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The Cryosphere
Discussions

Interactive comment on "Multi-tracer study of gas trapping in an East Antarctic ice core" *by* Kévin Fourteau et al.

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Fourteau et al. present a detailed and data-rich study of the gas trapping process at an East Antarctic site called "Lock-in". This is the first time such a detailed, multi-proxy study has been performed on a core from the East Antarctic plateau, making this a very valuable addition to the firn air literature. The authors come to several important conclusions. Particularly interesting is their observation that several aspects of firn morphology and connectivity are simply linked to local (i.e. small-scale) density, suggesting the latter to be the key controlling parameter independent of the relation of the local density to the bulk density at that depth. The paper is very well written, and easy to follow with good use of figures and references throughout. I am highly supportive of this paper, and think it should be accepted after minor revision.

My main suggestion for improvement concerns the observation that the firn air bubble trapping model of Rommelaere et al. overestimates air content compared to the observations. The authors suggest one solution, namely that closed pores may densify at a lower rate than open pores. As the authors themselves also observe, this explanation seems very unsatisfactory given that the hydrostatic overburden pressure that drives densification is several orders of magnitude larger than the air pressure difference between open and closed pores.

Mitchell et al. 2015 note a similar mismatch at WAIS Divide when using different parameterizations of closed porosity (green and blue curves in their figure 6C), in which case the model overestimates the air content. The stochastic closed bubble parameterization proposed in that study did simulate the correct air content (red curve). Perhaps what Fourteau et al. observe is the same phenomenon?

Mitchell et al. note that the density-closed porosity parameterizations found in the literature (similar to figure 2C in the current paper) are derived using local (i.e. cm-scale) data, and therefore cannot be applied to bulk (i.e. m-scale) density data (their page 2565, last paragraph). Firn models require smooth density and porosity profiles, but the order in which the smoothing is applied matters. Compare the following two scenarios:

Scenario A: Local density record is smoothed to produce bulk density curve. rho-CP parameterization is applied to bulk density curve to obtain bulk CP curve.

Scenario B: rho-CP parameterization is applied to local density record to obtain local (hi-res) CP record. Local CP record is smoothed to produce bulk CP curve.

From reading the text, it appears that Fourteau et al. used scenario A. This is the common approach in the firn air modeling literature, which is strictly speaking incorrect in my view. As Mitchell et al. argue, Scenario B is the correct one. Using scenario B instead of A will reduce the simulated air content, because much of the trapping is shifted to deeper depths. Perhaps the authors did already use approach B, in which case this point is irrelevant (please clarify in the text). However, the authors are still encouraged to try the stochastic porosity parameterization of Mitchell et al. (their Eq. 5-9) to see if this can reduce the model-data mismatch.

Hopefully this approach can explain the mismatch seen by the authors.

Other comments:

P2 L10: Witrant et al. investigate the thickness of the lock-in zone, which is technically speaking not the same as the trapping zone.

P2 L23: Note that this phenomenon was also recently observed at Styx Glacier by a Korean team (Jang et al., https://doi.org/10.5194/tc-2019-17).

P7L25: consider replacing “airtight” with “fully closed” or “mature”

P8L1-5: Consider citing the original papers on this method, such as Chappllaz et al 2013 and Stowasser et al. 2012

P9L19: consider citing Freitag et al. 2002 here

Figure 2, caption. Perhaps you can emphasize that the rho-CP relationship applies at the cm-scale, and cannot be applied to bulk density data.

Figure 2: Could you add a panel where you compare your new data to the various rho-CP parameterizations that are found in the literature? Barnola, Schwander, Mitchell, etc?

Figure 2: It is very interesting to see that applying the cut-bubble correction correctly (the way you do) makes the transition at CP=1 much sharper/abrupt. That is an interesting finding. The prediction from percolation theory is indeed that the transition should be abrupt – however, previous data sets often made this transition appear somewhat smooth. This difference has long puzzled me, and so the smoothness may have always been an artifact of the cut-bubble effect. Please discuss this briefly. In Mitchell et al. we introduced the parameter sigma_co to fit the smooth transition in the available

porosity data (page 2566); based on your observation, we may have overestimated the value of this parameter (could you suggest a better value?!).

Equation 3,4: it is common in physics to use single (not multiple) letters to denote variables – this avoids confusion when multiplication is involved. Why not use f instead of frac and R in stead of CP, and V instead of AC (or similar). “AC” in an equation makes one think you’re multiplying “A” and “C”.

P12 L15: “close” instead of “closed”

P13: Can you estimate the gas ages at 122 and 145 m depth, for our estimation?

A dome site like EDC probably has more stable accumulation than a flank site like vostok or lock-in – as the site moves over basal topography the surface slope (and thereby snow redeposition by winds) can change.

P13 L10: estimate the “effective” density of the firn closure

P14L1: the “effective” total porous volume of air isolation

P14 L22: see Mitchell et al. (2015) Figure 4, who also found that models overestimate air content when using standard parameterizations

P14 L18-22: please be explicit about the order of calculating CP from rho and performing the smoothing. So did you use scenario A or B from my example above

P16 L17: Why not use fractions 0.35 and 0.5 rather than percentages? You state alpha is between 0 and 1

P16L23: I agree with this reasoning. Maybe state also that the driving overburden hydrostatic pressure is much much larger than these small differences in bubble air pressure, and therefore alpha=1 is very much expected.

P23 bottom line: Maybe also add Freitag et al. 2013

P27 L3: typo, “to” should be “too”

Data availability: what about the CH_4 data?

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