The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-190-RC2, 2017

The Cryosphere
Discussions
Open Access

Interactive comment on "Recent rift formation and impact on the structural integrity of the Brunt Ice Shelf, East Antarctica" *by* Jan De Rydt et al.

Anonymous Referee #2

Received and published: 6 November 2017

The authors present a collection of detailed observation data regarding the recent formation and propagation of two rifts in the BIS which may be crucial for its future. The first, called Chasm 1 by the authors, emerged and has been reactivated from a pattern of curved cracks and now propagates towards MIR. The second one is the Halloween crack which has been initiated near the MIR, and moves away from this spot rapidly. Based on numerical calculations in conjunction with the observed kinematical data, a prediction of the future rift propagation trajectories is made.

The observation data are of great value, as they provide, among others, a detailed insight into the kinematics of the crack propagation, the widening and the deformations near the rift tips. Also the numerical crack path predictions are interesting and I am sure that they will attract considerable attention. The paper is well written and clearly in most parts. In addition to these rather general remarks, I would like to make a few comments on points which have struck me.

- The main point is the following: I am not absolutely sure, but there seems to be an inconsistency between the velocity field as shown in Fig.7a and the obvious assumption that both cracks are pure mode I cracks. Figure 7a shows clear velocity jumps across the entire rifts in their normal as well as in their tangential direction which suggests a so-called mixed-mode loading. Such a mixed-mode loading would usually lead (homogeneity of the ice shelf presumed) to a crack curving (in this case to the left in propagation direction) and not to a nearly straight crack as predicted from the model. I wonder what the reasons for this apparent contradiction are. It might be that the velocity field is not sufficiently correct. But, on the other hand, this field has been used to optimize the model, i.e. the model results should show the same tendency. Perhaps the authors can resolve this question easily.

- As the authors mention on page 15, the calculated principal stress trajectories have been used for crack path prediction instead of the measurable principal strain-rate trajectories because of the smoothening filter effect of the field equations. Here, a few explanations would be helpful, how this filter works and how its results depend on the input data as e.g. the measured velocity field. I wonder to what extent the filter may smoothen important observation facts like the velocity jumps.

- On page 18 the authors mention that their propagation algorithm makes use of the direction of maximum tensile stress, but that the stress magnitudes are not a good indicator for the formation of new rifts or for predicting the future propagation. This statement might be correct, but this is not directly shown in this paper. In this context it should be emphasized that usual fracture criteria never use directly the stress magnitude and that the question of an appropriate fracture criterion is not touched in this paper.

- Some more details regarding the used inversion method or the resolution of the measured velocity field and calculated stress field would have been helpful and interesting for the reader. For example, if available, one could use sufficiently accurate displacement-, velocity- or stress fields to check the applicability of different fracture criteria.

- In Fig 2b caption and page 7: Though clear from the context, the term 'local strain rate network' may mislead some readers. Measured are not strains but relative displacements (widening per length). Furthermore, the white marked stakes are hardly distinguishable from the yellow O0/N0 markers. In R18: combing -> combining

- In Fig.7b and page 19: Again, though clear from the context, the term 'effective strain' may mislead some readers. In addition, it would have been more informative not to use x,y-coordinates, but local normal and tangential directions.

- Fig.3b: I see the drawn black lines, but I cannot see the cracks (magnification problem?)

- P14, R24: '. . .based on the observed stresses. . ..': stresses have not been observed and cannot be observed directly! They may be calculated.

- Multiple: 'principle stress' -> principal stress

- P15, R30-35: the details of the principal stress rosette pattern in the vicinity of MIR can only be seen if the figures are sufficiently magnified. It is indeed 'radial' but, strictly speaking, not symmetric since the radial principal stress changes its sign during circulating MIR. The pattern corresponds to that of a point force in a plate acting in the direction of the HC.

P3, R20-21: I agree fully with the opinion that observation data in conjunction with numerical simulations, as they are impressively presented by the authors, will improve the understanding of rift propagation and of the accuracy of relevant predictions. But I doubt the need for specific iceberg calving laws. The calving law is already known and given by the laws of continuum and fracture mechanics. The only and very difficult problem is to describe the rift evolution until final separation.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-190, 2017.