

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.

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Referee #1

We thank the referee for careful reading of the manuscript, thoughtful criticisms and helpful suggestions.

Major issues:

1) The referee is correct of course: advection must be happening. Furthermore, just as (s)he states, using measurements of air content in ice, we can quantify the advective flux well. The referee is also correct that tuning the diffusivity profile in a non-advective model can compensate for the absence of advection and potentially allow good model-data agreement, even when the set of physical processes included in the model is

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known to be incomplete.

On one point, we respectfully disagree with the referee: While we can calculate the advective flux with relative certainty, the importance of that flux is hard to assess a priori because the actual size of the diffusive flux is not known (and is only estimated after tuning).

This latter point was the motivation for the exercise described in Section 7 of our paper. We wanted to know whether the absence of advection in a model could be mitigated with tuning. Naturally, the answer to the question will depend, in part, on the accumulation rate at the site.

We addressed this question in several ways, including the referee's suggestion of separately tuning each model. In retrospect, we agree with the referee that this is the comparison we should have presented (rather than using a common tuning). We have revised the text and figure to clarify the goal of the exercise, and reflect the use of separate tunings.

The firn diffusivity profiles inferred from each of these models are similar. We show them in the attached figure (fig. R1). The differences between the two tuned profiles are small compared to the difference between the untuned profile (based on the model of Schwander et al.) and either of the tuned ones. This further strengthens our conclusion that uncertainties in firn diffusivity are large compared to advective fluxes. While it would have been nice to use a single model with advection turned on and off, this was not possible without a major effort. We have no advective flux at all encoded in our basic model (Battle et al. 1996), while advection is built in to the moving coordinate model (Trudinger et al. 1997) and cannot be turned off.

2) We have now included more of the details of the tuning process and the firn models. However, we have not greatly expanded our description of our moving-coordinate model. There are two reasons for this. First, this paper is not the first time our moving coordinate model has been used for published work (see Mischler et al., GBC 2009,

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Montzka et al., GRL 2010, Aydin et al., ACP 2010). Secondly, the model is fundamentally the same as the Battle et al., 1996 model. The only modifications are the implementation of moving coordinates (following very closely the Trudinger et al., 1997 work) and the addition of exponentially damped eddy diffusivity in the convective zone (following very closely Kawamura et al., 2006). There really isn't much more to say about it.

Minor issues:

1) Done

2) Done

3) We have added a cross-reference in section 6.3 as suggested.

4) We chose to leave off the intercepts in the figure legend for two reasons: First, the values, whatever they are, have no physical significance since they depend on the choice of reference standard for the O₂/N₂ scale. Second, there is not much room on the plot, and we didn't want it too cluttered. We have now compromised by continuing to omit them from the figure, but adding a few words about this to the caption.

5) The reviewer is correct that, in principle, the data from WAIS-D should be informative about the activation energy. However, the number of uncertainties and potential dependences (gas species, diffusion mechanism, temperature and perhaps others) exceeds the number of constraints the WAIS-D dataset offers. Our plan is to address this question in a subsequent paper, bringing the power of multiple sampling sites and datasets to bear on the problem.

6) The reviewer is correct that the effect we observe here does indeed have potential implications for ice core O₁₈-O₂ reconstructions. That is the essence of our sentence in section 9.2 beginning "If corroborated...". We have chosen not to explore these implications further for two reasons: First, our result, while clear and unequivocal, is uncorroborated. Observing the phenomenon at another site would add a great deal

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to our confidence in the general applicability of this result. Second, we remain largely ignorant of the mechanism behind the observed fractionation. The possibility exists that the apparent size of the fractionation is related to the extraction of samples. Thus, working backwards to infer the "true" isotopic composition of the air left behind in bubbles is particularly perilous.

7) We have tried to improve the clarity of this paragraph and hope the new version stands on its own. In addition, here is a more informal restatement of our argument: We assert that there is little error in delta age if one assumes all bubble closure occurs in the lock-in zone. This follows from two facts: a. Mean age of firn air increases faster as depth in the diffusive column increases (the non-linearity shown in the Fig. 8 inset). This means that even if all bubble closure occurred uniformly throughout the diffusive column, the bubble air would have an age corresponding to a point well below the mid-point of the diffusive column. In other words, some shallow bubble closure still gives a mean age of bubble-air close to that of the lock-in zone. b. Del15N of firn air increases linearly with depth in the diffusive column (due to gravitational enrichment). Thus, compared to mean age, del15N is sensitive to uphole bubble closure.

These two points tell us that if there is significant uphole bubble closure, it will have a stronger impact on del15N than on the age difference between bubble air and lock-in firn air. Thus, because the del15N of the bubbles is not significantly different from the lock-in firn air, we conclude there is not enough uphole bubble closure to impact the mean age. From this, it follows that estimates of deltaAge made with the assumption that all bubble closure occurs within the lock-in zone will have negligible bias.

Technical Corrections: All done. Thanks!

Referee #2

We thank the referee for careful reading of the manuscript, thoughtful criticisms and helpful suggestions.

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Major comments:

1) We agree that the details of the model tuning process merit further discussion. We have expanded our presentation of this in Section 5.

2) Referee #1 raised many of the same concerns and we agree with both referees that our presentation in Section 7 of the paper was inadequate.

The exercise presented in Section 7 was intended to determine whether the absence of advection in a model could be mitigated with tuning. Naturally, the answer to the question will depend, in part, on the accumulation rate at the site.

We agree with the Referee that the best comparison would have been with a single model, run with and without advection, with nothing else changed. Unfortunately, this is not possible for us without a great deal of extra work. This is because we have no advective flux at all encoded in our basic model (Battle et al. 1996), while advection is built in to the moving coordinate model (Trudinger et al. 1997) and cannot be turned off. Thus, we are forced to run two different models. That said, the models are actually very similar (this is the reason we chose this particular pair). The moving coordinate model had the same underlying numerical scheme for diffusive fluxes, and differed only in box spacing (equal mass vs. equal height), coordinate system (moving vs. static), and parameterization of the convective zone (irrelevant in this context). We addressed our question in several ways, including runs of each model using the same tuning (presented in our ACPD paper) and runs of the models, each with its own optimized tuning. In retrospect, we agree with Referee #1 that the latter comparison is the one that best addresses our question. We have revised the text and figure to clarify the goal of the exercise, and reflect the use of separate tunings. Figure R1 (attached) shows how little adjustment to the diffusivity is needed to compensate for the absence of advection.

Minor comments:

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Comment 1: Done

Comment 2: Yes, equal-mass layers of ice. Added to text.

Comment 3: We agree that graphical display of this information is a good idea, but rather than adding a new figure, we have added a pair of linear fits to Figure 2 showing the two simplest methods used to estimate the depth of the convective zone. We think the figure remains reasonably clear, despite the additional information.

Comment 4: Yes, "from" is what it should be. Corrected.

Comment 5: The models do not include changes in bubble gas content due to permeation. We have added this to the text.

Comment 6: We have not done careful quantitative modeling with all of our measured species to assess the impact of uphole bubble closure on DeltaAge. However, we are reasonably sure that the most informative tracer for this purpose is indeed $\delta^{15}\text{N}$. This species has two particular advantages: On the timescales of interest here, we know the atmospheric history exactly, and we can measure it in both firn and ice with great precision. While the large changes in the diffusive column for some other species (such as CO_2 , F11 and other halocarbons) make them initially attractive, the limited precision of the atmospheric histories and firn (or ice) measurements preclude this kind of analysis.

Typos: Fixed. Thanks!

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

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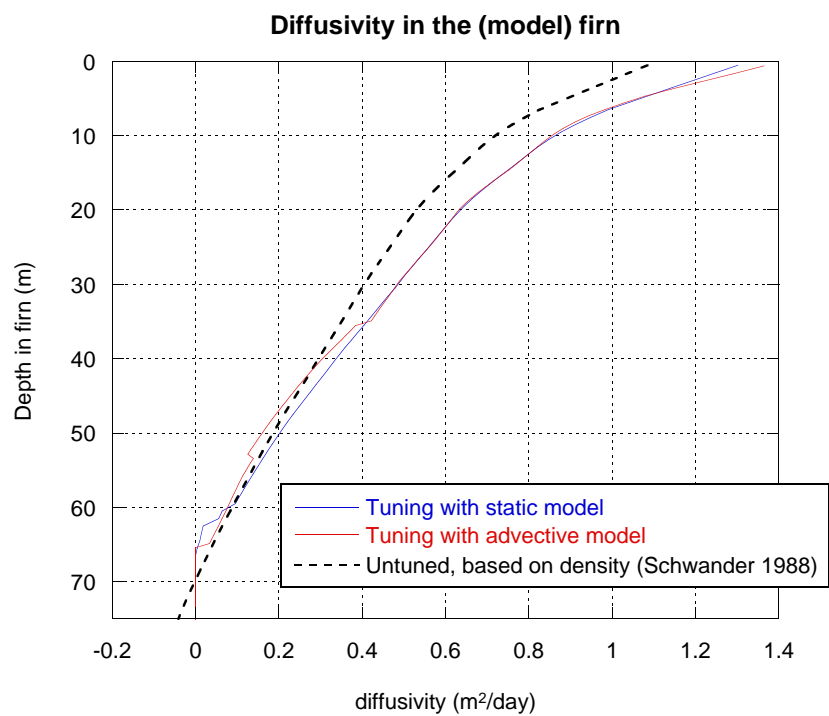


Fig. 1. Firn diffusivity at WAIS-D as inferred from models with and without advection.