

## ***Interactive comment on “The physical basis for gas transport through polar firn: a case study at Summit, Greenland” by A. C. Adolph and M. R. Albert***

**Anonymous Referee #2**

Received and published: 11 July 2013

The authors report new permeability and diffusivity data measured on firn samples from Summit, Greenland. They investigate the numerical relationship between the two data sets and compare them with modeled relationships between data and some physical parameters. They compare the diffusivity profile of Summit firn with indirectly (from known trace gas evolutions) obtained diffusivity profiles. Previous discrepancies between the two methods are confirmed. Except for the new data from firn samples this paper largely lacks the quality of new scientific findings. The discussion of permeation and diffusivity relations is mostly purely numerical and lacks basic physical context (Title!), like for example the different influence of pore geometry on diffusivity and permeability (viscous flow versus random walk). The important aspect that, especially

C1060

near the lock-in zone, open porosity instead of total porosity is the relevant factor for diffusivity and permeability is not considered. One main conclusion, the necessity of investigating the 3-dimensional aspect of air mixing, has been discussed for many years and has been mentioned in previous publications (Buizert, 2012; Trudinger, 2013). The paper does not provide any suggestions on how this topic could be brought ahead, as promising in the abstract: "Guidance is provided for development of nextgeneration firn air transport models." And only if such investigations could be made for past firn conditions the interpretation of ice core records could be improved. I recommend to either limiting the paper to the presentation and discussion of the new permeability and diffusivity data, or to substantially expand and improve the physical interpretation, with qualified suggestions for future firn air modeling.

Specific comments.

p. 2456, l. 24: ".. which separates the atmosphere from the ice cores .." Use glacial ice (or similar) instead of ice core.

p. 2457, l. 9: "and CO<sub>2</sub> records" Not only CO<sub>2</sub>, but all record extracted from the bubbles.

p. 2457, l. 16: ".. because they had not been measured ..". This is only partly true (as also mentioned in this manuscript). E.g. diffusivity has been measured and used for modelling, e.g. in Schwander et al. (1993).

p. 2459, l. 4: "Because of its location at the highest elevation in Greenland..". Summit, the usual designation of the GISP2 drilling site, is not at the highest elevation. It is located about 30 km on the western slope from the highest position (the GRIP drilling site). The two locations should be distinguished, although the climatic differences might not be relevant for the present study, but there are differences in average pressure, accumulation rate etc. "Summit has experienced a relatively constant accumulation rate in recent history.." This is an empty phrase: What means relatively constant? What is the context?

C1061

p. 2459, l. 18: "Porosity is then calculated as  $1 - \rho_{\text{firn}} / \rho_{\text{ice}}$ ." As mentioned above, open porosity should be considered, when discussing parameters influencing diffusivity and permeability.

p. 2463, l. 16-26, p. 2464, l. 1-13: Is there a justification for a linear model? A linear approach is questionable considering the physics involved, especially for  $k$  or  $\log(k)$  (compare e.g. Hagen-Poiseuille equation). With such an approach, one might miss important features especially at low permeabilities in the lock-in zone, despite the relatively good  $R^2$  value. And the fit is based on a single site only. So what is the value of this model? SSA, pore size and porosity are by far not independent and therefore not an ideal basis for discussing percentages of variability. The importance of pore size for the permeability is trivial considering the physics of viscous flow. Instead of the numerical game, a physical approach for inferring tortuosity and total flow resistance from measured microstructures (like e.g. pore size distribution) would be more helpful.

p. 2465, l. 8: " ..  $c$  is a calculated (not experimentally determined) constant." What value has been used for  $c$ ? If the average equivalent circle pore diameter is used instead of the critical diameter, then it would be surprising to get a good approximation by eq. (8) when using the standard value for  $c$ ? Whether or not  $c$  has been adjusted: What is the meaning of the comparison shown in Fig. 5 and 6?

p. 2465, l. 9: " .. is the minimum diameter present of the pores that are connected through the whole sample." This can be misunderstood. Actually it is the MAXIMUM diameter of the pores connected through the sample, measured at their NARROWEST part. In mercury intrusion experiments this corresponds to the pore diameter reached at breakthrough. It is a minimum reached at this point, but this doesn't mean that there are smaller pathways through the sample. Indeed a contradictory formulation appears in Garboczi (1990), p. 597: " .. minimum diameter of pores which are geometrically continuous .." vs. "maximum continuous pore radius".

p. 2465, l. 12: equivalent -> equivalent

C1062

p. 2466, l. 28: "Though this causes decrease in the diffusivity across the depth, the permeability is relatively constant". This is not correct. As demonstrated in Fig. 3 the slope (relative change) of permeability is always higher than the one of diffusivity. (Whether the x-axis is porosity or depth is not important for this behavior).

p. 2467, l. 7: " ... but it is in the application of these measurements to firn air modeling where these metrics can inform our methods for reconstructing gas age-ice age differences." The authors do not provide any clue on how this should be implemented in firn air modeling. We must consider here that gas age-ice age difference is predominantly determined by the age of the ice at the fir-ice transition, thus by the applied densification model. Further, as pointed out by the authors, the physical properties of firn samples do not represent the effective in situ processes of air mixing.

p. 2469, l. 8: transport -> transport

p. 2469, l. 22: "We do not expect a high degree of lateral inhomogeneity (spelling!) at Summit due to the fairly constant and relatively high accumulation rate, generally calm wind conditions, and radar surveys showing generally flat layering at Summit". Well, this is all relative. Storm systems are passing Summit every few weeks. Lee and luv sides of sastrugis have distinctly different microstructures. After all there must be some reasons for the discrepancy between sample level diffusivities and effective (in situ) diffusivity profiles.

p. 2470, l. 4: " .. leading to a higher concentration ..". Why higher? Where higher? I agree with a step in concentration but it could have either sign, depending on what happens in the atmosphere above.

p. 2470, l. 7, p. 2472, l. 2: variance -> variance

p. 2472, l. 5: " .. indicate that molecular diffusion can indeed occur in the lock in zone of firn." It is valuable that the authors present diffusivity measurements on firn samples from the lock-in zone. But it is trivial that samples with non-zero permeability will have

C1063

non-zero diffusivity, everything else would be mysterious. Other more important processes could be discussed here: E.g. any downward flux is effectively cut off when upward advection due to expelled air from the lock in zone locally dominates molecular diffusion (see e.g. Severinghaus, 2006).

References:

Schwartz et al. 1993 not found in References

Spelling of Faïn in text

Spelling of Hörhold in text and References

Severinghaus et al. 2001 not found in text

Severinghaus et al. 2006 not found in text

---

Interactive comment on The Cryosphere Discuss., 7, 2455, 2013.