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

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Schaller et al. present a new, extensive and highly valuable dataset on the bubble close-off process in polar firn, obtained using x-ray computed tomography. I would like to congratulate the authors on this achievement, which must have taken considerable time and analytical effort. The authors use this data set to provide strong observational evidence that bubble closure happens at a single porosity value, independent of the climatic conditions at the site.

Detailed observations of the close-off process are the only way to make progress on this complex problem, and I am very enthusiastic about this effort. The main experimental observation, namely that sealing of layers occurs at a constant density/porosity

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value independent of the site climatic conditions, is both important and well founded in percolation theory. I am thus highly supportive of publication of this work in Climate of the Past. I give several suggestions below, which are meant to improve an already good manuscript, rather than criticize it.

My main concern is that the authors could do a better job at placing their result into a wider context, and be more respectful of previous work on this topic by avoiding phrases like “corrupted data” and “misleading”. The pioneering work by Jakob Schwander, Jean-Marc Barnola and Patricia Martinerie is still relevant 25 years later, which is testimony to its quality. Rather than being “corrupted”, these data simply represent measured quantities that are complementary to the micro-CT data (rather than inferior to them). For example, the casual reader of the manuscript will come away with the impression that the often-used temperature relationship by Martinerie et al. (1992, 1994) is incorrect and should be abandoned. However, Martinerie et al. studied air content, rather than firn microstructure, and I trust those data to be correct (and not “corrupted”). To me, the more interesting question is: How is it possible that air content strongly depends on the climatic conditions at the site (as demonstrated by Martinerie et al.), while the critical close-off porosity is independent of site conditions (as demonstrated by Schaller et al.). This truly is a puzzling observation, and the answer may indeed be linked to layering and interactions between adjacent layers, as the authors hint at (which are captured in air content, but not in the presented data). Presenting previous studies in this light would do justice to the quality of that work and the researchers who made those pioneering contributions.

Also, the glacial d15N problem is addressed in several locations, but not explained well. The classic reference for this problem is Landais et al. 2006, and more recently Capron et al. 2013. The issue is most obvious in d15N, with the data suggesting a thinner glacial firn column, and the models simulating a thicker one. The consequences for Dage are not as well known, mostly because there are no absolute Delta-age constraints in Antarctica to calibrate the models to, like there are in Greenland (thermal d15N fractionation). The d15N and Dage implications are conflated in the manuscript, and could be clarified.

Specific line comments:

Title: The phrase "universal law" seems overbearing. First, the concept of universality in physics has a specific meaning, namely that near critical transitions, dynamical systems display scaling behavior that becomes independent of the details of the system being studied. This has not been demonstrated. Second, the fitting parameters (Table 2) are surprisingly different for the three sites, reducing the suggested "universality" of the behavior. I recommend that the authors revise the title of their manuscript. An example of a revised title could be: "Critical density of gas enclosure in polar firn independent of climate", or similar.

Page 1 line 5: Consider changing "universal" to "climate-independent".

Line 7: rephrase "misleading". How about: We demonstrate why indirect measurements suggest a climatic dependence

Line 10: "This may further help resolve. . .."

Line 22: change "safely" to "correctly"

Line 25: This is strangely formulated. The lock-in zone is commonly defined based on diffusivity (depth where d15N enrichment stops), rather than bubble closure. Of course the two overlap in depth. . .

Page 2 Line1 and throughout the paper: "firn model" is too vague. Specify whether you mean "firn densification model" or "firn air transport model".

Line 5: ". . .firn height and an indirect constraint on Dage (Sowers et al. 1992)."

Line 6: Severinghaus et al. interprets the thermal d15N signals, rather than the gravitational ones, so not the most logical citation. Also, what are the "other dating methods" referred to on line 7? For Greenland, firn densfication models do a good job based on empirical Dage constraints from thermal d15N, see e.g. Schwander 1997, Goujon 2003, Kindler 2014, Buizert 2014, Guillevic 2013, etc.

Line 12: What does "statistically solid" mean? I would just say: "we present an extensive data set of..."

Line 14: replace "misleading". See my suggestion for the abstract.

Line 26: is there a reference for the Otsu method?

Line 26: please specify that you look at the pore coordination number, correct? Normally when discussing the coordination number in firn, the coordination number of the ice grains is meant.

Page 3 Line 6: *At the* percolation threshold....

Line 6: I think you mean fraction rather than percentage.

Line 10-12: about the porosity range, what is this statement based on? Give a reference, or describe how this is seen in the data

Equation (1):

* Define all symbols

* The work by Mitchell et al. 2015 shows how layering can be introduced using parameterizations based on "local" (small-scale) samples to derive bulk properties. This is very important, because in modeling, firn properties are described as a function of depth, rather than porosity. When moving from porosity to depth, layering needs to be incorporated (going from local to bulk properties, in the language of Mitchell et al). Mitchell et al. use the functional form by Schwander 1989. To make the current work more accessible to firn air modelers like me, could you please try to fit the functional form of Schwander 1989, so that we can keep using the Mitchell et al. framework, but now with improved observational constraints?

* Again for practical modeling efforts, it would further be worth having just a single best fitting curve, rather than three separate ones. Maybe that could be provided also?

* I understand that the authors may think the last two points are an over-simplification, but please understand that it would greatly enhance the usability of your data in practical applications, which is an important motivation for doing detailed process studies like this.

* Do you think the extensive melting at Renland could explain why that site looks so different? Even the non-melted layers were exposed to near-melting summer temperatures in the upper firn.

Line 29: This makes no sense to me. Does "extract" here refer to the collection of the sample from closed pores (usual meaning), or removal of air from open by vacuum pumping that is then discarded?

Page 4 line 2: Note that most of the Martinerie samples are done on relatively mature ice (as opposed to lock-in samples used here). In mature ice the cut bubble correction should be smaller and relatively simple as most bubbles are spherical and unconnected.

Line 5-10: I think there is some confusion in nomenclature here, as the authors point out. Close-off is not a well-defined term, and means different things to different people (which does not mean previous authors are wrong. I also don't agree with the statement that this is due to attempts to make sense of corrupted data. It is just a different approach). The Goujon/Barnola close-off is an air-content close off, i.e. the density at which the total porosity matches the air content in mature ice. From Eq. (9) in Goujon et al. it is obvious that their definition of the close-off porosity is different from the one used by the authors. I would suggest that the authors try to clarify this by using a more refined vocabulary. They could explicitly define close-off as the point at which a thin firn layer has zero open porosity, and that their definition differs from definitions used by others such as the air-content based definition by Barnola. They could e.g. refer to their definition as the “full close-off” as opposed to the “air content close-off”.

Line 20: the relation between layers reaching close-off and the extent of the lock-in zone (as defined in the gas literature as the zone between where d15N enrichments stops and the deepest pumping depth), is an interesting one. Could you elaborate, and perhaps even give some numbers?

“Sealing” is a difficult phrase, though. While diffusion is strongly inhibited in the lock-in zone, gas flow still happens. We know this because the air content in mature ice is much lower than what would be expected if the lock-in zone were really sealed. The timescale for pressure adjustment is just much shorter than that for diffusive adjustment, which means the gases are effectively sealed from diffusing following Fick’s law, but not from permeating following Darcy’s law (this also gives rise to dispersive mixing, as shown by Buizert and Severinghaus 2016). Since the gases diffuse and permeate through the same pore space, the only difference must be one of time scale.

Line 31: how is air content measured? Cannot be done with micro-CT, as bubble pressure is unknown.

Page 5 Line 16: This is not always true. At Greenland sites and WAIS Divide things look good.

Line 22: this makes no sense to me. How does the Vostok d15N mismatch explain 1000-2000 years? I think the d15N and Delta-age problems are conflated here. Direct Dage constraints are problematic, as Bender concludes.

Figure 2: why not show the actual DE-08 and Summit data, rather than your approximation? I think your comparison is unfair here. Typically one would apply a cut bubble correction in the range of 5-10% to those data, at which point the closed porosity of the deepest samples would go to 100% (see e.g. Fig. 3 of Mitchell et al. 2015).

Figure 3: You’re comparing apples to oranges, because these are two different definitions of the close-off. I suggest you specify in the caption that you’re comparing the full,

single layer close-off (your study) to the air-content (bulk) close-off (martinerie). The difference in temperature dependence is due to some poorly understood interaction between adjacent layers, not an "attempt to interpret corrupted data".

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