Referee comments are indicated by boldface type. Author responses are indicated by italics.

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.

This is a constructive suggestion that will help improve the paper. In revisiting the manuscript, we softened the portrayal of firn modeling, and changes have been made accordingly throughout the article to focus this paper on the physical properties of the firn column and their interrelationship. The title, abstract and major focus of the paper have been revised, as we agree that doing so improved the paper.

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.

We have added information about the preservation of the firn cores and the change in density experienced during storage in the section describing the firn samples. Measurement errors are now discussed in the diffusivity and permeability methods section. The effects of firn core sample shape irregularities are also discussed in the density and porosity methods section, and methodology for microstructural parameters has been expanded.

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

We have taken into account the issue of open/closed porosity by applying the Schwander (1989) parameterization for determining open/closed porosity. In the cases where this makes a difference in the analysis, we have used the Schwander (1989) approximation, but if the difference is insignificant, we use our measured total porosity to avoid obscuring our measured values.

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).

We have greatly scaled back our comparison and our attempt to provide "suggestions" for modeling, as it does detract from the presentation and discussion of the new data. The revision addresses parameterization based on known firn parameters (like porosity) or incorporation of direct measurements into the fundamental permeability and diffusivity equations when applicable.

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.

This sentence has been removed to reflect the change in scope of the paper.

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.

This sentence has been removed to reflect this comment.

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).

In the revised paper, the discussion of modeling has been greatly scaled back; this section of the introduction has been removed.

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?)

This sentence has been removed in the revised version to reflect the change in focus.

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

The following sentence has been added: "This technique is based on the measurement of purely diffusive transport, instead of extrapolation from convective transport measurements."

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?

We have included a new section on density measurement and porosity determination, which includes sources of error. The Lomonaco study was restricted to fine-grained (winter) layers and their evolution through the firn column, it did not address the seasonal layering effects that generate layers of fine grained firn interspersed with coarse grained firn; hence, the increased variability in this paper is to be expected.

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. deq 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).

We have expanded this section to include more methodology and information about the parameters used.

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?

This is a constructive suggestion. The revised paper contains more information about the relationship between diffusivity and permeability, and particularly differences between fine and coarse-grained firn.

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).

In this case, we are using the large permeability dataset and many of the samples have a significant closed porosity, so we have used the Schwander (1989) parameterization to determine open porosity and have changed our analysis accordingly.

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

The issue of scale is now discussed in the shortened and revised comparison to firn models.

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

The statements have been removed as we have revised which analyses are included in the paper.

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).

We agree that this analysis was confusing, and it has been removed.

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.

These formulations have been removed and replaced with different parameterizations based on porosity, as we believe this will be more relevant for the firn community.

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

The presence of porosity in the equation was erroneous and has been removed.

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

The value of c (1/226) has been added to the text.

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.

We have expanded our discussion of the Kozeny-Carman and Katz-Thompson approximations. The value of c (1/226) used in the Katz-Thompson parameterization is not adjustable. We have found that the coarse and fine grained firn permeabilities are respectively under-predicted and over-predicted by these two formulas and have added a discussion on this finding.

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.

We believe that the discussion of SMI is no longer relevant, and has since been removed.

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.

We have incorporated a new continuous, relationship between diffusivity and permeability, and we believe that the new presentation of these results is more cohesive.

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.

We agree that the previous conclusion was confusing, and in the revised manuscript have removed that part of the data 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 deq versus porosity on Fig. 8.

This is a constructive suggestion from the modeling perspective. We have provided a continuous regression to relate permeability and diffusivity, and we feel that the presentation of SSA and d_{eq} *in a figure is no longer relevant.*

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.

We have minimized our discussion of convection to reflect the focus on the measurement results, and we believe that this suggestion is beyond the new scope of the paper.

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.

This section in particular has been removed, but a discussion of the similarity between our results and those of Fabre et al. (2000) is included.

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.

In the shift of focus of the paper, this qualitative discussion has been removed.

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.

This conclusion has been removed.

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

With the addition of some new sources and removal of others, the percentage of self-citation has been reduced. We note that most published permeability measurements have been made by the Albert group.

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

Sentence has been removed due to changed content.

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.

Sentence has been revised as suggested.

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

This sentence has been revised to remove that phrase.

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

Non-American references have been added.

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

The list has been shortened.

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

Change has been made.

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

Change has been made.

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.

The lowest measurable permeability is now included in this section and the figure has been changed as suggested.

p.2461 l.24: A peak

This has been changed to "The maximum."

p.2462 l.7: comparable

Change has been made.

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

Reference added.

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

Section is no longer in the paper.

p.2468 l.4: dominated

Sentence is no longer in the paper.

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

Sentence is no longer in the paper.

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

This analysis has been removed from the paper.