of these coefficients could be helpful for the quantitative classification of snow, as it may enable to distinguish the depth hoar and the facetted crystals from other snow types.

2. Permeability, density and equivalent sphere radius, directly related to the specific surface area, are strongly correlated. The equivalent sphere radius is thus a relevant characteristic length for permeability, which in addition is rather easy to determine in the field or from 3-D images. However, this strong correlation between snow permeability, density and SSA precludes considering them as independent variables for the sake of objective snow classification.

3. The anisotropy of permeability does not affect the regression curve performed on the 35 snow samples. Indeed, very similar regression curves have been computed by using each diagonal component of **K** as well as the average of these three terms. Thus, the proposed regression allows estimating an average permeability value only.

4. Our numerical computations of permeability from 3-D images are consistent with dataset from other experimental and numerical studies. Moreover, our regression succeeds in estimating the permeability data of previous studies with a small relative bias on the order of 20 % maximum, within a factor ~ 2 maximum relative deviations (40 %), except for the dataset of Arakawa et al. (2009). By comparing with other equations from theoretical models, fits and numerical data available in literature, our regression appears to be the best currently available simple relationship linking the average value of permeability to snow density and specific surface area. In particular, the well known fit proposed by Shimizu (1970) seems to significantly underestimate the permeability of snow for most of the tested datasets, most probably due to an inconsistency between the equivalent sphere radius derived from the specific surface area of snow and the empirically-defined snow grain size used by this author."

TECHNICAL CORRECTIONS: All figures have close to illegibly small numbering of axes and parts of the legends.

Upon acceptance of our article for final publication, we will adapt the font sizes to ensure readability in the final layout of *The Cryosphere* (different from *The Cryosphere Discussions*).

Fig. 4: what are the "3 mm" indicating? Size of the bar? What magnitude and unit has the velocity?

The "3 mm" label indicated the size of the length bar. Magnitude (from 0 to 3.84×10^{-4}) and unit ($m\ s^{-1}$) of the velocity have been added in the figure. We have rewritten the caption (page 21) as follows: "The color levels of the images correspond to the fluid velocity (intensity) computed for a pressure drop of 2×10^{-2} Pa along the z (left) and y (right) directions and a dynamic viscosity of air of 1.8×10^{-5} Pa s^{-1}. The ice matrix of the microstructure is represented in gray. The length of the color bar corresponds to 3 mm."