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

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 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 next generation 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.

We have revised the paper to focus it more clearly on the physical properties and their interrelationships and have reduced the discussion of modeling.

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

Sentence has been edited as suggested.

p. 2457, l. 9: "and CO2 records" Not only CO2, but all record extracted from the bubbles.

"CO2 records" has been changed to "atmospheric gas records."

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).

We agree and have removed that paragraph from the paper to reflect the change in focus.

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?

We have revised our description of the site location to clarify this.

p. 2459, l. 18: "Porosity is then calculated as 1−rho_firn/rho_ice." As mentioned above, open porosity should be considered, when discussing parameters influencing diffusivity and permeability.

We have used the Schwander (1989) parameterization of closed porosity to consider the effects of open/total porosity. In cases where the difference in open and total porosity affects the analysis, we have used the parameterization. If the difference is not significant to the results, we have used total porosity to avoid obscuring our direct measurements.

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 Rˆ2 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.

We agree that this analysis could be improved and have since removed the linear models in favor of relationships that have foundations in gas transport literature (Archie's Law for diffusivity and a parameterization similar to that developed in Frietag et al. (2002) for permeability)

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 for 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?

The value used for c is that calculated by Katz and Thompson (1986); c = 1/226. We have included that information in the body of the paper.

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".

We have reworded the text to clarify this point.

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

Change has been made.

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).

This particular aspect of the discussion has been removed.

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.

We revised the paper to focus on the physical properties more than on the modeling, and the new scope of the paper reflects that.

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

Change has been made.

p. 2469, l. 22: "We do not expect a high degree of lateral inhomogenaity (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.

This relative statement has been removed from the paper.

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.

This discussion has been removed from the paper as a result of the change in focus.

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

Sentence is no longer in the paper.

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 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).

Since the paper has shifted focus, we find that this is beyond the scope of the paper.

References:
Schwartz et al. 1993 not found in References

Reference added.

Spelling of Faïn in text

Spelling corrected.

Spelling of Hörhold in text and References

Spelling corrected.

Severinghaus et al. 2001 not found in text
Severinghaus et al. 2006 not found in text

Unused sources removed.