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 #2
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General:

The authors state that they have improved the LGGE firn densification model based on physical mechanisms. They argue with a better agreement between modelled and measured d15N data. But the physical arguments stand on shaky ground and the better agreement for some sites are opposed by significant lesser agreement of the model data at other sites. A general comment of caution regarding the physical approach: Though the concept of Arzt, based on monosized spheres, which deviates substantially from the physical reality, produces reasonable firn density profiles, they are not really better than those of empirical parametric models. The reason is the rigidity of the 'physical' model and it is not be surprising that an empirical approach with more free parameters, as e.g. in the Pimienta-model, may even better catch the reality!
The authors now introduce rather arbitrarily two additional Arrhenius-type mechanisms. Thereby they can readily simulate a higher densification rate at very low temperatures. A corroboration of the new model by the better agreement with a small glacial delta age (or delta depth, i.e. shallower glacial firn depth) by Parrenin (2012) is unjustified because this agreement was exactly the purpose of tuning the model.

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 at very low temperature in East Antarctica highlighted in Capron et al. (2013) and earlier studies. We did not perform a major revision of the model but tested the effect of relatively simple model changes (e.g. the activation energy and dust tests can easily be adapted to other models).

We agree that our parameterization has been chosen to better match the $\delta^{15}N$ data and hence the Δage estimate by Parrenin et al. (2012) at Dome C. This was already present in the previous manuscript and will be highlighted more clearly in the revised version. Our new parameterization should also not affect too much the agreement observed between model and data in higher temperature and accumulation rate conditions which is not trivial. We thus investigated to which extent the firnification model invoking different mechanism for firn densification is able to reproduce the $\delta^{15}N$ evolution over deglaciations in low accumulation rate sites of East Antarctica without degrading the good agreement observed with the old version of the model between measured and modeled firn density profiles and between modeled and measured $\delta^{15}N$ evolution over the last deglaciation at sites with higher accumulation rates.

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. Several reasons for this effect can be invoked. We proposed here an interpretation with different activation energies for firn densification based on previous studies showing different densification mechanisms over different temperatures. Still, our proposed interpretation is not definitive for explaining firn densification over a large range of temperature. In the revised manuscript, 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. We will also state that the effect of temperature could be misrepresented in our model by other ways than the value of activation energy opening the ways to other studies.

The approach regarding other transport mechanisms involved in sintering is not convincing. Why should surface diffusion explain the higher densification rates at low temperatures? First, surface diffusion itself does not lead to densification and second also indirect effects will most likely not favor densification. Surface diffusion increases neck diameters and thus decreases pressure at contact area, which decreases creep and in addition it increases curvature radii that decrease the generation of lattice vacancies, and thus decreasing lattice diffusion. Q3, the activation energy applied for surface diffusion seems unrealistically low. Higher values, between 30 and 50 kJ, have been reported.

We agree with the referee on the fact that surface diffusion does not directly lead to densification. We went back to Ashby 1974 (Acta Metallurgica, vol.22, pp.275-289) and actually the dominant mechanism at low temperature should rather be boundary diffusion from grain boundaries which is a mechanism enabling densification. We are not aware of activation energy values associated with this mechanism in firn. As stated above, we will make clear that the association of our three activation energies with three precise physical mechanisms is not proved.

As for the measured values of activation energy, Anonymous Referee#1 provided two references for the activation energy of surface diffusion in the range 14-38 kJ/mol and we will include in the revised manuscript a test showing that using 15kJ/mol leads to intermediate results between the "old" and "new" model results.

Considering the influence of dust on densification is interesting but does not substantially contribute to solving the discrepancy between model and date, because the densification enhancement by dust leads for too many sites to a deterioration of the modelled densities.

The tuning of Q1, Q2, Q3 and associated a1, a2, a3 has been done without dust influence. As a conclusion and as noted by Referee 2 (comment on lines 483+497), the implementation of dust necessarily slightly deteriorates the model – data agreement. This will be clarified in the revised version when discussing the dust influence addition.

We will also 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 as it was not designed for this purpose. One way to improve the constraints on the problem (how does dust influence the glacial firn depth ?) is to study other deglaciations, where the dust increase and the temperature increase are not synchronous.

The mentioned possibility of saturation of the densification enhancement by dust at high concentration would only work for Greenland but not for WAIS divide.

Indeed, the « dust saturation hypothesis » cannot reconcile the Holocene firn thickness at NGRIP and WAIS-Divide, which are nearly identical with 10 times more Ca at NGRIP compared to WDC. Another possibility is that the impact of dust depends on the densification mechanism, and is much more important at cold temperature.

My criticism shall not disesteem the huge work accomplished for improving the calibration of the model for modern firn densities that is also presented in this paper. This calibration with new improved firn density profiles certainly leads on average to slight optimization of the model parameters.
However, a better fit to glacial firn depths has only been achieved by a direct tuning of the creep factor at low temperatures. This should be clearly communicated as such.
And as the authors mention in the supplement this can be achieved with any other of the common densification models.

As mentioned in the previous manuscript, the choice of the very low value for Q3 is indeed designated for increasing firn densification at low temperature. This tuning of the creep factor will be stated more clearly earlier in the manuscript (section 2.2)

In my view, this lengthy paper clearly raises false expectations. The paper could be organized such that in a first part the improvement of the parameter calibration on modern density profiles are presented and in a second part one could investigate how the creep factor needs to be tuned in order to better simulate the glacial data for the different ice cores, with and without dust enhancement. The diverging results would readily show that so far no unified model can simulate the existing range of data.

>> We will implement a new (short) section 3.1 that present the agreement between modeled and measured firn density profiles using the revised version of the LGGE model with only one activation energy (i.e. corresponding to modifications depicted in section 2.1). To show the influence of this revisions compared to the old version, Figure 5 will display an additional vertical bar for each site.

Then, we will present our proposition that with a parameterization with three activation energies we are able to better fit $\delta^{15}N$ profiles at low accumulation sites in Antarctica. The limitations of this assumption presented at the beginning of the answer to Ref 2 will be clearly stated. We will then show that the implementation of 3 activation energies does not significantly deteriorate the agreement between modeled and measured firn density profiles but that some divergences persist for paleoclimatic simulations especially when adding the dust influence which call for future studies for investigating the influence of both temperature and dust on firn densification and lock-in depth prediction.

Some specific comments:
L. 2: title "constrains" -> constraints ok

L. 23: " we introduce a dependency of the activation energy to temperature and impurities in the firn densification rate calculation". It is rather a temperature dependent creep factor. The authors ’apply’ the impurity dependence, it was ’introduced’ by Freitag et al.

Yes, this will be rewritten in the manuscript.

L. 79: " questionable when used outside of its range of calibration". Not only then as in case of EDML.

>> It is true that EDML is difficult to reconcile with the model. We have shown in Landais et al. (2006) that it can be reconciled with the model only if accounting for large uncertainties in past accumulation rate and a reasonable convective zone (10 m) (Landais et al., 2006). This will be specified in the revised manuscript.

L. 186: Is A0 a constant? Value?
Yes, $A_0 = 7.89 \times 10^{-15}\ Pa^{-3}.s^{-1}$, this will be specified in the new manuscript. It is the same value as in Equation A5 of Goujon et al. (2003)

L. 210-228: This 'bending into shape' demonstrates again that a more parametric approach can be closer to reality than a 'pure physical' one.

True, but aiming to use physical formulation is perhaps safer when we venture out of the calibrated range. Still, our approach is very much empirical at this stage, since the creep constant and other model parameters such as D_0 have been calibrated empirically. We will make clearer the fact that our approach is not a 'pure physical' one: we will add a sentence at line 86 clarifying the fact that the simplified physical mechanisms in our model include adjusted parameters.

L. 245 (eq. 6) Is 0.1 bar the pressure at 2 m depth (L. 213)? This should be clarified in the text..

0.1 bar is a rough approximation of the pressure at two to three meters depth, which depends on the density profile of the overlying snow.
The sentence at lines 211-213 will be replaced with:
Indeed, since the model is not able to represent the metamorphism in the first two meters, we impose a constant pressure of 0.1 bar (see Equation 6), which is an approximation of the pressure at 2-3 meters depth. It results in a nearly constant densification rate in the top 2-3 meters rather than a constant density in the top 2 meters.

L. 305: " three different mechanisms highlighted above" There are more than 3 mechanisms mentioned above. Table 1, Fig. 2 may not be above.

>> Replaced by "three different mechanisms highlighted in Table 1 and Figure 2"

L. 321, 824: "surface lattice diffusion" Is this term correct for 'surface diffusion'?

>> Will be changed to boundary diffusion from grain boundary

L. 358: "assuming that the impurity effect is the same for all mechanisms". This seems very unlikely!

>> Ok

L. 362,3: "f1" subscript 1 ok

L. 403: "A subscript c" is a strange notation for accumulation rate. Define abbreviation in text. This parameterization is surprising. Of course temperature and accumulation rate are strongly correlated, so we expect an corresponding correlation with accumulation rate. But for two sites with equal accumulation rate but different temperature we don't expect the same LID as densification is strongly temperature dependent. So this parameterization may be justified for most conditions but one must be careful applying it in general and during glacial conditions.

"A subscript c" A is more commonly used but here, we wish to avoid confusion with A_i and a_i used in Equation 7 and Section 2.2. We will use "Ac" instead of "A_c" in the revised manuscript.
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 self-consistent. This is why we tested another LID definition. Note that we do not directly parameterize the lock-in depth but only allow moderate variations of the lock-in density. This will be clarified in the revised manuscript by using the notation "ρ_{LI}" instead of "ρ_{LID}" and concluding on the small impact of the definition change.

L. 427: Degree of polynomial fit?

It is site dependent primarily because the number of density measurements is highly site dependent. The polynomial fits used will be provided in the supplement of the revised manuscript.

L. 405,6: "This parameterization leads to a much better agreement of the modelled LID with d15N measured at the available firn sampling sites than when using the outputs of the old model" This has nothing do with the model. It is just a different parameterization. The better agreement would apply for the old model as well.

This will be corrected in the revised manuscript.

L. 456: "rough indicator of data quality" This seems a daring assumption, as natural variability might be in the same order of magnitude.

>> Completed as "rough indicator of data quality or estimate of natural variability"

L. 471: explain "traction constraint"

In the long term, mechanical strain conditions result in different crystal orientations in ice cores drilled near dome summits (where uniaxial compression along the vertical direction dominates) and along flow lines with a component of horizontal tension (see e.g. Montagnat et al., 2014, www.the-cryosphere.net/8/1129/2014/, and references therein). Moreover Vostok is located above a lake and the basal gliding results in a low shear stress, thus the crystal orientations likely results from a deformation in tension along the horizontal (Lipenkov et al., 1989). In the manuscript, we will replace “traction constraint” with “horizontal tension”.

L. 483 + 497: ?? " the original parameterization of Freitag et al. (2013) always remain in reasonable agreement with the data . . ... the incorporation of the impurity effects following the Freitag et al. (2013) parameterization in our model most often deteriorates the model-data agreement".

This is a good point, thanks. As some model tuning is performed prior to the incorporation of the impurity effect but not after, an overall slight but not significant degradation of the model results is obtained when adding the Freitag et al. (2013) parameterization. The inconsistency will be corrected in the revised manuscript.

L. 509: "This effect is due to a densification rate that is too high in the first stage, and this formulation is not affected by the new temperature sensitivity." If this is a general feature, why is it not accounted for correctly?

Figure 3 in Alley (1987) indicates that the modeled densification rates tend to be somewhat too low at low densities and somewhat too high at higher densities. This induces an “S” shape of the density profile that is also visible in our model results. Correcting this bias would require an in-depth revision of the Alley (1987) mechanism. As we say at lines 513-514, the first stage of densification is not crucial for predicting the LID: the modifications of the Alley (1987) mechanism did not improve significantly the model results. In the revised manuscript, Figure 5 will include results with the “new model” and a single value of activation energy. This will further illustrate the small impact of the modifications in the first stage of densification.

L. 613: "This observation questions the possible presence of a convective zone and/or .." Is the presence questioned or the constancy of the convective zone?

>> The **presence** of a significant convective zone is questioned for the LGM -> We will precise that it is clearly the presence of the convective zone at LGM that is questioned.

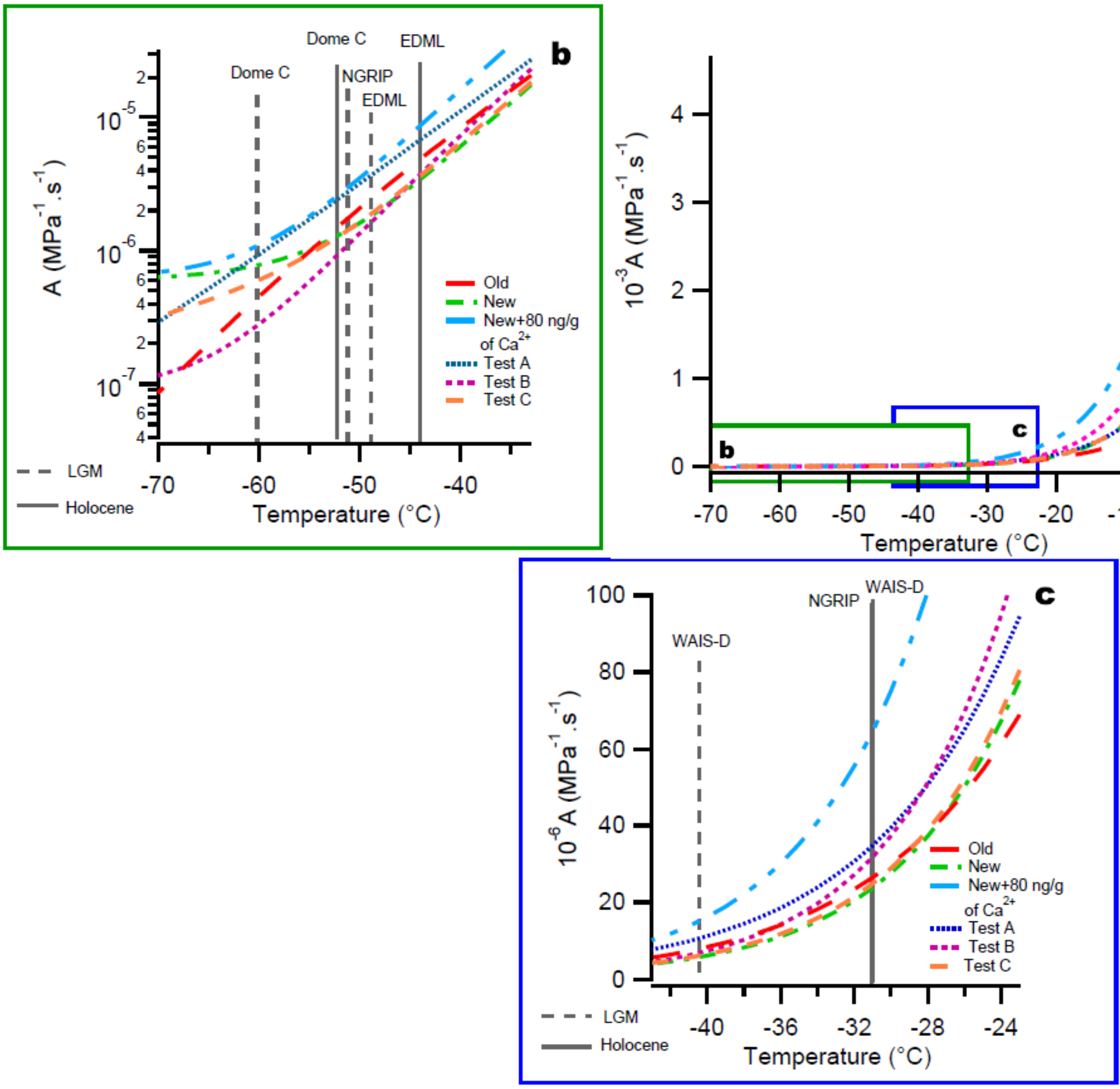
L. 689: "Evolution" It is not ’evolution’ but ’dependence’. Fig. 8b: Vertical axis title: not Log(A) is shown but A on a log-scale. Fig. 8 could be probably presented in one single graph with A on a log-scale facilitating the comparison between the different temperature ranges.

The caption of Fig. 8 and axis of Fig.8b will be modified.
We think that it is important to keep the 3 figures. They are useful to see the different scenarios for the different temperatures and especially the addition of figure 8c (by comparison with only figure 8b) permits to observe the intersection points between the different scenarios.
We have tried different representations but did not find a better way to present the results and the associated discussion.

L. 709: "an inversion of the d15N difference" probably better: "a correct sign of the d15N difference". ok

L. 711: I strongly question the value of tests A to C. The chosen parameters are very arbitrary. I don't see what the authors want to show us, except that some parameters are better here and worse there, which is rather trivial.

>> We agree that test C was not very useful in the previous manuscript., We propose to replace the original test C by a sensitivity test run with Q3=15kJ/mol a value close to the lower bound of the observed range for activation energies (Jung et al., 2004, reference provided by Referee#1). Using 15 kJ/mol leads to intermediate results between the "old" and "new" model results. 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 and hence to decrease Q3 largely below 15 kJ/mol (1.5 kJ/mol). This will be discussed in the revised manuscript.



L. 765: ".. instead of the Herron and Langway model.." -> instead of the parameters for the Herron and Langway model. ok

L. 781: "may act on deformation in opposite way" Why? Please explain.

>> “In particular, the solubility of dust particles, and their position inside or at the grain boundaries may act on deformation in opposite way” – the sentence is indeed not clear
“Dust particles do not always influence densification on the same way: dissolved particles soften firn and ice while the softening or hardening effect of non-dissolved impurities is less clear (Fujita et al., 2016; Alley et al., 1987)

L. 797: " an up-to-date version " This is an empty phrase.
The phrasing will be modified
L. 787: " the new parameterization of the creep parameter preserves good agreement between the old model outputs and data at sites that were already well simulated" Because the creep parameter is kept +- the same, so it is trivial.
The phrasing will be modified

L. 815: " This result is in agreement with the recent low delta age estimate by Parrenin et al. (2012) over the deglaciation at Dome C". No surprise, because it has been forced to agree.

>> This is true and will be specified.

L. 850: "Ore" -> Core ok