

## ***Interactive comment on “Modelling the firn thickness evolution during the last deglaciation: constrains on sensitivity to temperature and impurities” by Camille Bréant et al.***

**Anonymous Referee #1**

Received and published: 9 November 2016

Bréant et al. address an important outstanding problem in ice core research, namely the model-data mismatch of  $\delta^{15}\text{N-N}_2$  as a proxy for firn thickness during the last deglaciation. They offer an interesting new solution to this problem, by proposing a temperature-dependent effective activation energy for firn sintering. In their framework, this can be understood as the effect of three separate firn densification mechanisms working in parallel, each with its own activation energy. Their modified firn densification model provides an improved fit to the deglacial  $\delta^{15}\text{N}$  evolution at cold interior sites, while still being able to fit relatively warm sites that were already modeled well by existing models.

I would ask the authors to consider the following points in a revised manuscript:

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- The  $\delta^{15}\text{N}$  model-data mismatch has a long history in ice core research, and is described most clearly by Landais et al. 2006. Several solutions have been proposed for this problem. Without explicitly stating so, the present manuscript takes as the starting assumption that the temperature sensitivity of the densification model must be the problem, due to the absence of a modern analog. I'll refer to this as the “no-analog solution” to the LGM  $\delta^{15}\text{N}$  problem.

I think it would be important to introduce the LGM  $\delta^{15}\text{N}$  problem better, and outline some of the other proposed solutions. For example, Landais et al. (2006) concluded that reconstruction of past accumulation rates was the most likely solution. Why was that explanation abandoned in favor of the no-analog explanation?

It is unclear to me what the main objective is of the present paper. Is the purpose to simply test whether the LGM  $\delta^{15}\text{N}$  problem can be solved using a different activation energy scheme? Or is the purpose to present a new model that will replace the Goujon model in future research at LGGE? Both models fit present-day data equally well, so whether the new model is an improvement relies solely on whether you believe the no-analog solution to be the correct one.

Finally, did they solve the problem? From the conclusion section it is not exactly clear whether or not the no-analog and dust mechanisms fully solve the LGM  $\delta^{15}\text{N}$  problem. It seems like the dust mechanism is insufficient by itself, given that it makes sites the fit to sites like GISP2, NGRIP and WAIS Divide worse. The no-analog solution seems to do a better job, yet it requires an unknown process with very low activation energy (see below). Moreover, EDML remains confusing to me. It's warm enough during the LGM to have modern analog sites, yet it does show the  $\delta^{15}\text{N}$  model-data mismatch in traditional firn models. I would appreciate some added discussion on whether the LGM  $\delta^{15}\text{N}$  problem has now been solved satisfactorily, and whether we can forget about other proposed solutions.

- To get the densification rate to increase meaningfully at low temperatures, the authors have to introduce a densification process with an extremely low activation

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energy of  $Q_3=1.5$  kJ/mol (low enough to be essentially temperature-insensitive). They suggest this process to be surface diffusion. However, experimental studies suggest the activation energy for ice surface diffusion is on the order of 14 to 38 kJ/mol (e.g. Jung et al., doi: 10.1063/1.1770518, Nie et al., doi: 10.1103/PhysRevLett.102.136101, and references therein). The value used by Bréant et al. seems an order of magnitude too small to be surface diffusion. Therefore, they are essentially invoking densification by an unknown process with very small  $Q$ .

The authors should acknowledge that the values they use for  $Q_3$  seems unrealistically low. In my view, this is an important piece of evidence that the “no-analog assumption” by itself may be insufficient to solve the LGM  $\delta^{15}\text{N}$  problem – the authors may not share this view.

At the other end, their high- $Q$  process (suggested to be vapor diffusion) has a value that seems too high at  $Q_1=110$  kJ/mol. Vapor diffusion scales with the vapor pressure, and the enthalpy of sublimation in ice is only 51 kJ/mol.

- Ultimately the goal of firn modelling is to predict  $\Delta\text{age}$ , and therefore I was surprised that no  $\Delta\text{age}$  results are shown. How does the new activation energy scheme change the simulated  $\Delta\text{age}$ ? In Greenland we have direct constraints on  $\Delta\text{age}$  via the thermal  $\delta^{15}\text{N}$  signals. How well does the model capture those?

Likewise, in evaluating the model performance on page 18, the authors test only how well the model predicts the LID (i.e.  $\delta^{15}\text{N}$ ). The more important metric, in my mind, is how well the model predicts  $\Delta\text{age}$ . This can be evaluated via the integrated density from the surface to the LID (because  $\Delta\text{age}$  is essentially the mass of overlying snow divided by the accumulation rate).

- On lines 624-625 the authors conclude that uncertainties in the climate forcing cannot explain the LGM  $\delta^{15}\text{N}$  problem. However, to solve the  $\delta^{15}\text{N}$  problem one would need to make the LGM warmer, not colder! For some reason the temperature uncertainties in Fig. S9 are applied very asymmetrically, such that the LGM

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is always very cold.

- It is not very clear how the parameters in Table 1 were selected. How was the model calibrated? Did the authors minimize some cost function? The authors do give three representative examples in Table 3, but no criteria for choosing the best model.

In their preferred model (Table 1), process 1 ( $Q_1 = 110$  kJ/mol) doesn't do much. At all relevant temperatures, process 2 is at least an order of magnitude larger.

- The authors claim that the new model provides a better fit to modern data than the old model. The LID prediction improves by  $1.2 \pm 6$  meter. That hardly seems like a statistically significant improvement. Using as Student's t-test it should be trivial to show whether the null hypothesis (both models perform equally well) can be rejected.

As mentioned earlier, I would encourage the authors to not only compare the predicted LID to the fitted data, but also to compare the integrated density in the simulations to the integrated density of the fit. The latter metric is more representative of  $\Delta\text{age}$ .

- The closest analog to LGM conditions in East Antarctica is the Dome A site, with mean annual temperatures below  $58^\circ$  C. The old LGGE model provided a reasonable fit to density at this site (Cunde et al., 2008). Why is this site not included in the calibration data set?
- The MS does not give many technical details about the running of the models. What time and spatial step size are used? What is the lower model boundary? What geothermal heat flux is used? The latter is important in the stagnant firn columns of the LGM.
- The authors test the dust softening hypothesis of Johannes Freitag and Maria Hörhold. It is important to note that this model of Freitag et al (2013) was de-

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signed to simulate layering, rather than bulk density as it is used here. How were Ca data averaged in the model runs? How does the Ca data resolution compare to the model resolution?

Please discuss some of the caveats regarding dust. From talking to Johannes Freitag, I get the impression he believes layering to be more relevant than bulk density in this regard.

What do the authors recommend? Should future users incorporate dust or not?

- I do not understand the rationale behind the LID parameterization of Equation 10. The authors use a very complicated way to define the LID, namely the depth where the modelled  $\delta^{15}\text{N}$  in the open pores matches the  $\delta^{15}\text{N}$  in the mature ice. This approach involves simulating the bubble trapping process, which is very (!) poorly understood. The depth range of bubble trapping is completely unknown at most sites, unless measurements of closed porosity are available (which is not the case at most sites). Also, trapping depends strongly on density layering, as early work at Law Dome and more recent work by Rachael Rhodes and others have shown.

The lock-in depth is very clearly visible in firn air sampling data, as an abrupt change in the concentration slope of many tracers. Why not use this commonly-used and simple metric, which can be directly derived from data? I fear that modelling something as complicated as bubble trapping could easily lead to errors. How does the fit of Figure S5 look when using the common definition of the lock-in depth?

- One of the important achievements of this work is to compile a large database of reliable firn density measurements. This would be an extremely valuable resource to the firn research community if it were publicly available in a format that is easy to use. I would like to kindly ask the authors to make this database publicly available as a supplement to the manuscript, as is also strongly encouraged

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by the data policy of the journal (Climate of the Past).

The manuscript does not have a statement of data availability yet, which will need to be added as per the editorial guidelines of CP.

- throughout the paper the authors refer to “snow” as everything above the critical density, and “firn” as everything below. This is not common usage, and should be specified.

Some minor comments and typos:

L2: “constrains” should be “constraints”

L23: “to” should be “on”

L24: “existence”. Maybe “dominance” would be better?

L35: “depict”. What about “reconstruct”?

L61: The close relationship between A and T seems to be mostly an assumption. See e.g. Monnin (2004), Fudge (2016) or Van Ommen (2004).

L65: in HL the “change” in pore space is proportional to the increase in weight.

L89: Better write: “In the absence of thermal gradients”, the  $\delta^{15}\text{N}$  trapped . . . . The geothermal heat flux matters also

L101-102. Thermal fractionation does not only occur in Greenland, but anytime there is a thermal gradient.

L105: what is  $\Omega$

L106: What does this statement mean? Can thermal fractionation not exceed 0.15 permil? Or is this the maximum observed value?

Section 2: It may be more useful to identify the different stages of densification via their density range, rather than their depth range.

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L158-160: likely all three stages are blurred in reality, with the densification mechanisms overlapping, see e.g. Hörhold et al. (2011).

L213: remove “s” in “equals”

L236: I thought that  $\gamma$  was just an ad-hoc scaling factor to make things fit. Does  $\gamma$  have a real meaning, and does it correspond to some physical process?

L248: Please add a multiplication signs ( $0.5 \times 10^9$ )

L295: note that seasonally sensitive densification rates (as from Arthern et al.) cannot be compared to mean annual densification rates.

L328: Note that Arthern does not attribute his high activation energies to vapor diffusion. Vapor diffusion should have an activation energy of 51 kJ/mol, i.e. the enthalpy of sublimation. Again, please be careful not to conflate activation energies of models that do and do not resolve the seasonal temperature signal.

L357-358: This assumption is probably not valid for vapor diffusion? Section 2.2.3: please specify at what resolution you allow Ca to vary. Do you smooth/average the records in some way?

L376-377: This is an odd definition of the clos-off depth. Isn't the pressure in (closed) bubbles is always higher than that of the atmosphere? What about: the density at which the total pore volume, at the atmospheric pressure of the site, equals the air content of mature ice. (or similar)

L395: polar firn study “sites”, . . .

L398 “trapping density” should be “lock-in density”

L403: where does the  $\ln(1/Ac)$  functional form come from? Is this inspired by theory?

L405: . . .better agreement “between” the modelled LID “and”  $\delta^{15}N$  . . .

L405: is the  $\delta^{15}N$  measured on firn samples or ice samples?

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L413-414: In fact, it makes it worse.

L458 and L490. The limits of the summation are listed the wrong way around;  $i = 1$  should be printed below  $N_{max}$

L490: why compare the model to the fit, and not just to the data as was done in Eq. 11? That way you compare apples to apples in Figure 5.

L493-494: is this improvement statistically significant? Please provide t-test significance or similar.

L505-506: I think this is somewhat of a uneven comparison, you should be comparing  $\sigma_{model-data}$  to  $\sigma_{fit-data}$ .  $\sigma_{model-fit}$  could in theory be smaller than  $\sigma_{fit-data}$ , as the data has some inherent scatter and layering.

L513-514: first stage is important in getting the correct  $\Delta age$ , though.

L541: Do you make a correction for the convective zone?

L544-546: Do you include the geothermal heat flux?

L552-553: How do you include the borehole calibration from Dahl-Jensen et al (1998)? This is not clear

L592-593: How well do the different models fit the exact timing of the  $d^{15}N$  increase? This is set by  $\Delta age$ , so an important test for the models.

L639-642: Again, I think the link between Accumulation and temperature is not as strong as you suggest here, particularly near the margins where most accumulation is delivered by storms.

L666: “densification rates” (add “s”)

L682: compatible with the data “except at Talos Dome”. Table 3: please be more clear what all the numbers are. I assume these are Q1, Q2, Q3 etc, but this is not very clear.

L706-710: Please rewrite this sentence, it is really hard to follow.

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L725: change “less good” to “worse”

L734-736: Could this be because the uncertainty in the input temperature does not include the possibility that the LGM was much warmer than the optimal scenario?

L756-757: Ca from volcanic events?

L817: Is your new modeled EDC  $\Delta$ age consistent with the work of Parrenin et al. (2013)?

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Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-91, 2016.