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Climate of the Past
Discussions

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 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.

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

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.

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.

Some specific comments:

L. 2: title "constrains" -> constraints

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.

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

L. 186: Is A0 a constant? Value?

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

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

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

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

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

very unlikely!

L. 362,3: "f1" subscript 1

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.

L. 427: Degree of polynomial fit?

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.

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

L. 471: explain "traction constraint"

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".

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?

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?

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.

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

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.

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

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

L. 797: " an up-to-date version " This is an empty phrase.

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.

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.

L. 850: "Ore" -> Core

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