This paper is based around the hypothesis involving grain boundary sliding (GBS) presented by Goldsby & Kohlstedt (1997; 2001). Goldsby & Kohlstedt initially didn't describe a flow law for ice in their early papers, instead they made a proposition (e.g. 1997, p.1404) "we suggest that GBS accounts for the remaining strain" this is supported by one micrograph which shows grain sizes of ~20 microns (Fig. 6 in Goldsby & Kohlstedt 1997). This is considerably finer than any of the <1 mm NEEM ice illustrated in Figures 2 and 3 in the present paper and the micrographs in Fig. 1 in the Part 2 paper. Using the Goldsby & Kohlstedt assumptions and hypothesis the current authors are presenting two different flow models, based on arguments that came from originally low strain experiments (~0.2 strain). Whereas in nature strains are substantially larger and represent ice deforming well into a Tertiary creep regime, something that is difficult to replicate in a laboratory. There are a large number of assumptions being made by the current authors concerning deformation mechanisms and processes producing the grain sizes that we see in the NEEM ice core (page 7) and the shear stress (page 8). Nowhere in this paper were modelled strain rates (Figs 9 and 10) compared with real closure rates data (strain rates) obtained during or after drilling. It is well known that in many ice sheets there are large strains and significant strain rate variations with depth and laterally as pointed out by Bons et al. (2018) and very many other authors; this has also been recognized in other drill sites on ice divides. Therefore it is hard to see how these strain rates are going to be replicated particularly if the authors assume GBS is critical to their models. The authors assume a constant stress of 0.07 MPa along the length of the NEEM ice cores (page 8 line 28) which is highly unlikely. The aims of the paper are summarised in a statement on page 9, namely:

"As this paper explores the effect of grain size, grain size distribution and different micro-scale models on the dominant deformation mechanism and the total strain rate, it is beyond the scope of this study to derive a stress-depth model for NEEM because this requires knowledge on the rheology, which is the property that is investigated here."

Well this is a very unclear statement and aim should have been clearly stated earlier in the paper. What is meant by "total strain rate"? Surely there must also be a discussion of the rheology? If rheology is investigated here, then why is it not described in this paper? Later on the authors foreshadow a companion paper (Part 2) on rheology (page 9, line 25; but this only considers the bottom section of the ice core); you can't separate this out from the present study. It would be sensible to combine these two papers into one seminal paper.

Title of paper is misleading and too long – There should be no Part 1 but combine it with a Part 2, this will avoid a lot of repetition, deficiencies in Part 1 such as the lack of proper discussion on role of temperature, discussion of CPOs and clearly bring out the differences above and below the 2000 metre depth in the NEEM ice cores. Words such as "on the deformation of Holocene and glacial ice" should be deleted from title as the general reader won't understand that modelling is only undertaken in this one section and not along the total ice core length.

Abstract: The last sentence is misleading as this paper in its current form is not discussing rheology. Again I feel that by combining Parts 1 and 2 into one paper would avoid some repetition.

The introduction nicely summarises ideas concerning GSI vs. GSS mechanisms and discusses grain size distributions. However, the authors really ignore the grain scale processes that are described by many other authors such as the competition between new grain nucleation vs. grain boundary migration. They even state that they are ignoring this in their response to Referee 1. This is a very major failing of the current paper. Although there is some discussion relevant to this on page 17.

The last section in the 'introduction' is about grain sizes, which the authors consider to be the primary controlling entity on strain rates in natural ice. Instead these are a product of different strain rates, strain histories and temperature and it is tantamount to a conspiracy that the author's dismiss (page 4 line 5) observations from previous NEEM workers to immediately assume that GSS mechanisms may dominate the deformation through a 2000+ metre ice column.

In fact I find this 'Introduction" is far too long and could be appreciably shortened, it is verbose and this is a problem throughout the paper.

Discussion on grain sizes (e.g. page 3, 5, 6 and 7) and late reference to Fig. 5 (Page 7) could be better consolidated rather than being variously discussed in different parts of the text, with some of the text incorporated into expanded captions for figures 4 and 5.

Methods: The application of GBS processes in ice and the predictions and hypothesis of Goldsby & Kohlstedt (2001) have for many researchers always been considered speculative and have been seriously questioned by numerous researchers in the glaciology community (starting with Duval & Montagat, 2002). The existence of grain boundary sliding at high strains as described in the concluding paragraph of the 1997 paper of Goldsby & Kohlstedt have not been convincingly identified in other experiments nor in nature. During the revisions to this paper the authors inserted an inappropriate reference to Craw et al. (2018) on page 15 to support GBS; in the latter case it involved strain localisation and may be a case as described by Duval and Lliboutry (1985). Even if we go back to Duval & Lliboutry's 1985 paper, they suggest grain growth is important relieve to any GBS and the latter is not a dominant mechanism.

Referee 1 questions the lack of taking into the account of CPO development in their models. The authors dismiss this (page 10, lines 1-5) and in section 5.5. In section 5.5 the author's argue that GSS is important. However the authors fail to present any glaciological or experimental study using coarser ice that clearly illustrates this is an important and known phenomena particularly in the high strains and temperatures observed in ice sheets. The majority of previous assertions for GBS are based on theoretical grounds, the questionable interpretation of Goldsby & Kohlstedt and do not appear to be a reality in natural ice (e.g. Duval & Montagnat 2002).

This composite flow law model (page 5) for ice needs to be better supported by definitive ice experiments, but a lot of the cited references are based on experiments that researchers had no idea of the true grain scale processes. In the results section, Fig 8 should be compared to those of the large strain experiments of Peternell et al. (2019) where a steady state rheology was only reached in fast strain rate experiments. In contrast, during a slow deformation there is insufficient seeding of new grains to enable continuous recovery, and there is a bimodal grain size distribution, but no evidence of GBS. These results are explained by stress concentrations within grains and not GBS. Particularly where basal planes are in unfavourable orientations for basal slip (hard glide orientation) coinciding with the development of a bimodal grain size distribution. In this and may other in situ experiments GBS is not observed in a polycrystalline aggregate, except under very special circumstances where grains may be highly anisotropic.

Page 4 line 6: I take exception to the statement "while the microstructures in the glacier ice suggest that GSS mechanisms may be important" This is not true and is not accepted by most members of the ice community.

2.1 Study site: There is overlap between Part 1 and the Part 2companion paper. It would be good if these could be combined and much of the unnecessary detail in Part 2 be removed. I also feel that **section 2.3** boundary conditions could be included in this section. A more concise description could be achieved by amalgamating these two parts.

2.2 Composite flow law: Given my scepticism about the composite flow law this section is well argued. However, I would like to see more discussion about other flow laws e.g. Glens as this is what everything is being compared against and is better expressed in Part 2.

Section 4 _ Results: Initially I found this section difficult to read and verbose, but after reading **section 5.1** some clarification occurred (Except delete discussion about Olivine). It may be worthwhile to reword and shorten this section.

Section 5.3: In reading this section and comparing figures 10a with 10b the significant differences, implications and the derivation of the stress exponent between these models are not clearly enunciated in the first part of this discussion. I believe with rearranging this section it will become clearer to the reader. Also could these two figures be combined as there is overlap?

Section 5.5.

This section in its present form should be deleted. Much of the discussion here is about other mineral systems and is irrelevant to the present models. There is substantial overlap with what is said in the following section. Only statements such as below should go into a reworded introduction.

"The c-axis eigenvalues show a minor variability in the glacial ice of the NEEM ice core (Eichler et al., 2013; Montagnat et al., 2014) where the layers of high strain rate are predicted. The strong development of CPO and the development of 10 substructures with depth indicate that large amounts of strain are accommodated by basal slip of dislocations in the NEEM ice core."

Alternatively, if Parts 1 and 2 are combined into a single paper then the authors would address the concerns of Referee 1. The statement at the top of page 10 is vague

Section 5.6: The overall statements in this section are almost contradictory with the last paragraph of this section really invalidating some of the reasons of applying these GBS-limited models.

The conclusions to this paper rely solely on strain models and the effect of a temperature input is only mentioned briefly in results (page 10. Line 30); this is really left to Part 2. There is no discussion of possible limitations in the application of these models, except in **section 5.6** and this should have been better integrated with the conclusions. There should be a discussion and comparison to what is happening in the lower levels of the ice core but this is left to Part 2.

I would strongly recommend papers 1 and 2 are combined. A shorter more concise paper will have a far bigger impact. For instance a good comparison between differences between Fig. 10 in Part 1 Vs. Fig. 4 in Part 2 would better explain the differences different models as they relate to temperature and strain regimes. It would have been good if the scales for Effective strain rates were the same.

Chris Wilson

16/10/2019

Additional references

Duval , P. Lliboutry L. 1985. Superplasticity owing to grain growth in polar ices. J. Glaciol. 31 60-62.

Duval, P. & Montagat, M. 2002. Comment on Goldsby & Kohlsted - J Geophy Res. V107, B4 2082

Peternell, M., Wilson, C.J.L., and Hammes, D.M., 2019. Strain rate dependence for evolution of steady state grain sizes: Insights from high-strain experiments on ice. *Earth and Planet. Sci. Lett.,* 506, 168-174.