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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Gas transport in firn: multiple-tracer characterisation and model intercomparison for NEEM, Northern Greenland" *by* C. Buizert et al.

Anonymous Referee #2

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The submission "Gas transport in firn: multiple-tracer characterization and model intercomparison of NEEM, Northern Greenland"..." by C. Buizert et al. presents an extensive effort to develop a more robust and powerful method for determining the diffusivity of polar firn using a wide variety of passive tracers. The authors then use this method with 6 different firn models to characterize the age (and age spread) of air in the firn at the NEEM drill site, as well as the gas-age/ice-age difference. Finally, using 4 different, carefully chosen, hypothetical tracer histories, the authors compare the firn models with one another to get a better understanding of the strengths and weaknesses of each model.

Overall, this is nicely written paper on an important topic. It presents a great deal of carefully done work in a clear and thorough fashion. The paper does not represent a radically new approach, nor will it bring about dramatic changes in the modeling of firn air. However, it greatly advances procedures that have already been used elsewhere, making them rigorous and quantitative. The model intercomparison work also shows the relative importance of including various physical processes in firn models. Finally, the specific application to the NEEM site highlights two new and important phenomena in firn air: vertical non-diffusive transport within the lock-in zone, and measurable lateral differences in firn air over distances as small as 64m. To the best of my knowledge, these observations are the first to clearly show vertical air transport in a "conventional" lock-in zone (the Megadunes site, with its network of vertical fractures is not conventional) and are also the first to show disagreement between "replicate" boreholes.

This paper is clearly appropriate for this special issue of ACP, and meets the standards of relevance, importance and quality. I feel it is suitable for publication after one error has been corrected, and some minor concerns have been addressed.

First the error: P15989 Eq. 3: The RMSD term that defined is very close to Chi-squared-per-degree-of-freedom. The only difference is that, to be a true chi-squared variable, the divisor "N" should be adjusted for the number of free parameters in the fit to the diffusivity. Unless this is done, the conclusion that 'RMSD<1 implies conservative error estimates' is not strictly valid. Consider the limiting case of three data points that are fitted with a parabola. This will always yield RMSD = 0, regardless of whether the errors are tiny or enormous. Minimizing a correctly formulated Chi-squared figure of merit will give exactly the same optimized diffusivity as minimizing RMSD, but the values of Chi-squared can be interpreted quantitatively. Fixing this will also require reworking the discussion surrounding figure 4. I recognize that determining the appropriate number of free parameters may not be a simple task (and this may be why the authors chose RMSD as a metric), but the interpretation of RMSD as it stands is, at best, only qualitatively correct, and may be simply wrong.

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Other concerns:

Abstract: I would like to see the observation of inter-hole differences mentioned in the abstract. This is an important result and should be highlighted. I believe that it has been widely assumed in past firn air studies that adjacent boreholes will indeed give essentially identical results.

Section 1: The determination of diffusivity with the greatest possible accuracy is the raison d'etre for this work. However, it is also possible to learn a great deal about past atmospheric history from firn air using tracer-vs-tracer plots (rather than tracer-vs-depth) (e.g. Montzka et al. GRL 2009). This greatly reduces the model dependence on the firn diffusivity and the kind of high-quality tuning developed in this paper is unnecessary. This alternative approach should at least be mentioned in the introduction.

On page 15980, line 11-12 the authors state that uncertainties in the atmospheric reconstruction used to tune the diffusivity "are a large source of potential error". This is an important point and is underemphasized in the paper. It is only section 2.7 that any mention is made, and one can only assess the relative importance of the 7 potential sources of error by a careful reading of the supplementary material. Perhaps a table of the various sources of error would be a good addition to Section 2.7.

While it is touched on in the supplement, it should at least be mentioned in Section 2.5 that empirical correction for gravitation has the potential to also correct for mass-dependent sampling artifacts (Severinghaus & Battle, 2006).

Page 15988 and following: I really don't like the use of the term "dispersion" for the transport that is occurring through mechanisms other than molecular diffusion. In classical (and quantum) physics, dispersion indicates propagation in which different components of a substance move at different speeds (light waves in dispersive glass leading to chromatic aberration, etc.). This is exactly the opposite of what is going on in the deep firn. Here, we know the transport is not molecular diffusion precisely because the different species do move at the same speed. Eddy diffusion, bulk transport, or

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some other term is far preferable.

Section 3.2: How is the transition between the diffusive zone and the lock-in zone defined? Is it simply where the molecular diffusivity goes to zero? The location of this transition is essential for successful model tuning. Presumably the objective tuning processes that are used for the various models automatically find this transition, but this is not clear in the manuscript.

Section 4.1: We know from later in Section 4.2 that the treatment of gas movement within the lock-in zone is the most important of the differences between the different models. Thus, it is frustrating that the LGGE-GIPSA model uses a novel approach to the LIZ but is only fleetingly described here and references a paper that is still in preparation. It would be helpful if there were another sentence or two here clarifying the behavior of LGGE-GIPSA in the LIZ.

P15993 Line 7-8: I can see how the values in Table 1 lead to the "spread of around 30%" for the mean age and FWHM, but this statement doesn't look true to me for the spectral width. Please clarify.

Section 4.2.4 Line 14: The extreme difference between the CSIRO model and the others needs to be mentioned explicitly here, if only to say something like "The anomalous behavior of the CSIRO model is discussed in Section 5".

Page 16001, last paragraph: The first conclusion, to my mind at least, is misstated. The authors simply list aspects of the models that can be determined by examining their respective codes. The four diagnostic scenarios are not needed to see that some models have pressure-driven backflow and others don't. Instead, the four scenarios tell us which model-model differences are important, and under what circumstances. To my mind, these differences are what need more emphasis.

Minor editorial comments:

Page 15978 line 17: Missing reference Page 15987 line 6: This is the first appearance of "the CIC firn model". Either define CIC here, or make reference to Section 3.6. Page 15989 line 2 Remove "as" at the end of the line Page 15992 line 8: Replace "Especially, the well reproduced height. . ." with "In particular, the well-reproduced height. . ." Page 15992 line 14-15 should read ". . .most models require at least some dispersive mixing in the LIZ, since. . ." (although, as I point out above, the use of the word "dispersive" is unfortunate here). Page 15993 line 27: Should read ". . .by the ∼5% of the air that is trapped at depths. . ." Page 15999 lines 11-12: The values for the DF given here are from ice cores and are site specific. As written, the naïve reader might be led to believe that these values for DF are universal and generally applicable. Please clarify. Page 15999 line 13: Should read "For del13C of CO2, the observed range. . ." Page 16000 line 15: Should read ". . .from the aforementioned high-accumulation Law Dome. . ."

Supplementary Material:

Due to its length, I have not been able to give the supplementary material the same level of attention that I applied to the paper itself. Much of the material in the supplement is useful and important. However, some of the early sections in particular are more narrative in nature and cover the same ground as the main paper. To my mind, the supplement could be significantly shortened (and thus be more useful) if these sections were trimmed down. In addition, I do have a few brief comments.

Introduction: The sections in the main paper with supplementary support are marked with asterisks (not daggers).

Section 2.7.2: This statement is not clear to me: "Since the uncertainty estimates do not have a temporal resolution better than a few years, we believe this approach to be valid." I simply can't make sense of it. What are the authors trying to say.

Section 3.2: Open porosity is not necessarily zero below the depth at which firn air samples can no longer be extracted (see, for example, Aydin et al, ACP 2010).

Table 15: What is the difference between the two versions of Scenario II?

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Section 2.4.3: Uncertainties in the reconstructed atmospheric histories seem somewhat arbitrarily determined. For example, in the case of CO2, why choose half of the offset? Why 2sigma for the recent Law Dome record and 3sigma for the older Law Dome record? These choices may be good ones, but they are completely unjustified.

Section 2.7.6: What about the possibility of a seasonally rectified signal? For the highly seasonal species, there is the possibility of covariation of atmospheric mixing ratios and surface windpumping introducing a small bias to the annual mean vaues deeper in the firn. My guess is that this is negligible, but it should at least be mentioned.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15975, 2011.

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