Response to Reviewer #1

One place where I was confused was caused by not realizing the nature of how the solutions were derived, numerically using Equations 6, 7 and 8, apparently, as stated in the appendix. I didn't at first notice a reference to the appendix in the text.... after searching for it, I see it is referenced on page 7. It may be that the main body of the text needs a more forthright statement about what is done to produce the results, and a "louder" statement of what Appendix A is about would be helpful to some readers.

In a revised manuscript, I have made a significant effort to address this concern. I have added text at the beginning of Section 3 that clarifies the modeling approach. I've also incorporated the Appendix into the main text in a way that is more clear for the reader. I have also reorganized subsections (and sub-subsections) in Sections 3 and 4 so that the paper outline offers a better guide of the calculations that are presented. I have also added several "signposts" throughout the manuscript that attempt to orient the reader.

I think it is important to state somewhere at the outset and also in regard to future research that the fern-structure of the ice shelf may have additional bearing on the problem. In this case, the "touching of the top" is by weak, crushable firn. Also parts of the ice shelf that are in snow accumulation areas will have rift tops that are being actively filled with new material. Dealing with this is far beyond the scope of the present paper, but it worth identifying as a factor in future investigation.

Agreed. In a revised manuscript I have now explicitly mentioned the role of firn and its approximation in my model. I found the most natural place for this clarification to be at the beginning of the discussion section.

I am very impressed with the fact that observations, specifically (1) the absence of seismicity at rift tips, (2) the failure of a wave-forced propagation of a Nascent rift, and (3) the view of the compressive arch by Doake, are so nicely explained by the simple analysis of the theory presented. This, to me, is a great success and one which suggests that this approach may be what breaks any "log jam" over how rifting on ice shelves is to be pursued in the future.

Thank you!

Around line 30 of page 2. I wonder if a citation to a paper by Sanderson would be appropriate. He thought about ice-shelf margins. Journal of Glaciology, V22, 1979.

Sanderson was previously cited in my manuscript but not on this point. I have added this reference at this location as suggested.

Is equation 2 the bending moment due to the stress balance at the ice front that leads to a bending moment? Just a comment would suffice.

Yes, that's right. I've tried explaining this slightly differently in the text.

In the discussion along with Figure 1, it may be useful to point the reader to observational studies of rift walls: e.g., Scambos, T., Ross, R., Haran, T., Bauer, R., Ainley, D., Seo, K., . . . MacAyeal, D. (2013). A camera and multisensor automated station design for polar physical and biological systems monitoring: AMIGOS. Journal of Glaciology, 59(214), 303-314. doi:10.3189/2013JoG12J170 Note figure 8 in that paper.

I'm grateful to the reviewer for pointing out this reference. I've added a mention to it.

In describing the model, I think it is important to state whether a firn layer is going to be treated or not.

I do believe that treating the firn layer is beyond the scope of the present paper. Since the firn layer may nevertheless be of importance, I have added text stating this point at the beginning of Section 3.1 as well as in the Discussion section.

Also, although a minor point: I wonder if it is worth mentioning that brine-infiltration, horizontally along the bottom of the firn where it is permeable and where there is an ice front or rift wall might introduce secondary effects on rift wall bending moments etc..

I have made additional note of this process (start of Section 5).

page 9 just below line 185. f is given to 4 significant digits. I wonder if this could be considered misleading. I also note that the Young's modulus that is used in the study is expressed as if it were very accurately known. My understanding is that relative sizes are more likely to be significant in terms of what readers take away from the comparison t this point in the paper. Perhaps that should be stated.

I have changed this mistake. Significant digits are now consistently reported.

Figure 5, and some of the preceding figures. Do these results present the solution of Equations 6 7 and 8? I'm confused as to the specific process required to generate the curves and 2-d plot of displacement and other factors. A simple summary (before the results are presented) that describes how the model is implemented would be helpful to other researchers. Oh Dear! I see that this is all explained in the Appendix. (I should have noticed!) But, if my confusion (missing the reference to the appendix) can be of service in improving the exposition, let it so be.

Yes, this is useful. Again, as noted in the first comment above, I have made a significant effort to clarify the exposition.

Response to Reviewer #2

- What is the value the author uses for the critical stress intensity factor Kc? In the text, there is no explicit value given.

Following the work of Rist et al (2002), I use 100kPa m^{1/2}. None of the results in the manuscript are sensitive to this precise value. I have added text at the end of Section 3.2 that provides this information.

- Could the author please include the text of both appendices in the main text? The points that are discussed there are critical for the comprehension of the paper.

Yes, I have moved the text in the Appendix to the main text.

There is no reference to the Appendix and it is not directly clear, for instance, why the author uses the displacement in one direction only dependent on one critical stress factor of a certain loading mode (displacement direction method).

It is simply a matter of definition that each mode depends on an orthogonal component of displacement. This definition, however, bears utility in its relationship to fracture propagation. I have modified the text in Section 3.2 following the definitions of the stress intensity factors to explain this point.

The author can also shortly discuss that this method/approximation has first-order accuracy.

I have made such a note.

- At the moment it is also not clear which equations the author uses for the numerical finite element and which only for the analytical solution.

Following on the comments from Reviewer #1, I have made changes to the manuscript in an effort to improve clarity on exactly this point. I have added the exact equations that are solved in Section 3.1. I have also added text at the beginning of Section 3 that clarifies the modeling approach.

For instance, Eqs. (1)-(3) are only used for the analytical solution. Is this right?

Yes, and I have added a note to this extent in Section 3.1.

For the numerical solution, the displacement field can be derived with three-dimensional elasticity and the different boundary conditions and then in a post-processing step the stress intensity factors are computed out of the displacement field. Then the author should mention this procedure in the text that it is directly clear for the reader. Maybe it is then also better to solve Eqs. (6)-(8) for the stress intensity factors: KII(z)=: : :.

Yes, that is exactly correct. I have added a note that clarifies this point in the introduction as well as at the beginning of Section 3.

- Does the author also consider rifts that are not filled by water? The water cannot percolate in all rifts occurring in an ice shelf, for example, if the rift is too far away from the ice front also dry (not filled by ocean or melt water) rifts can exist. How is the stability of dry rifts? Maybe the author can also add a short comment on these studies in the text.

This is an interesting point and I have made mention of it at the beginning of Section 5.

- Figure 3: the arrows for mode II should also be plotted at the rift edges as the author did it for mode I and mode III.

I've made this change.

- What is the minimum element size along the rift? Did the author a mesh convergence study to also verify that the results are not mesh dependent (a crucial check if one would consider stress intensity factors at the crack tip).

Yes, this is an important point. I did verify that the results are not mesh-dependent prior to initial submission. The maximum element size near any boundary, including the rift tip, is constrained to be no greater than h/16 with ice thickness h. Furthermore, stress intensity factors are measured over several elements, an essential aspect of mesh independence. Although some of these points were already described in the text, I have added more detail in the newly-created Section 3.2.2.

Specific comments and questions:

- Eq. 6-8: I do not have access to the Tada et al. 2000 paper, but are the factors in these equations right? I found in Gupta et al. 2017 ("Accuracy and Robustness of Stress Intensity Factor Extraction Methods for the Generalized/eXtended Finite Element Method") sqrt(r/(2*pi)) and mu/(4-4*nu). Could the author please check the equations.

I'm grateful for the reviewer's attention to detail for catching this mistake. I verified that these equations were correctly implemented in my finite element calculation. It appears that this mistake was entirely limited to the manuscript and the appropriate correction has been made.

- Equations: Why do the author sometimes use an equality sign and sometimes the sign for identical statements with three strokes above the other (see Eqs. (2) and (3))? For example, Eq. (1) and Eq. (4) are both statements how the stress component or the stress tensor for the boundary condition is computed.

I use the symbol with three lines to denote a definition. I've checked that this is consistently used in all equations and also made a note for the reader.

l.58: the traction boundary condition should be zero (stress-free boundary due to zero pressure) at the top of the ice shelf. The author only gets non-zero values as the simplified assumptions of Weertman and Reeh are used. Here, a comment that these results are not necessary for the finite element formulation could be helpful for the understanding of this paper.

Yes, agreed. I added a sentence clarifying this point in Section 3.2.1.

l.93 and Fig. 2: The geometry of the rift in the figure looks like a rhomb, but in the text the rift is described with a uniformly 10 m width and only near the rift tip it is tapered. Could the author update the figure that it fits to the description of the rift? The author already states in the caption that the width and shape are exaggerated but if the author could also include the width of only 10 m in the figures, it will be clearer that LEFM could be applied where an infinitesimal small crack tip is absolutely necessary.

I have updated Figure 2 as the reviewer suggests.

l. 98: Is the perturbation stress tensor the deviatoric stress tensor? Can the author also include the word deviatoric to make it directly clear for everyone and maybe add at the end of the sentence "times identity tensor"? Eq. (4): Why is the pressure boundary condition only applied for the deviatoric stress tensor and not as common to the total (Cauchy) stress tensor?

Briefly, no, it is not the deviatoric stress tensor. The perturbation tensor is T- p_0 whereas the deviatoric tensor is T-p. As this is an important point, I explain the difference in detail in the beginning of Section 3.1. The perturbation tensor still allows for elastic compressibility whereas the deviatoric stress tensor does not.

Figure 3: Could the author please add a legend to the plots A, B, D, E? Could the author please also use capital letters for the reference to the figures, see for instance l.154,155.

I have made these changes.

Why does the author choose slightly different density values of 0.9 (l.55) and 0.89 (l.263)?

This was an oversight. In the revisions I've opted to use the value with fewer significant figures to reflect the uncertainty in this quantity.

l. 265: What are the boundary conditions for the case studied in Fig. 3? At each boundary water pressure?

I have clarified the figure caption to state that this figure is drawn for a marginal rift in a floating ice tongue.

Why are all computed values of this geometrical factor negative in Table A1 (2D and 3D)? Is this due to the boundary conditions acting in an embayment?

Yes, this is for an embayment geometry. I have added this clarifying point to the text.

Does this statement mean that a rift longer than 217m will never be stable in a free-floating ice shelf?

This comment prompted a change to how stability is evaluated in my manuscript. In the revised manuscript, I have introduced the optimally oriented stress intensity factor. I have also removed this calculation as I do not believe it is consistent the three-dimensional results.

Fig. 1: The author should add in the caption of this fuigure that the boundary conditions on the side of the ice shelf are too far away to have an influence on the rift. If this is not the case, then the bending moment of the water pressure at the side counteract the closure of the rift top by the opening of the rift.

I have made such a note.

Fig. 4: Why are the orange and blue curves for alpha»0.5 not reaching or converging to the red curve? The boundary conditions of free slip or water pressure are in this case far away from the rift and therefore the difference of all three cases should be minimal.

This is a real effect. An essential aspect of LEFM is that distant boundaries may still alter energy release rates. This statement is epitomized by the J-Integral of Rice (1968), which expresses the energy release rate as an integral over all boundaries.

Fig. 6B: Shows the red curve in this figure not an unstable rift if alpha is in between 0.1 and 0.3? For the stress intensity factor of the shearing mode (Mode-II) it is sufficient for rift propagation that the magnitude is greater than 1 (cf. l. 228).

I have revised the previously-numbered Fig 6. It and previously-numbered Fig 4 both now show the optimally-oriented SIF. As described in the text Section 3.8, the optimally-oriented SIF provides a better measure of stability. More directly to the reviewer point, I have also modified the discussion in (newly-numbered) Section 4.2 and 5.1 to give a more subtle description of the regimes of propagation.

Technical corrections. I have addressed each of the small technical corrections brought up by the reviewer.