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

Answer to Anonymous Referee #1

Bréant et al. address an important outstanding problem in ice core research, namely the model-data mismatch of $\delta^{15}N-N_2$ 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 $\delta^{15}N$ 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:

• The $\delta^{15}N$ 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 $\delta^{15}N$ problem.
I think it would be important to introduce the LGM $\delta^{15}N$ 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 $\delta^{15}N$ 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 $\delta^{15}N$ 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 $\delta^{15}N$ model-data mismatch in traditional firn models. I would appreciate some added discussion on whether the LGM $\delta^{15}N$ problem has now been solved satisfactorily, and whether we can forget about other proposed solutions.

The purpose of this paper is to evaluate the LGGE model of firn densification by comparison with data in modern and past climate conditions, with a focus on the major problem of the too small densification rate during the glacial in East Antarctica highlighted in Capron et al. (2013) and earlier studies. We will also state more clearly in the motivation for this study (introduction) that the modelled lock-in depth and associated Δage were too large at Dome C / Vostok during the last glacial maximum with the old model.

Indeed, Landais et al. (2006) have suggested that the forcing scenario can explain the model-data mismatch for sites like Law Dome and Kohnen (still, they needed a convective zone of 10 m during the LGM at Kohnen to reconcile $\delta^{15}N$ and firn model run with the low estimate of accumulation rate) but it is not realistic to reconcile $\delta^{15}N$ and model outputs in lower accumulation rate sites like Vostok and Dome C. Such model – data discrepancy was largely discussed in Capron et al. (2013) and we wanted to go one step further.

We did not perform a major revision of the model but tested the effect of relatively simple model changes (e.g. the dependence of activation energy to temperature and dust tests that can easily be adapted to other models). We agree that if the new model provides some ideas to solve the model – data mismatch at low temperature through modification of the activation energy, the problem is clearly not solved and this will be stated clearly and in a coherent manner in both conclusion and abstract. In the revised manuscript, we will thus clarify the fact that the association of three activation energies with three precise physical mechanisms is not proved.

We will also introduce better the d15N problem referring to previous works (Landais et al., 2006; Capron et al., 2013; …) and summarizing the associated results and other options to solve the problem (e.g. convective zone, thermal effect).

In the conclusion and perspectives, we will also mention the possibility to improve the constraints on firn modeling through the use of cross-dating on new ice cores with high resolution signals as already used by Parrenin et al. (2012).

• To get the densification rate to increase meaningfully at low temperatures, the authors have to introduce a densification process with an extremely low activation energy of Q3=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 Q3 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 δ15N 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 Q1=110 kJ/mol. Vapor diffusion scales with the vapor pressure, and the enthalpy of sublimation in ice is only 51 kJ/mol.

We thank the Referee for providing references for the activation energy of ice surface diffusion. We will include a test of a 15 kJ/mol in replacement of our previous “test C” in the revised manuscript. Using 15 kJ/mol leads to intermediate results between the “old” and “new” model results.

Actually, the only way to reconcile our model with $\delta^{15}N$ data by invoking a change of activation energy with temperature is to have a negligible influence of temperature on the densification rate below -50°C. We agree that the values are surprisingly low for activation energy at low temperature in order to fit the $\delta^{15}N$ data. There is a clear limitation of our approach which is empirical, and cannot isolate mechanisms, but it calls for further lab and field studies. The effect of temperature could also be misrepresented in our model (and in other densification models) by other ways than the value of activation energy.

As written above, we will make clear that the association of our three activation energies with three precise physical mechanisms is not proved. Indeed, while several mechanisms have been highlighted for the densification of ice over several temperature ranges, there is no unambiguous attribution of a particular mechanism to a particular temperature. The determination of the activation energies for our model has an empirical basis as for previous studies. We will further emphasize this aspect in the revised version, especially when discussing Figure 2.

Despite some empirical approach, it should be noted that although not necessarily linked to vapor diffusion, high values of activation energy have indeed been measured at high temperatures: values of Q_1 derived from ice creep tests (Jacka and Li 1994, Morgan 1991) can be up to 170 kJ/mol. Note also that the exact value of Q_1 does not influence the result of our study since we are working at low temperature where the effect of Q_1 is minor.

Finally, note that the range of effective activation energies that we use (Q_{eq} see supplementary figure S4) and allows to correctly simulate the observed densification rates is much smaller than the range of the Q_1, Q_2, Q_3 values. Above -25°C, Q_{eq} ranges between 58 and 60 kJ/mol.

• Ultimately the goal of firn modelling is to predict Δage, and therefore I was surprised that no Δage results are shown. How does the new activation energy scheme change the simulated Δage? In Greenland we have direct constraints on Δage via the thermal δ15N 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. δ15N). The more important metric, in my mind, is how well the model predicts Δage. This can be evaluated via the integrated density from the surface to the LID (because Δage is essentially the mass of overlying snow divided by the accumulation rate).

The following table will be inserted in the supplement (together with supplementary table S1) for the comparison of Δage at the bottom of the firn for the different sites studied here. Even if the comparison based on Δage or on modeled density profiles does not lead to exactly the same conclusion for each site, the main features already observed for the comparison of the standard deviation between modeled and measured density profiles are also observed here. This is the case for the worsened agreement between model and data at Talos and Mizuho when using the new parameterization or the improved model vs data agreement at low temperature and low accumulation sites of the bottom of the table (B32, EDML, South Pole, Dôme C, Vostok, Dome A).

Δage	Data	Old	New	New + dust
Dye 3	78	**68.4**	61.8	-
DE08	35.5	**36.8**	31.3	-
km105	104.4	111.7	**106.4**	-
Site2	112.2	**109.2**	103.3	-
Siple Dome	329	287.9	**296.9**	284.9
D-47	152	**145.3**	141.7	-
Byrd	238.9	225	**226.9**	-
NEEM	209.4	187.4	**191.9**	187.4
Crete	156.1	**150**	145.7	-
km200	137.9	156.7	**152.6**	-
WAIS divide	225	206.5	206.5	**233.3**
North GRIP	248.4	226.2	**236.5**	224.5
GRIP	209.9	**205.7**	**205.7**	200
B29	270.3	251	**264.7**	252.9
Mizuho	483.7	**518.4**	557	-
Talos	554.1	**592.6**	637.7	656.4
B32	896.8	816.1	**889.9**	978.4
EDML	874.5	787.3	**852.9**	899.7
SP	1160.9	965.2	1002.8	**1068.7**
DC	2639.1	2473	2461	**2557**
Vostok	2814.3	2960.4	**2810.4**	2919.5
Dome A	2812.7	3024.8	**2764**	-

For paleo application, we think that it is better to compare model outputs to $\delta^{15}N$ profiles because we do not have direct indication of the Δage (it necessary depends on the timescale). The only way to compare Δage model output with data is actually on the abrupt warming recorded very clearly both in the $\delta^{18}O$ in the ice phase and $\delta^{15}N$ in the gas phase on the NGRIP ice core because we have an accurate associated timescale (GICC05). This is the reason why we will present a table in the supplement showing the Δage outputs for the different model parameterizations and a comparison with data estimates based on $\delta^{18}O$ and $\delta^{15}N$ records of the abrupt warming in the ice and in the gas phases. The agreement between data and model is slightly better when using the new version but the addition of dust leads to a strong deterioration as observed on the $\delta^{15}N$ profiles.

NGRIP	Δage (old version)	Δage (new version)	Δage (new version + dust)	DATA
Bolling /Allerod	870 years	920 years	740 years (gaz)	1040±100 years
End of Younger Dryas	760 years	820 years	640 years (gaz)	800±100 years

NB: the uncertainty on the Δage from the data is mainly linked to the resolution of the $\delta^{15}N$ signal.

• On lines 624-625 the authors conclude that uncertainties in the climate forcing cannot explain the LGM δ15N problem. However, to solve the δ15N 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 is always very cold.

The uncertainties for the LGM temperature estimate on the Antarctic plateau are given in Jouzel et al. (2003) as cited in the manuscript in discussion. In this study, the authors have gathered all available constraints for the amplitude of temperature change between the LGM and the Holocene mainly at the Vostok and Dome C sites. The main conclusion is that temperature uncertainty for the amplitude of the last deglaciation is estimated to -10% to +30% in Antarctica. The reason for such asymmetry is mainly linked to outputs of atmospheric general circulation models equipped with water isotopes. These models suggest that the present day spatial slope between $\delta^{18}O$ and temperature most probably underestimate the amplitude of the temperature change between glacial and interglacial period. We have followed this estimate of asymmetric uncertainty on the amplitude of temperature change during deglaciation and this is the reason why the scenario with warmer LGM is not very different from the control scenario. We do not have references suggesting a much warmer LGM for Antarctica than the classical estimate from water isotopes. More recent studies focusing on the validity of the isotopes vs temperature relationship in Antarctica have also suggested that this relationship can be applied with confidence for glacial temperature reconstruction (Cauquoin et al., 2015) why one should be cautious for past interglacial temperature reconstruction (Sime et al., 2009). Finally, a recent estimate of the deglacial temperature increase based on d15N measurements at WAIS (Cuffey et al., PNAS, 2016) led to a 11.3°C temperature increase over the last deglaciation (1°C warming to be attributed to change in elevation), larger than the temperature increase reconstructed in East Antarctica from water isotopes by 2-4°C. This last estimate is against not in favour of a “warm” LGM.

Still, it should be noted that the uncertainty of 20% on LGM accumulation rate on central sites as given by DATICE in the AICC2012 construction is probably overestimated. Indeed, deglaciation occurs around 500 m depth at Dome C, hence with small uncertainty on the thinning function. Dating tie points can thus constraint quite tightly the accumulation rate.
Summarizing, we believe that the uncertainties given in our manuscript correspond to up to date uncertainties on temperature and accumulation for the last glacial maximum and do not underestimate the possible range of temperature and accumulation rate values for the LGM.

We will complete our manuscript with some explanations given above.

• 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 (Q1 = 110 kJ/mol) doesn't do much.
At all relevant temperatures, process 2 is at least an order of magnitude larger.

The values for the prefactors a1, a2 and a3 have been chosen to best reproduce the $\delta^{15}N$ variations over deglaciation at Dome C or Vostok while keeping a good agreement (at least not deteriorating the model-data agreement obtained with the old LGGE model) for (1) the deglacial $\delta^{15}N$ variations at higher accumulation rate sites and (2) the density profiles for present-day firn. Hundreds of sensitivity tests have been performed using a strategy based on dichotomy to reduce the mismatch between modeled and measured $\delta^{15}N$ change over the last deglaciation at low accumulation rate sites. The constraint of keeping a correct agreement of model results with present day density profiles and for the last deglaciation at warm sites strongly reduces the possible choices of a_i and Q_i. This is illustrated by the small range of Qeq values on supplementary Figure S4.

This will be better explained in the new manuscript.

• 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 +- 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 Δage.

We agree that the new model is not significantly better than the old one for density fit as well as LID and Δage predictions. This will be written more clearly in the new manuscript. As explained above, our main aim is to preserve the good agreement (no deterioration) between modeled and measured firn density profile while improving the $\delta^{15}N$ model- data agreement over the last deglaciation on the East Antarctic plateau.

Δage calculation for present-day firn will be provided in the supplement.

• The closest analog to LGM conditions in East Antarctica is the Dome A site, with mean annual temperatures below 58°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?

Dome A will be included in our revised manuscript as Dr Cunde kindly provided us the necessary data. Recent estimates of the accumulation rate at Dome A lead to a value of 2.3 cm w.eq. / yr (close to the values at Vostok and Dome C). With this accumulation rate, the model – data agreement is improved in the "new" simulations (see e.g. Δage values in the above table).

• 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 model spatial grids and lower boundary condition were not changed compared to the former model (Goujon et al. (2003), and the time step (site dependent) was chosen to lead to stable results. A section will be added in the Supplement to complete the description of the model running conditions.

• 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 designed 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?

Our main intention is to show that the Freitag et al. (2013) parameterization leads to interesting results and that the dust effect deserves further attention in future studies. When transposed to our model, it provides a reasonable fit of the data at all modern sites. Then, we acknowledge that the implementation of Ca effect in addition to the effect of temperature influence on the activation energy Q is not optimal since the parameterization of Q1, Q2, Q3 and associated a1, a2, a3 has been adjusted without Ca effect implementation. The implementation of Ca hence necessarily slightly deteriorates the model – data agreement.

As for the layering and its effect on the LID, it cannot be simply implemented in our firn densification model. This is the reason why the dust has been implemented through its influence on the bulk density which is indeed different from the simulations of Freitag et al. (2013). We will clarify the fact that we do not expect the Freitag et al. (2013) parameterization to be properly tuned for simulating the variations of firn thickness or Δage as it was not designed for this purpose.

We expect that the dust influence on bulk density or individual layer density is quite similar. Indeed, from a physics point of view, the density of a given layer is not directly dependent of the density of the surrounding layers as the load pressure on a layer is only controlled by the weight of the layers above (which depends on the precipitation rate but not their density). Thus the impurity effects on the bulk density and on individual layers density should be similar. This probably explains why the comparison is rather good between the polynomial fit of the measured density profiles (hence excluding layering) and modeled firn density profiles with dust influences according to Freitag et al. (2013).

For modern simulations, a single average calcium concentration is used (the value in Table S1). In past climate simulations, the calcium concentration data used were directly taken from the source data file available for each ice core. The temporal variations in Calcium concentrations are then simply interpolated at each model time step. The details on the temporal resolution will be given in the supplement together with the parameters used to run the model. The caveats regarding dust (e.g. using calcium as a diagnostics of dust content and the intention of the Freitag et al. (2013) parameterization) will be better introduced.

• 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 δ15N in the open pores matches the δ15N 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.

We agree with the Referee on the fact that gas trapping is insufficiently understood. However, the same uncertainties apply when using $\delta^{15}N$ measurements in ice (after this trapping) in past climate conditions to evaluate our model results. The intention of our definition referring to $\delta^{15}N$ in the mature ice is to get closer to what is used for past climate conditions. Systematic measurements of $\delta^{15}N$ in recent ice would be very helpful in the future to improve an LID definition relevant for ice-core interpretation. This will be mentioned in the revised manuscript.

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?

Different thresholds based on trace gas data in the open porosity of the firn were tested in Table 2 of Witrant et al. (2012). Unfortunately, different definitions (based on greenhouse gas concentration slope change, the $\delta^{15}N$ plateau, etc.) lead to fairly large differences especially at the most arid sites of the central Antarctic plateau which are a major focus of this study. In particular, as no $\delta^{15}N$ plateau is observed at these sites, the progressive bubble trapping should lead to less fractionated $\delta^{15}N$ values in mature ice than in deep firn at these sites. Only fairly small density variations are allowed by our LID parameterization. They will be further discussed in the revised manuscript. Our major conclusion is that the LID definition does not explain the mismatch between model results and $\delta^{15}N$ data during deglaciations.

• 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 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.

We made our best to document the nature and sources of the density data that we used. We are not the owner of the data in most cases, this is why we will provide the polynomial fits of the data rather than the datasets.

• 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.

We use the same words as Goujon et al. (2003). This will be explained in the new manuscript.

Some minor comments and typos:

L2: "constrains" should be "constraints" ok

L23: "to" should be "on" ok

L24: "existence". Maybe "dominance" would be better? ok

L35: "depict". What about "reconstruct"? ok

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).

This is indeed true but we focus better on site from the Antarctic plateau where the link should be true at least qualitatively. Moreover, we expect a certain correlation between change in accumulation rate and change in temperature when considering long term averages (several thousands of years for the LGM) while a certain decoupling between accumulation and temperature is expected at short scale because of the different possible snow accumulation processes. This will be explained in the new manuscript.

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

Yes, this is the meaning of our sentence, accumulation rate being directly linked with weight.

L89: Better write: "In the absence of thermal gradients", the δ15N trapped : : :.. The geothermal heat flux matters also ok

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

This is true but in order to have a strong thermal fractionation, we need abrupt temperature changes at the surface. We have thus added "strong" before "thermal fractionation"

L105: what is Ω ?

This will be explained in the new manuscript (Grachev et Severinghaus, 2003)

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

This will be explained in the new manuscript

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

Yes, we are agree, this will be changed is the new manuscript

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

We agree with this comment, we will precise it in the new version.

L213: remove "s" in "equals" ok

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

Equations (4) and (5) in our manuscript show the relationship between γ used in Goujon et al., 2003 and the parameters in Alley, 1987 : $\gamma = (2/15)(\lambda/v)(R/r^2)$ where λ is the bond thickness, v the bond viscosity, R the grain radius and r the bond radius. Alley (1987) then then calculates and discusses the activation energy for viscosity. On the other hand Goujon et al. (2003) simply used an adjusted value of γ, as explained in our manuscript text following Equation 5. We replace γ in Equation 4 by $\gamma'\exp(-Q/RT)$ in Eq. 6 and evaluate that Q=-49.5 kJ/mol. As γ' is still adjusted in the model, using γ or $\gamma'\exp(-Q/RT)$ does not change the model results.

In the modified manuscript, we will remind the relationship between Y and the physical parameters in Alley (1987) at line 236.

L248: Please add a multiplication signs (0:5 _ 109) ok

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

The large values up to 100-130kJ/mol are originally from Morgan et al, 1991, and Jacka and Li 1994 and their values are not related to seasonal thermal gradients, which are not taken into account in this study. Also note that the equivalent effective activation energy in out model (Qeq) remains lower than 60 kJ/mol (see supplementary Figure S4 and answer to second general comment).

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.

We removed the attribution to a specific mechanism and simply note: "At warm temperature, empirical determinations of Q1 lead to values of the order of 100-130 kJ/mol (Arthern et al., 2010; Barnes et al., 1971; Zwally and Li, 2002)."

These high values are actually not linked to a seasonal temperature signal : Jacka and Li (1994) made isothermal laboratory measurements to determine them.

L357-358: This assumption is probably not valid for vapor diffusion?

This is indeed correct. We will specify it. It should still be noted that this does not have any significant influence for the relatively cold sites studied here.

Section 2.2.3:
please specify at what resolution you allow Ca to vary. Do you smooth/average the records in some way?

No, the records are not smoothed (cf answer on dust data above). This will be specified in the new manuscript.

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)

We use the same definition of the close-off density as in Goujon et al. (2003). It defines the boundary between the second (firn) and third (ice) mechanical formulations in the model. As no model change was performed, the phrasing will be simplified in the revised manuscript.

L395: polar firn study "sites", : : : ok
L398 "trapping density" should be "lock-in density" ok
L403: where does the ln(1/Ac) functional form come from? Is this inspired by theory?

No, this is just an ad-hoc adjustment. In the previous model version (Goujon et al., 2003), the LID definition was related to a value of the closed/total porosity ratio adjusted for each drilling site but time independent (see lines 387-389). This dependency on the geographic location with independence from climate is not completely self-consistent. This is why we tested another LID definition.
The description of our LID definition and tests will be modified following the comments from both Referees.

L405: : : :better agreement "between" the modelled LID "and" δ15N : : : ok

L405: is the δ15N measured on <u>firn samples</u> or ice samples?

Yes, they are measured in firn samples, we will replace « measured at the available firn sampling sites » by « measured in firn samples at available sites »

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

L458 and L490. The limits of the summation are listed the wrong way around; i = 1 should be printed below Nmax ok

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.

There is no simple way to compare model results and density data in a multi-site consistent way due to the strong site to site differences in the data (measurement techniques, sample size, number of samples, depth dependent resolution, etc.). Model results are compared to the fit in order to better characterize:

- bulk density (as opposed to density variability) which we aim at predicting with the model
- site to site variations in the quality of the model prediction of the bulk density profile. The regular depth resolution used leads to an integrative diagnostics somewhat comparable to delta-age.

The manuscript section between lines 492 and 511 will be revised.

L493-494: is this improvement statistically significant? Please provide t-test significance or similar. No, it is not, , this will be mentioned in the revised manuscript

L505-506: I think this is somewhat of a uneven comparison, you should be comparing _model�data to _fit�data. _model�fit could in theory be smaller than _fit�data, as the data has some inherent scatter and layering.

We agree on the fact that the sentences at lines 505-507 are too much of a shortcut. On the other hand, the polynomial fit to the data is close to the bulk density profile that we aim at modelling. The manuscript section between lines 492 and 511 will be revised.

L513-514: first stage is important in getting the correct Δage, though.
A Table with Δage results (see above) will be included in the Supplement and briefly commented in the manuscript.

L541: Do you make a correction for the convective zone? yes, 2 m (L.580)

L544-546: Do you include the geothermal heat flux? Yes, the geothermal heat flux is included in the model and hence implemented for the calculation of $\delta^{15}N$ from the temperature profile.

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

The scenario for temperature evolution in NorthGRIP includes the borehole calibration from Dahl-Jensen et al., (1998) by imposing a 23°C temperature change between LGM and present-day. This corresponds to a temporal slope for the relationship between $\delta^{18}O$ and temperature of 0.3 permil.°C^{-1} over the deglaciation. This slope is thus applied for reconstructing the temperature evolution for the last deglaciation at NorthGRIP in this study. Then, the temperature amplitude for B/A and Y/D is adjusted on the $\delta^{15}N$ data obtained by Kindler et al. (2014) on the NorthGRIP core.

A paragraph explaining the temperature scenario will be added in the supplement.

L592-593: How well do the different models fit the exact timing of the d15N increase? This is set by Δage, so an important test for the models.

Indeed, the different models do not lead to the same calculated Δage as shown in the new table that we will insert in the supplementary materials. We will hence also comment this aspect on Figure 6.

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.

We agree that the link between accumulation and temperature is not always so strong and we will nuance this sentence in the revised manuscript. Indeed, near the margin, the link between accumulation and temperature is not straightforward as discussed for example at Law Dome in Landais et al. (QSR, 2006). However, here, we mainly deal with sites that are on the East Antarctic plateau and there is no simple way how we could explain a different evolution for temperature and accumulation rate at Dome C on the long timescale (i.e. on the difference between LGM and present-day).

L666: "densification rates" (add "s") ok

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. ok

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

When using the parameterization of Table 1 ("new model"), Figure 7 shows strong improvement of the simulation of the δ15N difference between EH and LGM. Indeed, the modeled EH-LGM difference now has the correct sign at very cold sites of East Antarctica (Figure 7) when compared to δ15N measurements.

L725: change "less good" to "worse" ok

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?

This is unfortunately not sufficient to explain the mismatch. Even if significantly higher LGM temperature (-6°C instead of -9°C) without any change in accumulation rate would decrease the $\delta^{15}N$ at LGM at EDML down to the level of the accu_minus scenario (Figure S9). This is unfortunately not enough to reconcile model and data on the amplitude of the change of $\delta^{15}N$ between the LGM and present-day. Moreover, note that such lack of correlation between temperature and accumulation rate changes can be observed in coastal sites but is again very unlikely in the East Antarctic plateau.

L756-757: Ca from volcanic events?

We do not know if the very fast peaks observed on the calcium concentrations correspond to volcanic eruptions, on the other hand we know that they do not affect the $\delta^{15}N$ profiles.

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

Yes, our study is in line with the work of Parrenin et al. (2012 and 2013). This will be specified in the new manuscript and was actually one of the aims of the tuning of activation energy values to temperature.