

Interactive comment on “Gas enclosure in polar firn follows universal law” by Christoph Florian Schaller et al.

Anonymous Referee #1

Received and published: 18 August 2017

General:

The authors present closed porosity data of firn and ice samples from three different polar sites in Greenland and Antarctica using 3D X-ray tomography. They find a 'universal' critical closed porosity where bubbles are sealed. While the technical approach seems robust and data are very interesting the interpretation and conclusions are too simplistic. The authors give the impression of being much closer to the physical reality than previous investigations and even accusing researchers in this field of producing corrupt data and misinterpreting them; however important details are not fully elaborated in this paper. The most important is how the scale-dependent porosity affects parameters like D-age and total air content (details in the specific comments below). The paper needs crucial revisions before it should be considered for publication.

[Printer-friendly version](#)

[Discussion paper](#)



Specific comments:

p. 1, l. 18: "direct record" seems not very informative. Probably direct records do not exist in the ice, but there is a large range of "indirectness". It should either be defined or reworded.

p. 2, l. 4: "direct measurement": the same as above.

p. 2, l. 12: "statistically solid dataset". This is a much undefined statement. Each record contains certainly a large number of data, but what does statistically solid mean? On the other hand universality of the critical porosity is deduced from only 3 records, which seem very marginal for such statement.

p. 2, l. 17: "The reduced coupling of proxies and surrounding conditions...will foster the development of new proxies, such as the air content as a marker of local insolation". This statement is somewhat unclear. I agree that it may help to put the interpretation of existing proxies on a more realistic basis, but to foster the development of new proxies is a very vague statement that calls for specific arguments.

p. 2, l. 27-30: This section requires some elaboration: "each data point": of what? "the remaining cut bubbles were less than 0.1%". How was this value determined? As the "sample" volume (1cm x 6 cm diam.) has a similar surface/volume ratio as a typical sample for porosity or total gas measurement this low value seems very surprising. Values in the order of 5-10% in the firn-ice transition zone would seem more realistic.

p. 3, l. 6: I suggest to replace "percentage" by "fraction" as the value is not given in percent

p. 4, l. 3: "Our estimation (Fig. 2b) proves a serious underestimation of the cut bubble effect and, in particular, confirms the existence of a critical porosity in contrast to recent assumptions of single-layer close-off occurring within a certain porosity range (Mitchell et al., 2015)". [occurring -> occurring]. I think there is a misunderstanding here. Mitchell et al. actually confirmed local density (or porosity) as a good predictor

[Printer-friendly version](#)

[Discussion paper](#)



for bubble closure. They only introduce stochastic variability of local density (porosity), which is well documented by measurements, to better describe the layering. But indeed there is a difference in the shape of the closed porosity (or total gas) vs. density function. Although various researchers have carefully corrected for cut bubbles an underestimation of this effect cannot be excluded. A smooth transition toward 100% closed pores as observed and still present after cut bubble correction contradicts your tomographic results an also simple percolation theory. This calls for further studies.

p. 4, l. 5-10: This paragraph needs clarification.

First, it is unfair to speak of corrupted datasets. All measured data have errors. Not all systematic errors may have been fully addressed, but therefore they are not corrupt.

Then it is most confusing to mention 37% critical closed porosity without presenting its context. This value simply relates the total gas data to the equivalent density (or porosity, or closed porosity) assuming virtual instant close-off. This has not much to do with the local pore close-off discussed here. Instead of suggesting "avoidance of such concepts" the authors should rather carefully discuss that beside the local pore close-off (at 100% local closed porosity) other factors affect total gas content in the ice (comparison with Martinerie data; Fig 3) and the concept of non- (or low-) diffusivity below a certain depth with a bulk porosity significantly above 0.1, which is crucial for the ice age – gas age difference.

p. 4, l. 13: "cannot resemble" -> "cannot fully reflect"

p. 5, l. 16-23: As mentioned above the local pore-close off is not the parameter that determines delta-age and delta-depth. It is rather the depth where diffusivity approaches zero. Better knowledge of the local close-off mechanisms is certainly very interesting but does not help to resolve the discrepancies in a simple way as suggested here.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2017-94>, 2017.

Printer-friendly version

Discussion paper

