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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Controls on the movement and composition of firn air at the West Antarctic Ice Sheet Divide" *by* M. O. Battle et al.

Anonymous Referee #1

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Battle *et al*. describe a recent firn air campaign near the WAIS-Divide deep drilling site and present new data from this campaign. They provide an extensive description of the different firn air transport mechanisms at the site, complemented with 1-D firn air model results. They observe an isotopic fractionation in δ^{18}O-O_2, which they convincingly link to molecular size fractionation in the non-diffusive zone.

As the authors themselves state in their conclusions, much of the work simply confirms what is known from other firn air studies, and is described in greater detail elsewhere. This somewhat reduces the impact of the work. However, the WAIS-D site is very relevant for atmospheric reconstructions, with a number of firn air and ice core gas records already published and more forthcoming. By showing WAIS-D is in perfect agreement with our understanding of firn air transport, Battle*et al*. increase confidence

in said records. The transport characterization presented in the work will be a valuable point of reference for WAIS-D.

The observed fractionation in molecular oxygen (and perhaps Ne) is an important new result, not previously reported in literature. The fractionation process has consequences for measured δ^{18}O-O_2 records in ice cores, and helps elucidate the molecular mechanisms responsible for close-off fractionation. Also the discussion on the influence of bubble trapping above the lock-in zone on Δage, which they support with new measurements on closed firn bubbles, is a valuable contribution.

The results are clearly presented and structured, and for the most part the paper is well written. There can be no doubt that the work fits well within the scope of the ACP special issue on firn air. All things considered, I recommend publication in ACP after several issues have been addressed by the authors.

Major issues:

1) Section 7 (Advection due to accumulation) contains some conceptual and methodical flaws (in my view). I suggest it should be revised. The authors state that "firn air has the potential to be advected downward" and "we can investigate the importance of this phenomenon at the intermediate-accumulation WAIS-D site".

Firn air must be advected downward, on purely theoretical grounds. Conservation of mass requires a flow of air in the open pores to replace the air that is trapped in the lock-in zone. This flow leads to advective transport, the magnitude of which can be estimated fairly accurately (e.g. Rommelaere *et al*., 1997). I do not see the point of investigating the relative importance of an effect that is known more precisely than any other transport mechanisms. For example, the diffusive flux needs to be reconstructed in an ad hoc way, giving much larger uncertainties.

Second, the method the authors choose to investigate the influence of advection is ambiguous. They compare two firn air models, one with, and one without advection. The authors use the same diffusivity-depth profile for both models, even though the physics of the models is different. If the diffusivity is optimized for the no-advection model, the advective model will perform poorly. And vice versa, if optimized for the advective model the no-advective model will perform poorly. Any outcome can be realized.

If the authors want to conclude something from comparing model output and data, they should tune the models separately, and perhaps introduce a quantitative way to assess the fit to the data. But, given the firm theoretical reasons for advection and its magnitude, I think the exercise could also be entirely omitted.

2) The authors present model results, but the description of the model input remains unclear. What density profile was used? What porosity parameterization? How were the three different models tuned? What reference gases were used? As the paper provides a rather technical firn transport description, such information is essential. I would suggest they expand section 5, and include such information. Also, the moving-coordinate model does not have a reference. If this is an unpublished model, perhaps some more technical details could be given (e.g. as a small appendix).

Minor issues / suggestions (in order of appearance)

1) In the abstract the authors state 1-2m of convective influence, but their estimates range from 1.4-5.2m in section 6.1.

2) In section 6.1 the slope fitting method is discussed. Perhaps it could be illustrated in Fig. 2 with a line?

3) In section 6.3 or 6.4 the artifact due to collection fractionation should be mentioned with a reference to section 10.

4) section 9.1/figure 4. Why does the δNe/N_2 vs $\delta O_2/N_2$ line not go through the origin (0,0)? This is the case for Severinghaus and Battle (2006). The equation given in the

figure (δNe/$N_2 = 24.7 \times \delta O_2/N_2$) leaves out the intersect.

5) At the end of section 9.1 the authors suggest that close-off fractionation is characterized by an activation energy. Can the results from WAIS-D South Pole be interpreted more quantitatively? (What is Q, for example, and does the temperature trend from the two sites fit the 1/T behavior)

6) In section 9.2, have the authors considered the effect of their observed δ^{18}O-O_2 fractionation on the ice core record? The observed insolation-driven O_2/N_2 variations are on the order of 10 permil (Kawamura *et al.*, 2007), which would imply around 0.1 permil variations in δ^{18}O-O_2 (using Fig 7). This is certainly not negligible compared to atmospheric δ^{18}O-O_2 variations observed in ice cores (Severinghaus *et al.*, 2009). For example, both the Dole effect and summer insolation have a strong 23 kyr precession signal, and hence δ^{18}O-O_2 effect.

7) In section 12 I do not understand the argument starting on page 18654 line 23. The authors state "this is because the age of the firn air increases with depth in a non-linear fashion in the diff column". Why does this make the effect of bubble trapping above the lock-in zone less severe? Also, I would think one should look how Δage changes with depth rather than the absolute gas age. Overall this argument is unclear to me as it is presented now.

Technical corrections

1) permil signals are missing in several (all?) places (e.g. p 18641 line 13, p.18645 line 10, p.18648 line 9, p.18653 line 10-12, p.18657 line 25)

2) In section 8 the (Orsi *et al.* 2011) reference is not included in the reference list

3) In section 11 the Mitchell *et al.* (2011) and Aydin *et al.* (2011) references are not included in the reference list.

4) page 18658 lines 9 and 11: typo δp vs Δp

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18633, 2011.