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Atmospheric Chemistry and Physics Discussions

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

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The authors would like to thank the referee for his / her careful reading of the manuscript and for the thoughtful comments. In our response below we omit those referee comments and suggestions which can be (and will be) directly implement in a revised manuscript.

RC: *"In the main paper, my general concerns relate to the sections 4.2.2-4.2.4 and 5. In particular for section 4.2 (except for 4.2.1), the motivation for the choice of these scenarios is not sufficiently clear. For example, what is the relevance of a 15-year*

oscillation in the atmospheric input?"

The diagnostic scenarios are not meant to be realistic or have a real-world analog; they are meant to probe the model physics. The 15-year oscillation is included to test diffusive attenuation of atmospheric signals with depth, and represent this in a way that is intuitive. The period of oscillations is chosen sufficiently slow, such that the fictitious atmospheric variations propagate to the deep firn (too fast oscillations being filtered out), while still probing the models capability to reflect transient atmospheric phenomena.

If one performs a spectral analysis of the variations in tropospheric composition, there will be some power at the 15 year period. This scenario tells us how much this atmospheric component is attenuated in the firn before being recorded in the ice.

RC: *"Section 5 presents in principle an adequate discussion of the preceding chapters, but in my opinion the paper would gain a lot from combining the discussion with the results section. At several places in section 3 and 4, it is unsatisfactory to read about results, where the interpretation is not straightforward, but interesting, without discussing these results immediately and in relation to what has been found in the earlier chapters (e.g. the differences between the models, or the importance of the various physical processes). In chapter 5, it all falls in place, but I had already forgotten what the results from the previous chapters were, and had to go back and forth between chapter 5 and various places where the results are presented."*

Agreed. The discussion (Ch.5) mostly focuses on the synthetic scenarios. We will combine Ch4 and Ch5, and discuss the scenarios in the sections where they are also introduced.

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RC: *"The supplementary material is in several parts useful and enlightening, but excessive in length, which in my opinion limits its usefulness. I am not convinced that relating it section-by-section to the main paper is the best choice. This leads to quite some repetition in some parts. I would suggest independent sections that really give essential additional information in the supplementary material."*

Especially because of the length of the supplement we prefer to have it reflect the structure of the main work, and keep the cross-referencing. The supplement should not be read in its entirety; it contains more in-depth information for those who are interested in the technicalities. However, we do realize the length is excessive, and we will work to shorten it.

RC: *"Section 3.1.2 is very insightful again. The model description in section 3.2 is excessive again. I suggest to shorten it to ∼1 page (at most 2) in total. The details given on the individual models are not in relation to the processes that are discussed in the paper and therefore not necessary for the understanding of the present paper. If the CIC model has not yet been described in the literature, his it should be done in a separate paper. For all models, the description should be limited to aspects that are relevant for this paper. If this is done, then the main paper can also refer to the supplemental material when discussing effects (for example differences between models) for which the underlying reasons are then explained in the supplementary material. I so not see this link for many of the detailed description of the models, whereas it is clear at other places."*

The work introduces 3 new firn air models that are not described elsewhere in literature (CIC, INSTAAR and OSU). There are no plans to publish separate papers on the model descriptions, as the models do not contain much new science. Since we show and discuss output of those models, a (somewhat) detailed model description should

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be available somewhere in literature. The supplement is the logical place.

RC: *"I expect that the authors have considered the issue of the very long supplementary material before, but I hope that these recommendations are useful in making a more concise supplementary material."*

The current version has already been shorted from earlier drafts. We will work to shorten it further.

RC: *"992 and elsewhere in document References to sub-figures are with small letters but in the figures they are with capitals."*

This occurred during typesetting. We will convert to small letters throughout the work.

RC: *"993 line 29: Why is the error applied symmetrically? If the air velocity is smaller than the ice velocity (p 15988), then the age in the closed porosity can only be younger than in the open porosity. Also, the errors in the values as mentioned above should be included"*

The lower bound on Δage is set by the combination of model uncertainty and the possibility pore closure above the LIZ, the higher bound by model uncertainty alone. The former one is applied symmetrically in the manuscript. We will give the asymmetrical uncertainty estimates in a revised manuscript.

RC: *"995 line 16: What is the motivation for this scenario? It is not likely an atmospheric scenario"*

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See comments above.

RC: *"996 line 2: Does this mean that the two scenarios are applied simultaneously? Otherwise, why is the signal amplitude derived from a combination of the two scenarios? And what does this combined amplitude tell you?"*

Both scenarios are run separately, and the model outcome is combined in a single profile as given by Eq. 4. The signal amplitude cannot be derived from a single scenario because the sine wave has nodes, where all amplitude information is lost. By combining a sine and a cosine the amplitude can be reconstructed at all depths. Alternatively a single scenario of the form $C = \exp(2i\pi t/15)$ can be used, which involves using (unphysical) imaginary and negative concentrations, which some of the models might not accept.

RC: *"001 line 16: That should have been clear form the beginning, but you have investigated what the effects are."*

Very true, we will rephrase the sentence. Referee 2 made a similar comment.

RC: *"003 line 4 and for all references: What are the numbers behind the references?"*

They give the page numbers where the references occur in the text as hyperlinks (standard ACPD typesetting).

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Supplementary material

RC: *"11, section 2.7.7: Shouldn't the samples where you only have data from the EU hole get the same additional error?"*

The mysterious offset was only observed for SF_6, and not for CH_4, CO_2, N_2O, CO, H_2, and $\delta^{15}N_2$ where we have data from both boreholes. It does not seem to be related contamination with modern air or with the sampling procedure. We have no reason to assume the other tracers are affected. Also, the EU SF_6 results are very consistent with the other tracers, and it appears the mysterious anomalous mixing ratios occur only for the US flasks. We cannot prove this without making any prior assumptions, so we apply the additional SF_6 errorbar indiscriminately to both boreholes.

RC: *"Before eq 17: please specify what exactly a non-isotopic species is in relation to Y (which is a non-isotopically substituted species, I mixed it up first). I think you mean the sum of all isotopologues."*

When calculating the mass of the "non-isotopic species", all atoms constituting the molecule have their average atomic mass (i.e. the sum of the masses of all isotopes weighted by their abundance). It is the same as the summing up all isotopologues weighted by their abundance (only easier to calculate, since there are many more isotopologues to account for). We will clarify this in revision.

RC: *"16, lines below eq 20: I think this is not precisely what the ^{17}O correction does. It corrects the mass 45 abundance for ^{17}O, so that ^{13}C can be calculated. But it does not account for all isotopologues, it actually assumes that they are randomly distributed."*

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What we did when calculating the diffusion coefficients is a weighted average of all isotopologues that contribute to the analysis. This is indeed not the same as the ^{17}O correction. The reference we used is irrelevant here and will be removed.

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