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Interactive comment on "Impact of physical properties and accumulation rate on pore close-off in layered firn" *by* S. A. Gregory et al.

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The responses to all comments are directly below the original comment. The start of all author responses are also indicated with an asterisk (*).

Anonymous Referee #1

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This manuscript describes measurements of the physical properties of ïňĄrn samples from two Antarctic sites with very different local temperatures and accumulation rates. Micro computed tomography was combined with measurements of bulk density and permeability. The analysis is concentrated on the lock-in zone of ïňĄrn with the aim to better understand the role of firn microstructure on the pore close-off process. Results

indicate that the grain size and the accumulation rate are more important factors for the lock in and pore close-off depths than the ïňĄrn density. I have read this article with interest and consider it a valuable addition to the current understanding of the close off process of deep ïňĄrn, which is of great importance in determining the gas age / ice age difference.

The article has a clear structure and is well written. The quality and readability of the figures, however, can be improved in my opinion. The axis labels and legends of Figures 1 and 5 are especially hard to read and the y-scales in Figure 6 seem to be too compressed.

Specific comments

P 2534, line 26 "bubbles of an ice core" should be "bubbles in the ice"

*Correction was made.

P 2535, line 16 "layers of ïňĄrn impede" should be "layer of ïňĄrn impedes"

*Correction was made.

P 2537, line 24 I think the words "to verify" or something similar are missing here: "and to verify the validity of"

*Changed sentence to "and to investigate the validity of"

P 2538, lines 14-15 The error mentioned here only refers to the precision (standard deviation), but not to the accuracy.

*The accuracy for firn density measurements depends both on the quality of the sample being measured, and on the accuracy of the measurements. For the first, we excluded broken and chipped samples whose volumetric shape was not an ideal cylinder. . For the second, density is calculated from measurements of the mass divided by the volume of the sample. The mass of the sample from scale measurement is accurate to 1%, and the volume of the sample calculated from caliper measurements is accurate

to within approximately 2%. These are clarified by adding “To increase the accuracy of bulk property measurements, broken and chipped samples, not of an ideal cylindrical shape, were not included . . .” to P2538, lines 16-17, and addition of the statement “The mass of the sample from scale measurement is accurate to 1%, and the volume of the sample calculated from caliper measurements is accurate to within approximately 2%.

P 2540, lines 19-24 I am not familiar with the SMI and could not access the publication by Hildebrand et al. (1997), but I am a bit puzzled by this definition. According to this equation the SMI has units of meters, which means it is not an index but a length scale. Elsewhere, I found the following definition of the SMI: SMI = 6dSdr VS2 (1) which is a dimensionless quantity. I suspect that the quantities in equation (2) in the manuscript are measured in voxels rather than in m2 or m3 and that S’ is not the artificially increased surface area but the difference between the increased surface area and the original area. With dr equal to 1 this matches with the definition above. Could the authors clarify this?

*S’ should be defined here as the change in surface area between the initial surface area and the artificially increased surface area and thus it is an index.

P 2543, line 26 “the same” should be “similar”

*Correction was made.

P 2543, lines 26-28 Fig. 4a shows a trend in SMI from values around 2 at 56 m depth to values around 2.7 at 75 m depth. With SMI values of 3 corresponding to cylindrical shape and 4 to spherical shape (P 2540, line18) the statement: “The pore structure is primarily cylindrical at 55 m with increasing SMI, evolving to spheres towards the ïňĄrn-ice transition.” seems to be incorrect. A similar statement is found at P 2544, line 7.

*The intention of including the SMI was to highlight the trend toward spherical shapes with depth from cylindrical-like shapes. The aim is to show that at depths just above

the lock-in zone (56m) the pore structure is not perfectly cylindrical but headed that direction as samples are analyzed down through the LIZ. Toward the end of the LIZ (70-75m) the pore structure is very close to cylindrical with some high end values reaching 3.5. The scatter in this plot is due to the layered nature of the firn that is reflected in the microstructure. As the pores continue to become more isolated and surrounded by ice below the LIZ toward the firn-ice transition their shape should become spherical. The data from 56-75m shows a trend in increasing SMI with depth that would likely continue to increase to a value of 4 (perfect spheres) around the firn ice transition. The text has been re-written to clarify the description of Fig. 4a. to reflect the above description, to emphasize that it is the trends in SMI with depth that are occurring, and to propose the hypothesis that below 75m, around the firn-ice transition, SMI should be a value of 4, where the pore structure has been seen by others to evolve into spherical bubbles completely surrounded by ice.

P 2545, line 1 I don’t see how from Fig. 5 it is “visually evident” that the less tortuous matrix causes the firn to be more permeable.

*Good point. We added language that more clearly explains that the less complex and less tortuous pore space enables the firn to be more permeable for Megadunes despite having lower values of open porosity than WAIS Divide.

P 2546, line 12 “At depth” is a very unspecific term. Maybe rephrase this to something like: “Just above and in the LIZ”

*Agree. We changed “At depth” to “below 45m” where Megadunes firn first becomes consistently more dense than WAIS Divide firn.

P 2548, lines 14-17 and lines 22-24 The authors repeat themselves here with two very similar statements.

*We agree; the two statements are very similar – we changed this so that the second was omitted and replaced by: “High accumulation sites should on the other hand

exhibit a higher degree of microstructure variability resulting in a greater depth difference between fine grain close-off (LID) and coarse grain close-off (COD)." Following a sentence describing what is expected at typical low accumulation sites.

P 2550, lines 4-6 The comparison between measured porosities and permeability's with the predicted power law relation is not very clear from figure 10 (especially for the Megadunes data). It might be more illustrative to plot k as a function of n 3.4op and verify a linear relationship between these. This could be instead of the current ïňĄgure 10b. Alternatively, one could make a ïňĄt to the measured data assuming a relation of the type k = anb op and compare the values of the parameters to those of Freitag et al. (2002).

*The purpose of this figure is to emphasize the importance of pore structure at a given open porosity. Due to the open pore structure, the permeability at a given site will have a range for each open porosity. The magnitude of the permeability values at a given open porosity is dependent on the variability between coarse grain and fine grain firn layers at a given site. The Megadunes site in general, is more permeable at a given open porosity than WAIS Divide due to its large less complex pore structure. Fig. 10b is meant to illustrate that the importance of pore structure at low open porosities that are generally considered to have very low permeabilities in polar firn. We re-wrote the text to clarify the language and reasoning behind Fig 10.

I would also strongly encourage the authors to plot error bars in these and other scatter plots.

*The error in precision is smaller than 10% for permeability and much smaller for density, so in some cases this makes the error bars smaller than the points on the plot. The error for properties taken from microCT measurements are much harder to define based on the limited amount of samples run more than once. The majority of other papers dealing with similar measurements also do not include error bars in their figures.

P 2550, line 9 "200-700 %" should be "200-1900 %". Or is there a reason for excluding

this point?

*Good observations; there is not a reason for excluding this point and we have corrected the text.

P 2551, lines 18-19. Could you quantify "high" and "low" accumulation sites?

*Valid point. In the ice coring community, sites are commonly referred to as "high" or "low" accumulation sites depending on whether they get sufficient snow accumulation to preserve high-resolution climate records; for example the WAIS Divide site in West Antarctica is referred to as a "high accumulation" site (accumulation rate is $\sim$ 22 cm/yr (water equivalent)), in comparison to sites in East Antarctica such as Vostok , or megadunes (for example) that are referred to as "low accumulation" sites where the accumulation rates are on the order of 3 cm/yr w.e. or less. Currently in the research community there is not a strict dividing line between high and low. Therefore in this paper, we added language in the text to clarify the use of high and low.

Interactive comment on The Cryosphere Discuss., 7, 2533, 2013.