Second review, paper by Fan et al. 2020, The Cryosphere

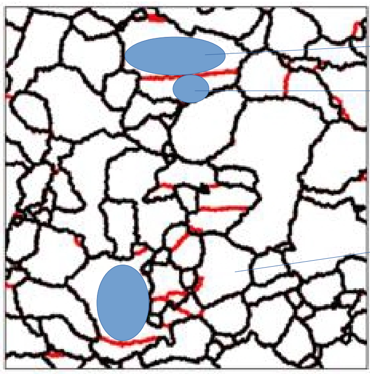
Responses to the authors' responses to my comments.

Thanks a lot to the authors for the detailed and complete responses to my comments and those of Olivier Castelnau, the other reviewer.
The responses are very detailed, what convinced me to review the paper a second time.

I will not go too much into details again, and I will focus on the main problems that I still see concerning some interpretations. I will provide more details below.

- First, I acknowledge the simplification made by removing the part concerning the WBV study that was, indeed, not utilised enough. This lightens the paper, and makes it clearer.
Nevertheless, my remarks concerning the calculation of a subgrain size remains the same since, whether one uses the WBV analysis results, or misorientation measurements (here with a threshold of 10°) does not modify the fact that the results presented here do not show any subgrain substructure that would enable to identify an "average" subgrain size.
In the situation of figure R1.1 a, when considering the large grains separated by a unique and nearly straight subgrain, what is the subgrain size deduced from your measurement? The same question arrises when considering the grains that are surrounded, at their boundaries by a few small subgrains. What is the subgrain size in this condition? Does it include the part of the inner part of the large grain (that is, indeed, a subgrain)? See figure below, within which we can really question the representativity of a subgrain size...
To bring this to light, maybe you could have plot not only the median, but the quartile, that would very likely have shown a large spread of data.

From figure 4c



Is it SG1?

SG2?

And in this grain, one very small subgrain, and another one nearly the size of the grain? Or is only the small one taken into account?

Since the authors strongly reduced the focus on the subgrain size data, this aspect concerning the metric is less critical, but the problem has not been solved to my point of view.

- Second, concerning the willingness of the authors to use GBS in order to explain the weakening associated to their observations.
Once again, there is absolutely no observation made here, neither the necessity, that render this GBS explanation robust.
Weakening can be explained by GBM, nucleation, subgrain rotation that are very clearly observed.
Texture formation can be very well explained by the same processes, together with dislocation creep, and the results at -30°C are new and very interesting in the sense that they make a clearer link between nucleation of small grains and strong clustered texture, so here again, GBS is not necessary
… so why?

Why is it so necessary to evoke GBS?
Let me mention that most of the articles evoked to justify GBS are coming from the same team as the authors', and also mention GBS as an interpretation, and are not providing any evidence of GBS. These interpretations are basically all built on the only observation of area with small grains.

On top of that, I have a strong concern about the explanations given after my comment 8-2, and based on figure R1.5.
The flow laws that are commonly used to characterized the mechanical response of a material are based on STATIONNARY behaviors, even if very short such as the peak stress for ice (or minimum creep rate in constant load conditions). Therefore a law of the type given by equation R1.1 (or the Glen's law for the minimum creep rate of ice), does hold only during this (pseudo) stationnary state. In the case provided by the curves of figure R1.5, the only place where it should be tested is the peak stress, since no other minimum, maximum or stationary behavior could be reached (such a stationary state is sometime reached during tertiary creep after 10% strain, as in Treverrow et al. 2012 for instance, but this quasi-stationary behavior results from a balance between a recovery process and a hardening one).
Then, the only way to verify a grain size dependance as written in equation R1.1 is to hold tests at different initial grain sizes, with all other parameters remaining similar, and over a large enough range of grain sizes. During the transient part (here before and after the peak stress), the law to be verified should include a time dependent parameter (such as a Andrade type law for instance). Ignoring this time dependant parameter could lead to a false grain size sensitivity (or any other type of sensitivity).

- One comment about figure R1.6: Wheeler et al. 1999 make it very clear, in the way they evaluate the WBV, that it is not an absolute measurement, and that it only provides an "lower bound", and therefore can only be used as a comparative tool with a lot of care (same type of surfaces observed, very similar deformation history, enough grains for enough statistics, etc...). We have tried to be very careful about that in our papers (Chauve et al. 2017, and Journaux et al. 2019) although our quantitative comparison must still be taken with care, and we made it "relative" (WBV_c / WBV_a). I thank the authors for this clear demonstration of such a necessity to treat this type of EBSD analysis with care.

- About comment 14-1:
I am sorry to read that a link was done between "spontaneous" nucleation and the study by Falus et al. 2011. Spontaneous nucleation, like modelled by Duval et al. 2012 can not give any clue about the nucleus orientation... and Chauve et al. 2017 only evoked it as a way to explain an orientation (of one single nucleus!) that had, apparently, no relation with the parents' orientations. Falus et al. 2011, and applicable also for most nuclei observed by Chauve et al. 2017, mentioned subgrain rotation (rotation or continuous recrystallization) as the main explanation for a weakening of the texture, and orientation spread away from the parent grain orientations, but not totally disconnected from these initial orientations. In section 4.1.4, mentioning nucleation by subgrain rotation (including bulging resulting from strain induced grain boundary migration, as in Chauve et al. 2017b) would be enough to explain the texture weakening, and more in phase with your observations in the small grain networks where small grains keep a strong relation of orientation with parent grains, cf fig 7b and 12 (such as explained by Humphreys and Haterly 2004 for metals, but already mentioned to impact recrystallization texture by Guillopé and Poirier 1979, suggested also for recrystallization along ice core by De La Chapelle et al. 1998, and clearly shown by Falus et al. 2011). Spontaneous nucleation is not expected to produce nucleus with any specific orientation, and GBS could also lead to very different orientations that are not observed here.
By the way, the increasing role of subgrain rotation with decreasing temperature (clearly stated in your conclusion, points 2 and 3), together with the fact that there is more difference between small

grain orientations and large grain orientations at lower temperature is coherent with the dominant role of nucleation by subgrain rotation at lower temperature.

- Another question, that I am not sure to have asked in the previous comments: at strain rates close to 10-5 s-1, with no hydrostatic pressure, ice is weakening by the formation of decohesion or fracture at or close to grain boundaries, in order to accommodate the imposed strain and the strain incompatibilities between grains. The hydrostatic pressure prevent microcracks to open, but then, what could be its impact on recrystallization mechanisms? And could it be that, as in Bourcier et al. 2013, a regime so close to the brittle behavior would, indeed, enhance GBS by the help of microcracks and decohesion?

Comments about the new version of the manuscript:

- Abstract: lines 9-11. Please correct, see previous comment, by replacing the mention to spontaneous nucleation by the mention of nucleation by rotation of subgrains (rotation recrystallization) instead. Spontaneous nucleation model, as existing so far, does not allow to predict any type of orientation relation between nuclei and parent grains.

- Part 3.3.2: I don’t understand what is this square mean root diameter... I know Root Mean Square parameter (RMS), that is square(mean(x^2)), and that has a statistical meaning, but I don’t know the meaning of mean((square(x))^2), or (mean(square(x)))^2, it is not clear... Please verify

- Part 3.4.4, line 14: Fig 8-10 instead of 9-11?

- p 12 line 27, “strai,n” → “strain”

- p 15 line 9, only here is the definition of “number density” given while it is used before, please give the definition when first using it.

- p 16 line 2: reference by Placidi et al. 2004 has no reason to be here since it is modeling.

- p16 line 16-17: I don’t see why is this study by Eleti et al. mentioned here? I would suggest to keep it for the discussion part.

- p 16 line 20: please also mention the reduced role of GBM when decreasing temperature in impacting the grain size! This is likely the most important one, since grain boundary mobility is strongly reduced...

- Part 4.1.4: please see my comment about the various hypotheses for nucleation mechanisms and impact on texture.
Again here, the two strong hypotheses to mention concerning the weakening of texture in the small grains would be (1) nucleation by subgrain rotation (strengthened by the observation of subgrains whatever the level of strain and the temperature conditions), and (2) GBS in the fine grain necklace (I am not convinced, but let’s assume it as a likely mechanism, and here you could mention the ref of Eleti et al.). The first hypothesis has been documented directly and indirectly by several authors (see reference in my previous comment on this subject, but there might exist others). Spontaneous nucleation could be mentioned, but can not be used at the same level since there exist no study showing it systematically, and showing the clear effect on nucleus orientation. Furthermore, if this spontaneous nucleation dominates, then there should be no orientation relation between nucleus and parent grains, while you observe one here.

And please correct the fact that Falus et al. 2011 only mention rotation recrystallization and not at all spontaneous nucleation.

- p 18 line 32: please correct “observations” into “interpretations” since Craw et al 2018 do not show more proof than you of GBS, but use, as you do, GBS to interpret their observations...

- p 19 line 9-11: please be careful with the interpretation of a “hidden grain size sensitivity”. I suspect that this is mainly because data from non stationary flow are used to extract parameters from a law including a grain size sensitivity devoted to a stationary state, see my comment about your figure R1.5. Therefore this is not “hidden”, this is just not applicable...

- p 19 line 12: replace “spontaneous nucleation” by “nucleation by subgrain rotation”.

- p19 line 21: the assertion (1) is incorrect, or not clearly stated... You might mean “softening owing to the reduction of stored strain energy by nucleation and grain boundary migration (or recrystallization processes)” (not “defects”, because we don’t know which defect you are talking about). This softening has been documented for ages by people studying recrystallization... So please cite Humphreys and Haterly 1996 for a review (or maybe Derby and Ashby 1987), and for ice, maybe Duval 1979, or maybe also Weertman 1983 ?

- p 19 line 27-28: again, the statement is wrong. The balance is between accumulated stored strain energy through dislocation (hardening) and recrystallization mechanisms that reduce this stored energy (both nucleation and GBM, recovery processes)... And again, Montagnat et al. or Sakai et al. are not the one showing that, it had been demonstrated by the whole recrystallization community for a long time!

- p 20 line 9: same mistake again! Not at all the balance between “GBM and nucleation”! Both processes are the softening processes associated with recrystallization... So it is about a balance between softening and hardening processes (that is indeed not balanced here since your experiments still are in the softening part).

- p 21 line 6 “we interpret that the of”... something is missing

- conclusion point 5: reference to “spontaneous” nucleation should be replaced, here, by the mention of nucleation by rotation recrystallization, as detailed in my comments before. Or you could keep it, but as a 3rd and very hypothetic mechanism.

- M. Bourcier, M. Bornert, A. Dimanov, E. Héripré, and J. L. Raphanel. Multiscale experimental investi- gation of crystal plasticity and grain boundary sliding in synthetic halite using digital image correlation. Journal of Geophysical Research: Solid Earth, 118(2):511–526, 2013.

- T. Chauve, M. Montagnat, F. Barou, K. Hidas, A. Tommasi, and D. Mainprice. Investigation of nucle- ation processes during dynamic recrystallization of ice using cryo-ebsd. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 375(2086), 12 2017 b.

- B. Derby and M.F. Ashby. On dynamic recrystallization.Scr. Metall., 21(6):879–884, 1987.

- P. Duval. Creep and recrystallization of polycrystalline ice. Bull. Mineral, 102:80–85, 1979.

- F. J. Humphreys and M. Haterly. Recrystallizationand related annealing phenomena. Pergamon, 1996.

- J. Weertman. Creep deformation of ice. Ann. Rev. Earth Planet. Sci., 11:215–240, 1983.