

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

Anonymous Referee #1

Received and published: 9 July 2011

This is a thoroughly written paper that presents a multi-tracer firn modeling framework for the NEEM site. Furthermore, it summarizes the present state of art of forward firn modeling and presents a comparison between 6 available models in the group of authors. Overall, the paper is clearly written, and the science is sound.

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?

Section 5 presents in principle an adequate discussion of the preceding chapters, but

C6140

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.

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.

For example:

Section 2.1. and 2.2. are quite narrative and do not give really critical additional information.

Not all the details in section 2.3 are necessary, either.

Section 2.4 and 2.7 provide important information regarding the new multi-tracer approach and could be a main section of the supplemental material

Section 3.1.1 is a review of existing diffusivity data and in my opinion not necessary for this paper.

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, this it should be done in a separate

C6141

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 do not see this link for many of the detailed description of the models, whereas it is clear at other places.

Section 4 should stay focused on showing the results in the main paper for the US hole, but with short additional discussion only.

In summary, this would result in sections on

- Atmospheric scenarios and assignment of errors for the minimization routine.
- Diffusion coefficients (concentrating on the relative diffusivities)
- A shorter description of the models focusing on the points that are relevant for this paper
- The results for the US hole

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.

Specific suggestions (only the last three digits of the page numbers are given, and line numbers on these pages):

Main manuscript:

977

6: consider adding ...reference gas WITH KNOWN ATMOSPHERIC HISTORY

978

7: isotopologues

C6142

17: reference missing

982

10: please add "where s_{CO} is the close-off porosity, or similar

985

23: addensures THAT

986

13: ...explanations THAT we considered

987

5: write out time scale

992 and elsewhere in document

References to sub-figures are with small letters but in the figures they are with capitals.

993

21: better: To calculate....

25: please give uncertainties for Dage

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

995

16: What is the motivation for this scenario? It is not likely an atmospheric scenario

996

2: Does this mean that the two scenarios are applied simultaneously? Otherwise, why

C6143

is the signal amplitude derived from a combination of the two scenarios? And what does this combined amplitude tell you?

11: 60 years and 4 periods

13: please consider interpretation of this scenario right away instead of in section 5. What does this part of the model intercomparison tell us?

997

1: Again, more discussion here would be very helpful. Do you understand WHY the models behave so differently based on the analysis of the different terms above?

15: Maybe mention (and discuss again?) large difference of CSIRO model.

999

14: isotopologues

001

16: That should have been clear from the beginning, but you have investigated what the effects are.

003

4 and for all references: What are the numbers behind the references?

Supplementary material

Introduction may not be necessary if the added value of the SM is made clear.

4

table 4: I suggest including the values for A_{abs} also in this table.

11, section 2.2.7: Shouldn't the samples where you only have data from the EU hole get the same additional error?

C6144

14

after eq 14: The first sentence should be moved to where you first really use X and Y. I think this is in eq 17 ff

5 lines below eq 15: subscripts

15

Table 12: In this table, please be consistent as regards the atoms without mass designation. E.g. for $^{12}C^{18}O^{16}O$ I assume that the last O stands for ^{16}O , in the line above it includes the other masses (as indicated with the asterisks). You could make this more simple to the reader if you write $^{12}C^{18}O^{16}O$ where necessary (e.g. where N atoms are involved or for O_2).

Third footnote: rather cryptic remark, please be specific $^{14}COO/^{12}COO$, correct? Col 2, 1: which fact

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.

16

5 lines below eq 20: replace impractical by impossible

same sentence: 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.

Section 3.2: This section is far too long, see general comment above. The details given on the individual models are not in relation to the processes that are discussed in the paper. This applies to almost all of the individual model descriptions. In particular, 3.2.1 is a full description of the CIC model, which should be removed and published separately. This review does not include the equations of the CIC model.

C6145

17

above eq 30: strange character

sentence below 33: consider rephrasing “fallen out of”

18

-9: comma after zero

19

unit for J_{eddy} in (41) seems OK, but written in a weird form: $m^3m^{-2}=m$

24

Eq 52: add permil sign or remove factor 1000

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

C6146