

## ***Interactive comment on “Strain heterogeneities at the ductile to brittle transition; a case study on ice” by Thomas Chauve et al.***

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We are thankful for very interesting and helpful comments provided by both reviewers. We hope that we were able to make the best use of these comments to improve our paper. In black are the reviewer comments, and the authors response appear in between.

We provide a new version of the manuscript as a supplementary.

Referee #2

This manuscript describes original results on the DIC analyses of poly-crystalline ice

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under creep deformation. As described in the authors' previous paper (Acta Materialia (2015)), application of the DIC method to ice provides a powerful tool to investigate evolution of strain fields during plastic deformation. In the present manuscript, very interesting results on behavior of local strain fields associated with cracking are presented, and the argument addressed are suitable for publication in Journal SE. However, I found some of the authors' explanations difficult to follow. The manuscript should be improved before acceptance for publication, with considering the following points:

(1) (General comment) For convincing argument, focusing on the experimental results found in the present study, distinguish more clearly those from others already presented in the previous papers by the author(s) and other researchers.

## RESPONSE

Thanks for this comment. We tried to make this distinction by highlighting the main useful results from our previous studies in the introduction, p. 2 lines 15-19. Grennerat et al. 2012 made the first measurements of strain field evolution in ice polycrystals. Chauve et al. 2015 showed the evolution of local strain field with dynamic recrystallization processes (nucleation and subgrain boundary formation). Sentences were added p11, lines 13-16.

## END

(2) Reconsider the description in 'Abstract'. The main purpose of the study must be to clarify 'the evolution of local strain fields around cracking' by the use of the DIC method as described in the top sentence in 'Abstract', but the description on the most important result obtained by the study is not clear. For example, if the argument is concluded by the last sentence 'A strong interaction between cracking and dynamic recrystallization is therefore evidenced', I wonder if it is a new finding. Such a general phenomenon may be already presented elsewhere. Consider carefully what is the most important finding made by the study. In addition, the title suggests 'strain heterogeneity' for the main topic of the paper but no descriptions about it in 'Abstract' and 'Concluding remarks'.

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## RESPONSE

Following your advices, the title was modified in “Strain field evolution at the ductile to brittle transition; a case study on ice.” We also modified the abstract and put in the notion of heterogeneities, the link between changes in microstructures and stress redistribution. We left and modified the sentence rising the role of DRX as a way to relax the crack-tip stress field, since we didn’t find any paper making such a direct relation. Although it might be specific to ice, we add “ice” into the sentence. Then, we tried to give more consistency between the conclusion and the abstract.

END

(3) ‘Introduction’ should be more concise, with focusing on the main topic of the paper.

## RESPONSE

OK, we have shortened it a bit putting the accent to the ice case. We left the description of ice deformation behavior, and on previous work characterizing strain heterogeneities and evolution, in order to show that these studies were only performed in the ductile regime.

END

(4) I found very interesting results are presented in section 3 ‘Strain field evolution ....’. It should be emphasized more clearly what is found in the present study, and describe it also in ‘Concluding remarks’.

## RESPONSE

It is not clear for us which of the interesting results were not highlighted in the concluding remarks. - we mentioned the fact that cracks appear at the side of deformation bands, where we can expect higher Schmid factor conditions - we mentioned that cracking modifies the deformation band pattern - we mentioned the co-existence between micro-cracking and dynamic recrystallization mechanisms, with DRX plasticity-

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driven at the crack tips. - we therefore highlighted the efficiency of DRX mechanisms to relax local stresses. - and also the fact that changes in microstructure due to cracking and DRX induces a redistribution of local stress field. . . Maybe the reviewer could help by providing a clearer statement of what we missed? In the new version, we tried to improve the concluding remark part.

END

(5) (Line 5 to 6 on p.15. In section 5 'Concluding remarks') What does 'large' mean in 'large bands' (large in width, length, or thickness) ?

RESPONSE

'large bands' mean band with a length larger than the grain size. We added this information.

END

What is the difference between the 'band' in 'strain field localises into large bands' and the 'zone' in 'strain localization zones'?

RESPONSE

There is no difference between strain 'zones' and 'bands'. We clarified this in the text.

END

In addition, the description 'cracks appear nearby but not on the strain localization zones, where deformation by dislocation glide must have been impeded by low Schmidt factor conditions' is difficult to understand. If dislocation glide is impeded in the 'strain localisation zones', how does strain localise into the 'zones' ? A clear-cut description is required in 'Concluding remarks'.

RESPONSE

We meant that stress must concentrate at the side of strain localization bands, where

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crack later appear to relax it, and one of the reason for such a stress concentration might be that there exist regions where local dislocation glide is easy (where strain concentrate), but if the region just nearby has a low Schmid factor, deformation can not occur, and stress concentrate up to cracking. This is why “nearby” is very important. We reformulated this part to make it clearer.

END

(6) (Line 9 to 10 on p.15) The description ‘a strong redistribution of the local strain field such as already observed by Chauve et al. (2015)’ should be revised to distinguish more clearly the original results obtained by the present study from the results already presented in other paper to avoid readers’ misunderstanding in evaluation of this paper.

RESPONSE

This clarification has been done in the discussion part. We hope that it answers this comment.

END

(7) (Line 11 to 14 on p.15) This paragraph is not easy to follow because the experimental results (facts) and speculative descriptions are not well distinguished. As a concluding remark, what was found in the present study should be more clearly described.

RESPONSE

We tried to emphasize more clearly the main result of our study by reformulating this part.

END

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2017-24/se-2017-24-AC2-supplement.pdf>

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