Reviewer 2

I enjoyed this case study, which looks at the development of rifts on the Brunt Ice Shelf. The authors present a very nice suite of in-situ and satellite observations to track rift development. There is perhaps a missed opportunity to explain rift propagation through space and time beyond being generally dependent on "ice shelf geometry and degree of contact with a pinning point". For example, how can understanding the Halloween Crack formation be applied to rifting at other ice shelves?

I have difficulty appreciating high value in the inverse modelling time slices. Given the excellent observational package, the additional modelling neither expands the time/space coverage of the study, nor yields additional process-level insight. Some specific challenges with the modelling:

1) Using shallow continuum mechanics while ignoring fracture mechanics. There is clearly a lot of energy going into fracture rather than deformation here. Stresses are also changing over short length scales, meaning non-trivial coupling stresses.

2) Characterizing the rheology of a floating ice shelf: With no basal drag, is it possible that ice shelves are rather low deviatoric stress environments? (Pettit2003; http://doi.org/10.3189/172756503781830584).

3) By prescribing rift locations, it is difficult for the model to provide independent insight on rift processes. The inferred changes in fluidity surrounding the prescribed crevasses do not seem physically based. Or are the authors suggesting ice properties like viscosity have actually changed ~10 km from the rifts?

Where diagnostic modelling could be helpful is assessing local strain rates and principle stresses. This could provide insight on whether the initial fracture was flow perpendicular (i.e. pure Mode 1 opening) or not flow perpendicular (i.e. additional Mode 2/3 thrust/shear fracture). With the ice rumple in play, virtually any combination of mixed mode fracture is conceivable (Colgan2016; https://doi.org/10.1002/2015RG000504). The offset between principle stresses from rift orientation that would provide this insight, which might be the most applicable diagnostic modelling pursuit.

We thank reviewer 2 for a further thorough and constructive review which will assist us in improving the work and the manuscript. The specific comments are addressed below. In common with reviewer 1, the review highlights parts of the methods section that require clearer or deeper explanations, which we will achieve by rewriting the methods section paying particular attention to areas which were not clear to reviewers, and by integrating parts of the Supplementary material texts back into the main manuscript. The main concern raised by reviewer 2 concerns the modeling. We take onboard their suggestion that analyzing the offset between principal stresses and rift orientation would provide additional insight, and will undertake this extension of the modeling and include it in the revised manuscript. This, *combined with the literature review in section 2. Study area will hopefully provide further insight into rift behavior through time. In response to the specific comments about the modeling, We do not believe we ignore fracture mechanics. We manually insert a rift into the ice shelf geometry, but this isn't inconsistent with any basic tenet of fracture mechanics. It is consistent with the concept of Griffith energy balance, where energy release during fracture growth goes into creating new surface energy. We didn't attempt to physically model the growth process, just the rift-ice shelf interaction once the rift has formed. St. Venant's principle in continuum mechanics leads us to believe that a 50 km rift exerts changes in the stress field over ~50 km, so we believe that microcracking that could alter an effective damage parameter over that scale is reasonable.*

Line 29: A range of factors influencing rift propagation are mentioned, but ice properties (i.e. meteoric versus marine ice and/or damage history) seem overlooked in this listing. Presumably both could be important for the Brunt Ice Shelf.

This list should be exhaustive in terms of factors influencing rift propagation, we will therefore add citations to publications detailing the impact of ice properties e.g. Borstad et al., 2017; McGrath et al., 2014; Kulessa et al., 2014.

Line 105: The reader would benefit from seeing the ATL03 product plotted along the ATL06 product for an example rift. It remains somewhat unclear why the algorithm looks for elevation gradient inflections in the ~200 m spatially averaged ATL06 product instead of elevation thresholds in the ATL03 product.

On L 110 we refer the reader to Wang et al., (2021) Figure 1c which provides a comparison between the ATL03 and ATL06 products for a rift in Amery Ice Shelf. They state that "the edges of those transverse fracture features can be effectively captured from the ATL06 data despite its reduced spatial resolution". Rifts are the largest such features on ice shelves, further negating the need to use the higher spatial resolution product. The ATL03 product also contains abundant non-signal photons. As Wang et al., (2021) have done a thorough comparison of ATL03 and ATL06 for a range of ice shelf fractures, and we do not do the same for HC, we feel it is appropriate to refer the reader to their publication for this comparison.
The ATL06 product is calculated from 40 m segments of ATL03 photons by iteratively selecting signal photons, with adjacent ATL06 elevations separated by ~20 m (i.e. there is overlap between adjacent 40 m segments). The rift measurement algorithm works on 200 m (along-track) segments of ~10 ATL06 elevations, calculating the slope using a line-of-best-fit. By using the ATL06 product, we greatly reduce data volume, and do not have to design an algorithm to distinguish between signal and noise photons (which would replicate something already carried out by the ATL06 algorithm), however, as Wang et al., (2021) showed, sufficient resolution is preserved to effectively capture large-scale fracture features.
We use the steepest slope method as we believe it is the most reliable way of locating the rift walls in order to measure their along-track separation and ultimately rift width perpendicular to the large-scale rift axis. We decided against a rift measurement algorithm based on the separation of points under a threshold because of the variability in rift depths (absolute and

proportional) due to ice mélange thickness (which can be very thick in old, refrozen rifts), and the variability in mélange topography within a single rift.
However, we do acknowledge that a lot of information regarding the methods is relegated to the Supplementary material (as the reviewer points out at the end of this review). So that the reader can gain a deeper understanding of the method, we will expand this section using material from the Supplementary material, and explicitly deal with the issues raised by the reviewers with regards to the methodology.

Line 120: It is unclear what “below 50% of this” means in terms of an elevation. If the mean ice shelf elevation is 200 m, for example, does this mean 100 m elevation threshold?

We applied a 10 km running mean to the ATL06 data to create a smoothed ice shelf surface, which we compared to the ATL06 elevations. The running mean is very similar to the ATL06 elevation across flat portions of the ice shelf, but differs where the running mean smooths out rifts. Any ATL06 elevation that is less than half of the corresponding running mean elevation is identified as potentially being within a rift (i.e. if the running mean elevation at a given location is 200 m, if the corresponding ATL06 elevation was <100 m it would be identified as potentially being within a rift). From there we search outwards in both directions to identify the rift walls (or discard the detection). This threshold was found to be sufficient to mostly identify the Halloween Crack. Ultimately we hope to implement a more advanced rift detection algorithm when we move to the ice sheet scale (the paper aims to validate the rift measurement part of the workflow). We acknowledge that there are issues regarding the clarity and completeness of the description of the methodology, and will aim to resolve them by rewriting this section, paying particular attention to the points that were highlighted as unclear, as well as expanding using material currently relegated to the Supplementary material.

Figure 2a: The inset is too small, perhaps it should be its own figure? More generally on Figure 2, there should probably be a scale bar in each subfigure, given the number of spatial scales. I think that 20 sub figures are too many sub figures for a single figure. I’m also not sure if both the red and blue subset areas are needed, given their overlap.

This figure was expanded in the final round of edits prior to submission to include subfigures c and m-t to include the calving of iceberg A-81 from the Chasm-1 rift. In earlier versions the subfigures would have been ~50% larger. Evidently this expansion has reduced the readability of the figure. For the revised manuscript we will investigate ways to increase the size of the inset, the size of each subfigure and reduce the number of subfigures. This could include making the inset a subfigure, dividing the figure into 2 or more figures (one figure comprising inset and a-d, a second/second and third comprising the historical subfigures of Brunt advance/Halloween Crack calving cycle, and contemporary subfigures of Chasm-1 growth and damage to the area near McDonald Ice Rumples), Blue and red subfigures could also be merged.

Figure 3: It is unclear how the rift centerlines are determined. It is not precisely centered in this figure.

The dashed line in Figure 3 does not precisely trace the rift centerline, rather is an indication of the large-scale rift orientation (we refer to it as the "large-scale rift axis"). It traces the large-scale shape of the rift, but not the smaller-scale meanders. It is used as a reference with which to rotate the ICESat-2 track oriented rift measurements into rift-perpendicular geometry. We note in the supplementary material the effect that small-scale meanders not captured in the large-scale rift axis have on this correction (supplementary Figure 6). Whilst we aim to automatically calculate the large-scale rift axis as the project progresses and we move towards the assembly of an Antarctic Rift Catalog, at present for the purposes of validation of the rift measurement algorithm, we manually define the large-scale rift axis. We will expand the text at L 130 to explicitly state that the large-scale rift axis is manually defined.

Line 131: A sentence is needed introducing what block-bisected rift is.

This point was also picked up by Reviewer 1, we respond in greater detail there, but to reiterate: we will introduce block-bisected rifts and explain the importance of the difference between "opening width" and "wall-to-wall width" here in the text, and investigate including Figure S4 as an additional subfigure of Figure 3.

Line 159: It should be explicitly stated how velocity azimuth is determined in the Gardner2020 (https://doi.org/10.5067/6II6VW8LLWJ7) product. Many satellite-derived products are simply displacements projected down the direction of steepest surface slope of a DEM, which can make azimuths dependent on DEM choice rather than an independent 3D solution.

We will refer back to the Gardner et al., (2020) manuscript in order to address this point, and if necessary include a statement to the effect that this is a major source of uncertainty in the velocity product, and therefore a source of uncertainty to the ice shelf spreading correction to the calculation of Halloween Crack opening rates from the pairs of GNSS receivers.

Line 192: This paragraph sounds more like methods than results.

We agree that this introductory paragraph to the section on the historical behavior of Brunt Ice Shelf from satellite imagery and previously published front positions reads more like methods. We will cut this down to a minimum and integrate with the following paragraph (from L 201), and move the bulk of this paragraph to Section 3.2 Satellite Imagery (L 136-144).

Line 208: Finding that the Halloween Crack formed in the same locations in the 1968 and 2016 is perhaps a very important, but currently downplayed, finding of this study.

The observation of repeating rifting at the location of Halloween Crack/1968 rift is consistent with rifting on Brunt Ice Shelf being driven by the internal stresses generated by the flow of the ice shelf into the pinning point at McDonald Ice Rumples. We thus show that the rifting and calving observed on eastern Brunt Ice Shelf is part of a multi-decadal calving cycle. Antarctic calving cycles are long, and we acknowledge that relatively few observations such as this exist.

We will re-examine the relevant literature to ensure this observation has not been made previously (and if it has, cite the relevant publication at this point), and if not we will ensure that this finding is properly highlighted in the discussion/conclusion/abstract.

Table 1: What is the "Stancomb-Wills Ice Tongue" header meant for here? Also Line 219 says the total ICESat-2 rifts is 375, not 380, as shown here.

The "McDonald Ice Rumples" and "Stancomb-Wills Ice Tongue" labels in Table 1 are intended to orient the reader as the RGTs are listed from the western (MIR) to eastern (SWIT) tips of Halloween Crack. See supplementary Figure 2. We will expand the caption to read:

"The number of times HC could be manually identified in ICESat-2 ATL06 data spanning 2018-10-14 to 2021-07-15, compared to the number of times it was found and measured by the rift detection and measurement algorithm. RGTs are listed from the western ("McDonald Ice Rumples") tip of HC to the eastern ("Stancomb-Wills Ice Tongue") tip (Supplementary Figure 2). The RGTs highlighted in bold are used in the validation of the rift measurement algorithm. The five errors are rift width underestimations caused by semi-detached ice blocks bisecting the rift resulting in a pair of troughs in the ICESat-2 data, with all points within the narrower trough being flagged as low quality"

The disagreement between the caption and L 219 is a result of the 5 underestimations caused by not including the narrow part of a bisected rift due to data quality issues as described above. The algorithm produced 380 measurements, of which 5 were underestimations ("errors" in Table 1), leaving 375 "successful" measurements. So that the text and table agree, we will rewrite L 219-221 to read something like: "Our rift measurement algorithm produced 380 measurements of HC width along 17 RGTs (Table 1, Fig. S2), of which 375 were successful and five were underestimated by ~100 m as a result of a block dividing the rift and all ATL06 measurements within the narrower trough being flagged as low quality".

Line 231: If the reader has perhaps forgotten which are the high/low power lasers, an explicit statement here saying whether laser power influences retrieval ability would be helpful.

Thanks for the suggestion! We hadn't considered the differences between the strong and weak beams. We'll look into it!

Figure 4: It seems unnecessary to include the ICESat-2 launch as a vertical dash in all sub figures. It seems asymmetrical as Worldview and Landsat satellite launches are not highlighted.

The ICESat-2 vertical dashed line could be removed. Its only purpose is to show that rift initiation pre-dates ICESat-2 launch by ~2 years, accounting for the lack of ICESat-2 rift width measurements in the early part of the record.

Line 270a: I understand these opening rates are given relative to ICESat ground tracks, but I would appreciate a clear statement on the minimal (?) of rift advection on apparent opening rates. For example, a rift with non-uniform width – even if it is not opening or closing – could still yield width changes across a given ground track as it is advected across that given ground track.

We agree that we measure the rift with ICESat-2 and satellite imagery in a fixed, Eulerian reference frame, whereas the GNSS receivers and the rift are advected by ice flow (Lagrangian reference frame). The repeat measurements from the satellite platforms therefore do not measure the same portion of the rift on each pass, and the rate of opening they measure is a combination of the opening rate and some apparent opening or closing rate that depends on the offset of the ice flow direction and shape/width of the rift. Ice flow at Brunt in the vicinity of HC is largely east to west and parallel to HC (and therefore considerably offset from the ICESat-2 tracks). Given this possible source of disagreement between satellite and GNSS measurements (and by extension, true rift opening rate), we modeled the opening rate of a rift using flow law parameters n = 1, 2, 3. The model rift was open on the McDonald Ice Rumple side (where ice damage is greater), and was tuned such that the modeled and measured rates of opening agreed at the location approximating the western GNSS pair. We extracted opening rates at locations along the rift approximating the validation locations, and locations 1 km upstream (accounting for the 700 - 1000 m/yr ice flow speed). In all cases the difference in opening rates over 1 km was < 10 m/yr. This is shown in supplementary figure 15. We conclude from this that the difference in opening rates due to the differing reference frames is small over the short timescales we are considering. We acknowledge that this may not be the case for other rifts, for example where ice flow is faster, or rifts are broader wall-to-wall but shorter tip-to-tip. At present this is mentioned briefly in the discussion (Section 5.1 Validation of Rift Measurement Algorithm, L 324-329), and the figure caption of supplementary figure 15. We are of the opinion that it is appropriate for supplementary figure 15 to remain in the supplementary material, but we agree that there is scope to mention this where we show the agreement between the 3 datasets (results Section 4.7.3 Rift Opening Rates), and expand on the discussion of this in Section 5.1.

Line 270b: The observation that rift opening rate appears to briefly slow down after calving is interesting. Does this imply that iceberg A-74, while still attached to the Brunt Ice Shelf, exerted a net extensional stress on the ice shelf? Or is there possibility a kinematic wave at play?

We believe that the reduction in the rate of Halloween Crack opening results from the reorganization of ice flow which occurs following calving of iceberg A-74 from North Rift. The removal of ice flowing to the north of McDonald Ice Rumples led to an anti-clockwise rotation in ice flow of the nascent iceberg between North Rift and Halloween Crack, reducing the difference in the rift perpendicular components of ice flow across the rift. We believe a kinematic wave would be too slow to account for our observations.

Figure 6: It is difficult to discern the different shapes of the small markers.

We presume this comment mainly pertains to Subfigures c & d which show the opening rate from optical satellite imagery (Landsat/triangle and WorldView/square), and by extension could *apply to Figures 4 and 5. We initially used black edges for these markers, but this did not improve clarity. We will attempt to improve the clarity with different marker types and/or larger marker sizes, although we are unsure whether alternative marker types will provide the necessary clarity, and larger markers would increase the overlap between adjacent markers. Ultimately, the distinction between the different satellites is not of great importance.*

Line 285: The motivation and framework of the simulations should be described earlier, in methods. Indeed, much of the model description should probably be moved from supplementary to the methods, including ice temperature/viscosity assumptions and how they interact with fluidity.

We acknowledge that L 284-288 and L 289-291 are descriptions of the modeling carried out and it would be appropriate to move them to the methods section. Whilst the methods section discusses the basics of the model and the three time periods ("pre-calving", "calving", "post-calving"), it does not go into detail on the model runs performed, in part because these are based on the results from the other parts of the paper. We will move and integrate L 284-288 and L 289-291 and additional material from Supplementary material to the methods section in the revised paper, and edit down L 284-288 and L 289-291 in the Results section to only reiterate what is necessary for the reader to understand the results being presented.

The supplementary material contains many great figures, but I wonder if much of the supplementary text could be worked into the main methods for ease of the reader? At 484 Lines, this manuscript seems to have space within the word limit.

We have addressed this point where relevant in the review. We will endeavor, within the word limit, to include additional information on the methodology and modeling which is currently in the Supplementary material in the main body of the manuscript.