Review of "Calving cycle of the Brunt Ice Shelf, Antarctica, driven by changes in ice-shelf geometry" by De Ryt et al. - Joe Todd

This study combines multiple observational records with inverse modelling to study the ice dynamics, stress & fracture of the Brunt Ice Shelf. Data from satellites & in situ measurements are assimilated into the SSA model Úa to invert for the flow parameter A across the shelf, and the resulting stress maps are analysed to build up a timeline of ice shelf stress conditions before, during and after the re-activation of Chasm 1 and the propagation of the Halloween crack. This is an interesting and well-presented study which warrants publication in The Cryosphere; as the authors note, the 'natural' cycle of stress concentration and release on ice shelves is a major factor controlling calving. I strongly agree with the conclusion that full-thickness rifting should be resolved in ice-sheet models. I think the manuscript could benefit from some additional details on the modelling results and some clarifications.

Thank you for these positive comments.

General comments:

It is not totally clear from the figure captions & text whether the stress maps shown in Figs. 2 & 3 come from Úa model output. I can see 3 possibilities: (1) The stress maps are produced using observed velocity (from which strain can be derived) and an assumed constant flow parameter A. (2) As above, but A comes from Úa output. (3) The stress maps are a direct output of Úa simulations. The text strongly implies (3) but, from reading the methods section, I do not think that any fractured domains were studied with the model. Is rifting accounted for through inversions (i.e. low A where rifts exist)? If (3) is the case, more details should be added to explain how the rifting is accounted for. If (3) is not the case, clarifications and modifications are required in the text to avoid giving a false impression to the reader. It would be nice to see the results of the model inversion (maps of 'A') and this would probably also help clarify the point above. In general, it's just not very clear at present exactly *how* the model was used.

Snapshots of observed surface velocities and ice shelf geometries were assimilated into Úa, and for each snapshot, an optimal solution for *A* was obtained through an inverse method, which optimizes the misfit between observed and modelled surface velocities. The resulting solutions for *A* together with the diagnostic surface velocities were used to calculate the stress maps. We have clarified our methodology in section 3, and added further details about the inverse method and resulting maps of *A* in Appendix A. To address your concern about the absence/inclusion of fractures in the domain, we also provided a comparison for *A* and the stress field between (1) the inversion with a continuous mesh (i.e., rifts are filled with ice/melange), and (2) the inversion with rifts as holes in the mesh (i.e., rifts correspond to open ocean) in Appendix B. In summary, method (1) produces high values of *A* along the trajectory of active rifts, whereas in method (2) these high values are absent. Yet, the derived large-scale stress distribution of the ice shelf remains qualitatively similar between both methods, which demonstrates the robustness of our results with respect to the representation of rifts in the model domain.

Specific Comments:

P3L9-10 – Can the broad-scale pattern of ice shelf thinning be established? Paolo et al. (2015) seem to provide data which covers the BIS. You make a compelling argument for the first-order importance of internal dynamics/heterogeneity for crack propagation, but does this completely preclude any external environmental signal?

We have no additional data on broad-scale changes in ice thickness other than the maps produced by Paolo et al.. This work was cited in the paper.

P4L3: This sentence implies that all the stress results shown in the paper are Ua model output. Is that the case?

All stress maps were calculated from diagnostic model output, i.e., an optimal solution for *A* and corresponding surface velocities for different snapshots in time. 'Optimal' is defined in the sense of inverse theory, i.e. as a minimum of the cost function which penalizes the difference between (gradients of) the observed and modelled surface velocities (see Appendix A for further details).

P4L8: Could you show the inverted-for A parameter, perhaps in supplementary material? Presumably there are some pretty interesting patterns.

More details about the model inversion and patterns for *A* are included in Appendix A.

P5L3-11: I am not totally clear what the approach is here. How do you shift the DEM to an effective timestamp? What does the LIDAR data provide?

Both points have been further clarified in the text.

P6L14: Again, this strongly implies that Fig 2 & 3 represent model output.

The calculation of the stress field requires the surface velocities in combination with a rheological model. As a first approximation, the rheology *A* can be assumed to be spatially constant and observed velocities can be used to calculate the stress field. However, a more accurate approach is to allow for a spatially variable *A* (obtained through a formal inversion of the observed surface velocities) and use the corresponding modelled surface velocities. In the latter approach, which was followed here, the forward model can be interpreted as a physically-based filter to reduce the measurement noise. These points have been clarified in section 3.

P6L26: 'Ocean pressure acting on newly formed rift surfaces' - I'm slightly confused by this. On a floating shelf, the overall ocean pressure should be equal to the hryostatic pressure which existed before the crack formed. The exception, which I guess applies here, is if the intact shelf was under significant tension. But is it really accurate that the ocean pressure is pushing the rift apart? I'd have thought that its the concentration of the supported stresses onto a narrower band (the remaining intact ice) which promotes further fracture growth.

We did not mean to imply that ocean pressure drives rift propagation, rather to emphasize the importance of normal pressure acting on newly formed surfaces for the force balance. Note however that perturbation experiments by Gudmundsson et al. 2017 have shown that if the ice melange inside Chasm 1 is removed and replaced by a normal ocean boundary condition, changes to the flow of the ice shelf remain small compared to the 2-fold increase in flow speed since 2012. To avoid confusion, we have reformulated this paragraph.

Fig 1 or 2: As I was reading the results section, I was thinking it'd be nice to clearly visualise the compressive arch. Could you perhaps add a panel (or overlay Fig 1a) showing regions with extension in both directions, versus one compressional component (like Doake et al., 1998).

In the interest of keeping the figures as simple as possible, we had hoped that the reader would be able to approximately trace the outlines of the compressive arch from the arrows in Figure 2, as black arrows are extensive and red arrows are compressive.

Fig 4b: Observations & model match well to the south of the MIR, but the difference grows further north. Can you speculate why?

The key process that causes a slow-down of the ice shelf is the regrounding of the MIR, which is represented well in the model. Further away from the MIR, where the impact of regrounding becomes weaker, other factors come into play, such as temporal changes in the rheology or damage, which are not represented in the model (this has been further clarified in section 6). In particular, further towards the east, just outside the limits of the figure, lies another active rift called the Brunt-StancombWills rift (see e.g. [Gudmundsson et al., 2017]) that locally affects the observed velocities.

Minor Comments:
P2L27: A bit pedantic, but I think 'single' would be better than 'singular' here. 'Singular' tends to refer to an exceptional event or thing.

We have removed 'singular' from the text

P3L15: 'preconditions for rifting were re-established'. What were these preconditions? I think the rest of the paper lays out what these preconditions were, but its perhaps a little premature to say this here without explanation.

We added '(as will be explained in section 4)'

P3L18: 'singular' again

We have removed 'singular' from the text

P4L11: slight formatting error – ref in brackets

Brackets have been removed.

Fig 1: North arrow?

As we plotted parallels and meridians (dashed grey lines) in figure 1, we did not see the need to include a north arrow.

Fig 2: Unless is really reduces the clarity of the figures, I'd think that for a colour scale with a white minimum, the minimum ought to be 0 kPa.

In this figure we draw the reader's attention to spatial patterns, with most of the relevant variability between 40 and 130kPa, as reflected in the colour scale. We did not see any benefit in colouring areas with background stresses below 40kPa, as these do not contribute to the story.