

The Cryosphere Discuss., referee comment RC2
<https://doi.org/10.5194/tc-2021-188-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on tc-2021-188, Stoll and co-authors

Maurine Montagnat (Referee)

Referee comment on "Microstructure, micro-inclusions, and mineralogy along the EGRIP ice core – Part 1: Localisation of inclusions and deformation patterns" by Nicolas Stoll et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-188-RC2>, 2021

This paper presents some interesting observations of impurity locations on 10 samples extracted from the EGRIP core between 138 and 1339 m depth. The experimental protocol appears robust.

The main results coming from these observations are first that insoluble impurities (the ones observed here) are heterogeneously spread on the samples, and within the grains and second that the observed spatial distribution depends on the observation scale. A specific area, 300 microns distance from the detected GB is defined in order to test a specific location of impurities closer to GBs, but there is no clear signal of it on the observed samples. A "companion paper" is mentioned all along the paper and many of the results provided in this companion paper are evoked in the paper, and a large part of the discussion is based on these results. Furthermore, very little of the discussion stands on the results presented here. Most of it refers to other studies or studies that would be required to be able to improve the "story".

Based on these main considerations, I would suggest the authors to gather the presented observations to the companion paper in order to make a paper with noticeable results to comment and discuss. I am pretty sure that such a complete study would have a valuable impact in the field.

The present paper seem "too weak" on this aspect to provide a publication per-se.

I also have a strong concern about the way the specific location of grain boundaries is treated in the paper.

It is first clearly stated that the authors consider an arbitrary selected area, of 300 microns thick, around GBs as being representative of a closeness to GB. Then, in the text, and it is particularly problematic in the discussion or in the conclusion, this area is referred to as "AT grain boundary". I think this is dangerous as it could be easily mis-interpreted and it should be replaced by "close to GB" or "in the area of GB" all along the text.

Indeed, readers that will not go through the details of the study will likely omit the strong assumption that observations are not made ON or AT the exact GB, but in a nearby area. I would also suggest the authors to add a critical analysis of the impact of the chosen thickness value on the statistical results they provide.

Following are some comments concerning various parts of the paper that, I hope, will help clarify them.

P1, l18: I think it should be mentioned some of the pioneer works of paleoclimate reconstruction from ice cores, such as Lorius et al. 1985 or Petit et al. 1999 (the authors may know other pioneer work from other teams)

p2, l. 33-34 : About the impact of grain size on the deformation of ice. I think the statement is too simplistic. Situation is different in the case of ice deformed in the lab, for which experimental investigations were properly made and show that grain size only matters for transient creep (Duval and Le Gac 1980) while the minimum creep rate of secondary creep (therefore the Glen's law) or the compressive strength does not depend on grain size (Duval and Le Gac 1980, Jones and Chew 1981, 1983, Jacka 1984, 1994). An exception concerns the work by Goldsby and Kohlstedt (1997) that's dealing with very small grain sizes not relevant for natural ice on Earth. Concerning ice deformation in the conditions of the central part of polar ice sheets, most study related with the impact of grain size are modeling studies, or interpolations, and the grain size effect mostly comes from the fact that assumptions are made of grain-size dependent rheologies to match the model or interpolation results. Therefore this dependance is a pre-given assumption rather than demonstrated by results.

Fig 1: I am not sure, but it seems to have an incoherency between the pole figure representation of the texture and the color-code of the microstructure shown. For instance, on Fig 1 (k), green color represents an orientation that is perpendicular to the girdle represented in the pole figure. If I am right it might just be a question of representations, but it would make sense to have them coherent with each others.

Fig 2: Why is the last sentence here in the legend? It is a result analysis that should, to my point of view, remain in the text.

Part 2.2: please provide the spatial resolution used for your AITA measurements.

Part 2.3: why did you study only one sample of the last glacial period? It seems too weak for a statistical comparison with the other period, and in the mean time, it is complicated to compare this single sample with the 9 others owing to there different climatic origin.

P7 l. 142-143: here starts the problem with classifying "AT the grain boundary" the inclusions that are, in fact, in an area nearby a GB, whose size was arbitrary chosen to be 300 microns. Please replace by "nearby" or "in the vicinity of", etc. By the way, it would be good to justify this value of 300 microns?

P7 l. 146: I would call R_GB "the ratio of micro-inclusions to grain boundary area" and not the contrary?

P7 About grain size: considering the fact that you mention the grain size distributions to be wide, and very likely far from a gaussian, I think it would be more correct and more informative to provide a median, and a distribution (at least for a few samples). I know that mean area has been classically used, so it would be OK for me to keep giving this info, but I think it's time for us to be more correct in the way we present grain size values.

P8 l. 2: you are mentioning a "vertical girdle", but for me, on fig 1, the girdle is not vertical but rather inclined. Please correct or specify why you will consider this girdle to be "vertical".

P8 l. 185-186: is it realistic to hope having a clear trend with depth based on only 10 samples with 9 coming from the same period and only 1 coming from another climatic period?

Fig 3: I don't think the first sentence of the legend should appear here since there is no sign in the figure of stress or strain evaluation, only microstructure observations (interesting indeed!), so it is interpretation that should be given in the text.

I don't think either that these observations can explicitly tell that there are nucleated grains. You only observe small grains at GBs and triple junctions that you choose to interpret as nucleated grains, and that needs being justified. So maybe again, referring to nucleated grains should be left for the discussion part.

P10 l. 202: could you please give references about this hypothesis of "amorphous zone"?

Part 3.3: mentioning a percentage of inclusion AT grain boundaries is not equivalent to mentioning them as being "in the vicinity of" or "close to" grain boundaries...

In this part again, you observe 33 % of all inclusions to be located in your defined vicinity of GB, and this area (the vicinity of GB) represents 26% of the grain area... Can't you conclude out of that that the spread is homogeneous?

What would those percentages become if you were to increase or decrease this vicinity area?

What would your test of null significance become in these cases?

Discussion:

Please note that in many places in the text, and in the discussion, the reference to the companion paper is given. A large part of the discussion would not exist without the results of this companion paper, maybe that means that the two papers should be gathered together, since the present one is too weak to exist on its own.

Another "sign" of that is that very few of the results presented here are mentioned in the discussion, where it is very often referred to other studies, or other techniques to use in order to "fill" the discussion, in particular when talking about the impact on deformation, or in the recrystallization part.

For illustration:

p 14 l. 282-283: why aren't these observations focused on cloudy bands not shown here?

P 17 up to the end of part 4.3: everything that is discussed here is based on the results of the companion paper... And very few (if not nothing) on the presented results.

Part 4.4: in this part, it is mostly mentioned the recrystallization markers observed, with very few links with the impurity observations, although the paper does not deal with recrystallization per se, and that all these observations have already been commented in previous studies. And as mentioned, there is not enough observations here to discuss the specificity of recrystallization mechanisms.

p. 14 l. 268: NO, your result do not show that micro-inclusions are preferentially located AT grain boundaries. They eventually show that they, in some cases, are located nearby GBs... it is not the same at all! Especially when discussing GB pinning, influence of impurities on GB mobility, interaction with impurity interfaces, etc.

P 16 l. 329: do you think you have enough observations to be able to reject the hypothesis of Zener pinning?

P17 l. 335-336: you analysed only one sample from Glacial ice, it is enough to give this statement?

Conclusion:

"First systematic analysis": this sentence seems to me a little "two much" for 10 samples, and only one from the Late Glacial...

"Grain boundary are slightly preferred locations ...": I think that your results do not show that.

"The combination of optical microscopy and Cryo-Raman ...": this is not done in this paper.

"Observed clustering of micro-inclusions ...": do you observe enough of them?

"Grain boundary mobility ...": maybe I missed something but I think that nothing in your results allows to assess the influence of the impurities you observe on this mobility. And this is what is said in the discussion.

References:

-P. Duval and H. L. Gac. Does the permanent creep-rate of polycrystalline ice increase with crystal size? 25(91):151-158, 1980.

- A. Harte, M. Atkinson, M. Preuss, and J. Quinta da Fonseca. A statistical study of the relationship between plastic strain and lattice misorientation on the surface of a deformed ni-based superalloy. Acta Materialia, 195:555-570, 2020.

- T. H. Jacka. Laboratory studies on relationships between ice crystal size and flow rate. Cold Regions Science and Technology, 10(1):31-42, 1984.

- T. H. Jacka and L. Jun. The steady-state crystal size of deforming ice. Ann. Glaciol., 20:13-18, 1994.

- S. J. Jones and H. A. M. Chew. On the grain-size dependence of secondary creep. *Journal of Glaciology*, 27(97):517–518, 1981.

- S. J. Jones and H. A. M. Chew. Effect of sample and grain size on the compressive strength of ice. *Annals of Glaciology*, 4:129–132, 1983.

- C. Lorius, J. Jouzell, C. Ritz, L. Merlivati, N. Barkovi, and Y. Korotkevich. A 150,000-year climatic record from. *Nature*, 316:15, 1985.

- J. Petit, J. Jouzel, D. Raynaud, N. I. Barkov, J. M. Barnola, I. Basile, M. Bender, J. Chapellaz, M. Davis, G. Delaygue, M. Delmotte, V. M. Kotlyakov, M. Legrand, V. Y. Lipenkov, C. Lorius, L. Pépin, C. Ritz, E. Saltzman, and M. Stievenard. Climate and atmospheric history of the past 420,000 years from the Vostok ice core, Antarctica. *Nature*, 399:429–436, 1999.