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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 #1

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The article presents innovative measurements of diffusivity (using an improved methodology) and the first permeability profile over the whole depth range of the firn. These datasets of gas transport parameters are valuable and the extension of this work to other polar sites undergoing different climatic conditions should be encouraged. However the manuscript may have been written somewhat too fast, the analysis of the results has confusing aspects and I think that the comparisons with some related work tend to present other approaches in a too negative way. The abstract and introduction show very high ambitions in terms of direct implications for large scale modelling of gas transport in firn, which are not really necessary for the value of the paper. I think that these implications should be presented in a more careful and precise

way. Suggestions are provided below.

General comments

The main uncertainties in structural parameters related to the diffusivity and permeability profiles should be discussed, in particular the firn core conservation (e.g. frost in the bags?), shape regularity for density measurements, and the methodology for micro-structural parameters determination.

The role of the open/closed porosity ratio should be considered in the deep firn.

The "black box" models (this term should be changed) of gas transport in firn aim at representing the large scale properties of firn and are more successful at doing it than the manuscript suggests. Their primary aim is to relate concentrations in firn to atmospheric trends at a multi-decadal time scale. Computing time efficiency requires a different approach than detailed gas transport in an observed (micro-structural) 3D porous network as done in Freitag et al. (2002) but not applicable to the whole firn and long time scales. The two approaches are both valuable but pursue different aims and will not converge in the near future, because extrapolating micro-scale parameters to multi-decadal gas transport parameters is not trivial.These scale issues should be better considered, several aspects are discussed in the cited references (e.g. for diffusivity in Fabre et al. (2000) and open/closed porosity ratio in Freitag et al. (2002)).

In my understanding, the most promising perspective for the "black box" models from this study is to explore the relative roles of permeability related transport driven by pressure gradients (Eq. 2) and molecular diffusion related transport driven by concentration gradients (Eq. 1). This would require significant model developments (and is of course beyond the scope of the manuscript) but more specific recommendations could be provided in terms of improving the parametrised representation of firn physics in these models (especially how to best relate permeability to simple bulk firn parameters such as porosity).

Specific comments

p.2456 l.18-21: (last two sentences of the abstract) the precise recommendations for future large scale firn modelling provided by this study are unclear to me at this stage.

p.2457 l.15-16: this is not true for diffusivity. The main motivation is the macro/micro scale issue discussed in Fabre et al. (2000), as mentioned in e.g. Buizert et al., 2012, p4260 and Witrant et al., 2012, p.11466.

p.2457 l.19-23: this negative vision of large scale models of gas transport in firn should be reconsidered. The three cited articles deal with a recent advance in such modelling: taking into account multi-gas constraint allows to better determine the large scale effective firn diffusivity. Their discussion of uncertainties should not be confused with model failure at achieving their aim (simulating the main features of large scale gas transport in firn).

p.2457 l.26-27: this statement should be either more general (e.g. better understanding firn physics would help improving these models) or more specific (which assumptions would not be required?)

p.2458 l.2-7: the main methodological improvement(s) (absence of carrier gas flow as I understand it) should be introduced.

p.2459 l.16-18: density data are important in this study as they are used to calculate the sample porosities. The uncertainties should be introduced. The density data presented on Fig. 2 show a high variability which could be related to sample size, uncertainty on sample shape, natural variability, etc. Lomonaco et al. (2011) estimated the sample densities from imaging and obtained less dispersed data on their Fig.5, could the two approaches be compared?

p.2461 Section 2.4: this short section should be extended and clarified in several aspects. The motivation for changing the thresholding procedure and its impact on the results should be explained. Lomonaco et al. (2011) used samples of smaller size

than those used for diffusivity and permeability measurements. micro-CT sample sizes should be commented. d_{eq} was not a target parameter in Lomonaco et al. (2011), this quantity should be better introduced (uncertainty, previous use, significance in relation with gas transport). Are there significant differences between the results of this study and Lomonaco et al. (2011) which focussed on fine-grained layers? I did not find a mention of a relationship between SSA and tortuosity in Lomonaco et al. (2011).

p.2461-2462 Section 3.1: Fig.1 suggests that the variability of permeability and diffusivity both show a common roughly decreasing trend with increasing depth. This could be interesting to mention. An analysis of the variability of diffusivity and permeability at similar depths would be interesting: do they co-vary? Is the variability correlated to some firn structural parameter? Attention was paid to fine/coarse grain size layers in sample selection and cutting, do the fine/coarse grain layers show different permeability and/or diffusivity?

p.2462 l.19-24: Freitag et al. (2002) related permeability and diffusivity to open porosity, not total porosity. This could significantly bias the comparison results at low porosity values. An evaluation of this effect could be performed using a parametrisation of the closed/total porosity ratio (e.g. Schwander, 1989 or Goujon et al., J. Geophys. Res., 108(D24), 4792, 2003).

p.2463 l.3-6: this sample size issue should also be discussed in terms of large scale firn models versus data comparison.

p.2463 l.9-12: these strong statements contradict the correlations in the next section (p2464 l.3-5) and should be rephrased.

Equations (5) to (8) and regressions on Figs. 7 and 8: the different ways diffusivity and permeability are related to structural parameters are confusing. Eqs. (7) and (8) linearly relate permeability and diffusivity whereas a logarithm is used for one but not the other in Eqs. (5) and (6). Eq. (7) suggests that permeability should be linked to (1/SSA) squared whereas the logarithm of permeability is related to SSA in Eq. (6).

Have different formulations of Eqs. (5) and (6) been tested? The physical relationships between the different parameters should be better discussed. Such choices are important for possible future implementation as scaling laws in large scale firn models but clear recommendations are not provided.

p.2465 l.1-2: the presence of total porosity in the right term of Eq. 7 should be explained.

p.2465 l.8: the value of c should be provided

p.2465 l.18-21: I find the results of the Kozeny-Carman and Katz-Thomson approximations fairly good. On Fig.6, large deviations occur only at low porosity values where the open/closed porosity ratio and minimum measurable permeability could play a role. A large number of data points are very well fitted. Do the less well fitted points show some specific physical structure ? I find it surprising that the simpler Katz-Thompson parametrisation, where a minimum diameter is approximated by an average diameter, gives better results. Is it because c is used as an adjustable parameter ? These interesting results should be further discussed.

p.2466 l.4-13: It is unclear to me whether the SMI used in this section are those measured in this study or the fine grained layers only SMI values of Lomonaco et al. (2011). This long introduction could be shortened.

p.2466 l.15-18: The linear relationship between diffusivity and permeability below 40 m depth is not reflected in the choices made in Eqs. (5) and (6) to correlate diffusivity and permeability with structural parameters. I suggest to combine sections 3.1.2 and 3.1.4 making consistent choices in the way to relate the different parameters.

p.2466 l.23 - p.2467 l.4: This conclusion is confusing as it contradicts the primary role of porosity derived from Eqs. (5) and (6). In the perspective of large scale firn modelling, the quality of the correlations that can be found between simple bulk parameters (e.g. porosity or open porosity) and micro-scale parameters is of importance. This should

be kept in mind in the analysis.

Figs.7 and 8: In the perspective of modelling and comparison with future measurements at other sites, I would provide a continuous regression on Fig.7 (the large discontinuity is not consistent with the overall dataset) and plot SSA and d_{eq} versus porosity on Fig. 8.

p.2467 l.26 - p.2468 l.23: The strong physical link between permeability and Darcy's law on one side and convection and wind-induced flows on the other side (e.g. Colbeck, J. Glaciol., 35(120), p.209, 1989) could be made clearer for non specialist readers. Note that Kawamura et al., Atmos. Chem. Phys. Discuss., 13, p.7021, 2013 question the validity of the exponential decay form in Eq. (9) and suggest to take into account permeability variations.

p.2469 l.14-21: It should be mentioned that this explanation is consistent with the one given in Fabre et al. (2000): sample size issue and scale effect.

p.2469 l.26 - p.2470 l.11: I suggest to suppress this qualitative discussion, which is not supported by data. Note that high concentrations caused by retarded gas motion are not observed in the very heterogeneous Devon Island firn where multiple refrozen melt layers are present (e.g. Witrant et al., 2012), and that the somewhat irregular trace gas concentration profiles at Devon Island can be matched by a "black box" model.

p.2472 l.6-12: This conclusion should be revised: the proposed approach cannot improve the results of large scale firn models which aim at best relating past atmospheric trends and concentrations in firn. On the other hand, developing small scale 3D gas transport models as initiated by Freitag et al. (2002) could lead to a better understanding of gas transport processes.

References: M. R. Albert co-authored almost 40% of the cited references. This high self-citation rate does not seem appropriate to me.

Technical corrections

p.2456 l.7-8: ”data which is retrieved to analyze” could be rephrased: data retrieved to reconstruct

p.2456 l.23-25: this sentence could be split in two: Firn is A complex porous material, comprised of aged snow more than one year old. It separates the atmosphere from the ice which preserves a unique record of atmospheric history.

p.2456 l.25: I would suppress “the pore space within”

p.2457 l.4 and 10: this choice of references seems American-oriented to me.

p.2459 l.9-10: this long list of self-citations should be shortened.

p.2459 l.20: I would replace “that evolves from” with “related to”

p.2459 l.21-22: on selected firn samples using a technique designed for use on firn cores that has been validated on glass beads

p.2461 l.21 and Fig.1: the definition of “impermeable” should be provided (I guess it is based on the lowest measurable permeability given at p2471 l.8-9). I suggest to use different symbols for samples with/without measurable permeability on Fig.1.

p.2461 l.24: A peak

p.2462 l.7: comparable

p.2464 l.22: Schwartz et al., 1993 not in reference list

p.2466 l.4: “Using this information can inform” should be rephrased

p.2468 l.4: dominated

p.2469 l.8 and p.2472 l.9: transport

p.2471 l.22: “Effect tests” is unclear to me

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