

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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June 20th,

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.

A new version of the paper is attached.

Referee #1

In the work described in this MS, the authors investigate, using the method of Digital

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Image Correlation (DIC), the localization of plastic strain and of cracking within one specimen of S2 freshwater ice as it shortened by up to 5.5% under a set of creep conditions just on the ductile side of the ductile-to-brittle transition; specifically, under a compressive stress of 1 MPa at -7°C (or $0.97 T_m$). They present observations which show that strain is concentrated within a few bands, by a factor as high as 10 to 20. In addition and this is the novelty of the MS—they claim to see evidence of dynamic recrystallization at the tips of short, deformation-induced cracks that formed outside the regions of localized plastic flow. Recrystallization, they state, serves to relax and then to redistribute stresses that develop within the crack-tip plastic zone and thus, presumably, to stabilize cracks against propagation. The work could be seen to support an earlier model of the DB transition (Renshaw and Schulson, 2001), even though that model does not specify the need for dynamic recrystallization, and thus to add further detail about a phenomenon that is important to the inelastic behavior of a variety of materials. That said, the MS leaves something to be desired.

The major shortcoming is unambiguous evidence of dynamic recrystallization at crack tips. The claim is made (p.10, lines 18-19; p.14, lines 5-8) that cracks-2 & 3 of Fig 8c-8d appear to be localized within an area where new grains recrystallized. While it is quite reasonable to expect that recrystallization could occur within the plastic zone at the tips of cracks, particularly within material as warm as that examined here, the evidence to support this point the key point of the MS not compelling.

RESPONSE :

Our observations are, here, only supported by the observations of the microstructure (with a 20 micron resolution) before and after the experiments. The fact that the microstructure modifications observed close to crack tips are associated with dynamic recrystallization (DRX) mechanisms was based on published recrystallization observations like those recently performed in Chauve et al. 2017, where comparison between similar thin section observations and EBSD observations enabled to clearly assess microstructure changes associated with DRX. Of course, our resolution is not accu-

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rate enough to pretend that the new grains or subgrain boundaries appeared exactly at crack tips, but they are located very close to them (new purple grains around crack 4, or blue grains close to crack 3 and crack 2 tips, even light green new grain at the bottom of crack 1) Here, the reviewer mentions figure 8, showing the strain field around cracks, while our DRX observations are based on figures 3 and 5 where new subgrain boundaries and new small grains are visible in the crack region. The sentence p.10, lines 18-19 refers to figure 5, and mentions the relation between deformation bands and the new grains : “By looking at the final microstructure (figure 5), these new deformation bands appear to be localised in an area where new grains recrystallized”. In sentences p.14, lines 5-8 (now page 13, line 15) we have changed “at crack tip” by “close to crack tip” to be in better agreement with the limits of our accuracy, and to follow the reviewer’s comment.

END

Another shortcoming, perhaps more an oddity than a weakness, is the apparent absence of shear deformation within the near-vicinity of cracks. In Figure 8 strain near the three cracks is shown to be predominantly tensile. Yet the cracks are inclined to the direction of loading and so one would have expected a shear stress to act in their plane. In the ideal case of no end-constraint (point 3, below), the ratio of shear stress to normal stress is given by $R = \tan \theta$ where θ is the angle between the normal to the plane of the crack and the direction of loading; in the real case of end-constraint, $R > \tan \theta$. For crack-3 in Fig 8, for instance, $\theta \sim 15$ degrees so that $R > 0.25$. This is a rather large ratio, begging the question: why is no shear strain detected in the near-vicinity of the three cracks?

RESPONSE

We totally agree with the reviewer, and we were also expecting to observe more shear strain localized in the crack-tip areas. Nevertheless, our observations only show local strain configuration, and to relate it to a local stress state is not straightforward. The

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main explanation, that we tried to give in the discussion (but we tried to make it even clearer in the new version) could be related to the strongly anisotropic behavior of ice, and the fact that our microstructure has large grains, and is far from being isotropic. Therefore, as shown already by Grennerat et al. (2012) and Piazzolo et al. (2015) (refs in the manuscript) the tensorial local stress state (simulated in these works) can be strongly different from the applied one. On top of that, Chauve et al. (2015) have shown the ability of DIC measurements to evidence local shear strain. The fact that we don't observe a strong shear strain here could therefore be associated with an heterogeneous local stress field, differing from the applied one, and leading mainly to a mode I opening of the cracks.

END

Thus, owing to the points noted in the previous two paragraphs, this MS as presently developed should not be published. However, the authors should be encouraged to pursue their work, for the presentation of unambiguous and compelling evidence of the main point they have in mind, assuming it to be correct, would be a positive contribution to the literature. In so doing, they should consider and then address the following points:

1. In calculating strain from relative displacement of points on a speckle pattern using the DIC method, what precaution was taken to ensure that the only movement detected from one image to the next was through deformation of the ice? In other words, to what extent did vibration and other extraneous movements of the camera contribute to apparent displacement and hence to inelastic strain?

RESPONSE

The DIC method used to extract displacement fields, and then strain fields, from image correlations is now very well documented (see Sutton et al. 2009 Image Correlation for Shape, Motion and Deformation Measurements, for instance, but also Vacher et al. 1999 for the 7D software used here). In particular, it was shown that the method

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enables to remove any effect of a displacement of the relative position of the camera compared to the sample (in the sample plane). Accuracy tests were performed in the specific case of ice, and with the specific equipment and experiment configuration used here by Grennerat et al. (2012), therefore we refer to this work. And at the end, we can exclude vibrations (negligible in our conditions, creep test, no motor) to provide anything else than noise on top of the signal. Their effect is, therefore, included in the resolution evaluation.

END

2. Identify using an arrow “decohesion features” in Fig 3c, and then define them. Are they the kind of feature reported by Picu and Gupta (Acta Mater., 43(10), 3791-3797 ([http://dx.doi.org/10.1016/0956-7151\(95\)90163-90](http://dx.doi.org/10.1016/0956-7151(95)90163-90)) and by Weiss and Schulson (Phil. Mag. A, 80(2), 279-300 (dx.doi.org/10.1080/01418610008212053)).

RESPONSE

You are right that, in Weiss and Schulson paper, a clear distinction is made between decohesion and cracks. We took this work for granted, assuming that both can exist during brittle deformation of ice (or at brittle to ductile transition like here), but we did not perform the work to distinguish them, since we only focused on what happened around cracks. We could remove the term “decohesion” for clarity? But maybe the best is to add the reference to Weiss and Schulson 2000 paper. See changes p.5 line 4 in the new version of the paper.

END

3. Could the deformation bands shown in Fig.4 and elsewhere be a result of end-constraint imposed on the square-shaped (9 cm x 9cm) specimen by boundary conditions external to the ice (i.e.,by the loading platens)? Boundary conditions are mentioned in the Discussion (p.11,12), but more within the context of grain boundaries and their influence on local stress state than within the context of end zones. Given the

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square shape of the specimen, the entire volume of the ice was effectively confined. To know whether deformation bands are an intrinsic feature of ice creep, experiments need to be run using specimens whose length to width ratio is closer to 3 or more.

RESPONSE

Deformation bands oriented at close to 45° from the compression axis are expected and observed (Grennerat et al. 2012) for this type of experimental setup. Indeed, when the sample is homogeneous (small grain size, RVE), maximum shear stress occurs within these two orientations. Grennerat et al. 2012 have shown that due to the large grain size, to the plastic anisotropy of ice, and to the “non-RVE” configuration of experiments performed on these square-shaped specimen, the bands are deviated from their theoretical orientation. The purpose of this work is clearly not to focus on the intrinsic feature of these deformation bands, and the analysis of their formation in the specific case of ice (since it has already been done by Grennerat et al. 2012 in ice, but also by Heripre et al. 2007, International Journal of Plasticity in metallic materials for instance), but we aim at showing the link between the strain localization, crack mechanisms, and recrystallization. Since it appear that we may not have been cleared enough on this point, we tried to clarify the way we mention boundary conditions in the Discussion.

END

4. To Figure 6 and elsewhere where blue (compressive strain) and red (tensile strain) arrows signify the two principal strains, add a scale.

RESPONSE

Thanks, this has been done.

END

5. In the increment of strain from Fig 8b to 8c, crack-3 appears to close. Closure is claimed (p.10, line 22; p.14, lines 15-20) to be caused by a local compressive stress

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which is related to the formation of new boundaries formed by nucleation. How exactly would recrystallization develop a compressive stress normal to the plane of crack-3?

RESPONSE

Local compressive stress is needed to explain the crack closure as written p.14, lines 15-20 “In order to obtain a local closure of cracks, the stress field must provide a local compression component, perpendicular to the crack surface.” Hence, to explain such a change in the local stress field from tension (leading to crack opening in mode one) to compression (leading to cracks closing), we assumed that some changes in the local stress field has occurred during the test. The most likely reason for this change in stress field configuration was attributed to the modification of the microstructure, and this change is due to dynamic recrystallization and cracking. Therefore we do not attribute directly the stress changes to recrystallization, but to the modification of microstructure induced by recrystallization, see p.10 lines 27-28 and p.14 lines 8-9. Such redistribution of the local stress field due to microstructure evolution is coherent with previous results of Chauve et al. 2015 (Acta Mater), and is enhanced by the high visco-plastic anisotropy ice.

END

6. Typos The images in Fig.4 should be reversed, in that the one on the left is of the lower spatial resolution.

Ok

On p. 7, “if the AITA” should read “of the AITA”

Corrected

On p.10, lines 29 and 34, “there” should be spelled “their”.

Corrected

On p.14, line 26, “beyong” should be “beyond”.

Corrected

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2017-24/se-2017-24-AC1-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2017-24>, 2017.

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