Reviewer 1

Manuscript Review

~~“Measurement of Ice Shelf Rift Width with ICESat-2 Laser Altimetry: Automation, Validation, and the behavior of Halloween Crack, Brunt Ice Shelf, East Antarctica”~~

“Ice Shelf Calving due to Shear Stresses: Observing the Response of Brunt Ice Shelf and Halloween Crack to Iceberg Calving using ICESat-2 Laser Altimetry, Satellite Imagery, and Ice Flow Models”

Ashley Morris, Bradley P. Lipovsky, Catherine C. Walker, and Oliver J. Marsh

Summary:

Morris et al. present a new method for measuring Antarctic rift widths using ICESat-2, which they validate using optical satellite imagery and GNSS data for Halloween Crack on the Brunt Ice Shelf. They determine opening rates from repeat measurements of the rift width. They compare these to opening rates derived from other observational sources and then to ice-shelf velocity data via data assimilation into a shallow shelf ice flow model. They show that their ICESat-2-based algorithm can successfully measure rift widths and opening rates and is a tool that can complement existing, optical imagery methods. They use this data to describe the recent evolution of the Halloween Crack, and suggest that the Brunt Ice Shelf geometry and contact at a key pinning point determine the evolution of the rifts, in agreement with existing work, and support this with digitised historical data on the Brunt Ice Shelf.

Overall Comments:

I enjoyed reading this paper and being brought up to date with the latest observations of rifting on the Brunt Ice Shelf. I can clearly see the benefits of this approach (although I think you could make those clearer, earlier on in the manuscript). You have done a thorough job in validating your new method, and I particularly liked the work of collating historical data to put recent events into the full context of Brunt’s calving cycle. The work was well-referenced throughout, and I particularly liked the extensive links to secondary literature around line 30 which will allow the reader to follow up on Antarctic rift studies.

I recommend that the paper be published following revisions and after addressing the points laid out in the rest of this review.

My main concerns relate to 1) The details and clarity of explanation of the new algorithm and the methods used; and 2) Questions about the necessity of the modelling approach and interpretation

1) I think it could be made much clearer in the methods section what exactly is new about this algorithm, and what was done in previous work. This is the central contribution of the paper as I understand it, so I think this needs to be made more

explicit – together with more detail and clarity on exactly how the algorithm works (see Specific Comments section) – and less on the technical specifications of the satellite used.

2) There are details about the modelling setup and approach that are not clear from the text (see Specific Comments). But I also am unsure about the need for inverse modelling to examine ice-shelf velocities and opening rates as opposed to using the ice velocity data directly. As you don't analyse the stress field I don't see why the velocity data needs to be assimilated into an ice flow model. I would like to see an explanation for why this was done in the text, or for the velocity data to be used directly instead in the analysis.

We thank reviewer 1 for their careful reading of the previous version of the manuscript and their constructive comments which we have tried to take onboard in the revised version of the manuscript. We respond to each of the specific comments below. We have rewritten large parts of the methods section, particularly those focusing the rift measurement algorithm and the ice flow modeling. We have removed unnecessary detail about the ICESat-2 satellite and addressed the parts of the description of the rift measurement algorithm which were not clear to the reviewers. We have clarified that it is not a modification of an existing method, and that some thresholds etc. are based on our own testing and development. We have added more detail about some aspects, for example the quality filtering and division of the rift into two troughs by (semi-)detached ice blocks. This gives the reader the information they need to understand the method, with further details remaining in the supplementary material.

We also took onboard the criticism and suggestions of both reviewers concerning the limited scope and value of the ice flow modeling. We expanded the modeling to include glaciological stress fields as suggested, and this has led to deeper insight. We have rewritten and expanded the methods section on the ice flow modeling, with further details including equations available in an expanded section in the supplementary material. The greatest changes are to be found in the results and discussion sections concerning the ice flow modeling, and the conclusions. We discuss the rifting and response of Halloween Crack to North Rift calving in terms of the stress fields derived from the pre-calving, immediately after calving and after-calving models, and include a new section on the need to incorporate shear stresses into ice shelf calving laws. In all, the limitations identified by the reviewers and the improvements suggested - particularly with regard to the ice flow modeling - have significantly improved the manuscript.

Specific Comments:

L 4: I don't think the part about this being part of a larger effort is required. Suggest removing.

The Abstract has been rewritten to focus on the important aspects of the manuscript and the new focus on the stress field and calving process. The aspiration to make a continent-wide rift catalog is now only mentioned in the Conclusions section.

L 20: grounded ice speeds only change when the shelf ice that is lost is providing sufficient buttressing. The calving or thinning of passive ice areas will not result in a change in grounded ice (e.g. Fürst et al., 2016; Reese et al., 2018)

The wording has been altered to clarify that a change in ice shelf flow speed will only occur as a result of calving or thinning of areas of ice shelf that were providing buttressing. The Reviewer is thanked for their citation suggestions which have now been included.

L 47: Suggest removing this final sentence (from "Greene et al. (2022)" onwards) – it repeats a point made on L19 which doesn't need reiterating here.

The repetitive sentence has been removed.

L 53: I'm not sure what seaward-landward offset means without referring to the reference. Please provide a short definition here.

We have included a brief description of the "seaward-landward offset" and continue to refer the reader to Walker and Gardner (2019) for further information.

L 83: This needs to be clearer than saying "both directions" – as all directions are possible.

This sentence was supposed to convey that the Halloween Crack did not originate from McDonald Ice Rumples and propagate solely upstream, but originated ~15 km from MIR and propagated both towards and away from the ice rumple. We have rewritten the sentence to clarify that the rift propagated in both easterly and westerly directions from the onset location.

L 95-110: I think these two paragraphs can be nearly entirely removed, and replaced with references to the technical specifications of ICESat-2. The key information we need is that you use the ATL06 product and some information about the temporal and spatial resolution of that product.

We have removed a lot of unnecessary detail on the technical specifications of ICESat-2 and instead give a brief description that is necessary to understand the manuscript, and point the reader to relevant literature for additional information.

L 112-115: Is this a method that you used/adapted? If so, then you need to say more about how you used this method and why you chose it/how it works. If not then you do not need to go into this level of detail.

We take a different approach to the one taken by Wang et al., (2021). To avoid the reader making the assumption that our method is based on/related to the work of Wang et al., (2021), we removed the description of their method and instead refer the reader to the Wang et al., (2021) manuscript along with other relevant literature.

L 120-121: Why did you choose these parameters to filter your data? Are they recommendations from the ATL06 product manual, or previous studies? Or from your own testing? Please clarify here.

Filtering is necessary to remove sections of data with erroneous height measurements or data gaps. We found the ATL06 quality flags to be incompatible with our methodology, as (apparently) valid height measurements in the area of rifts were often flagged as low quality. The filtering employed is not something recommended by the ATL06 product, or based on previous studies, but is something we developed through testing. We have added a sentence here to provide further details about the filtering and confirm it is something that we developed.

L 122-126: I have reread this sentence/paragraph a number of times and I'm still not clear on this method. With the shortening of earlier parts of this section, you could go into more detail on each step in your method here. I think this is necessary as the algorithm is a key novelty of the work you present in this manuscript.

Both reviewers raise the point that the description of the methodology for measuring rift width is not sufficiently clear. We have re-written the description here and added greater detail.

L 132: Could you explain here why you need to differentiate between "wall-to-wall" and "opening" width here? Fig S4 nicely shows how they are different but it would be good to have an explanation of why it matters here when you introduce them.

We differentiate between "wall-to-wall width" and "opening width" due to the presence of icebergs, peninsulas/semi-detached icebergs, and, in the early stages of rift formation, 'bridges' formed by adjacent sections of rift which have not opened sufficiently to lead to the detachment of these blocks from one or both rift walls. We continue to refer to two supplementary figures (S5, S6) to give the reader a better understanding of this. We do not bring these figures into the main manuscript for reasons of space. We have added further detail to this section to give the reader a better understanding of the difference and why "opening width" is the appropriate measurement for comparison with GNSS receiver separation.

L 138: You need to introduce the RGT acronym in the main text here (or before this point in the revised section 3.1)

RGT (Reference Ground Track) is now specified at the first mention in the main text. Thanks to the reviewer for pointing out this oversight.

L 140: What is the spatial footprint of a pixel here – so we can have an estimate of the magnitude of the error in metres?

We have included the size in metres of the 2 pixel uncertainty estimate at this point, to give the reader a better appreciation of their size.

L 170-174: This is a very brief summary. There must be more parameter choices informing your optimisation of the fluidity field – an initial guess for the fluidity field, an error field for the velocity observations, and some parameters related to regularisation? You could briefly outline the choices you have made here – and point the reader either to your source code (which is very helpfully attached, thank you!) or to a fuller explanation in the supplementary file.

We acknowledge that the description of the ice flow modelling was too brief. We have re-written and expanded the details in both the methods section and the supplementary material

L 176: Again, how did you decide to smooth the ice thickness map using the ice flow model? Perhaps you could point to a fuller explanation in the Supplementary here?

The ice thickness map is smoothed to prevent unrealistically high driving stress resulting from spatial thickness changes around features such as rifts and crevasses. We have provided details of the smoothing performed in the supplementary material.

L 178-179: What do you mean by 'defining' the extent of HC and smaller fractures near MIR in the model? Are these treated as 'holes' in the mesh, and if so with what boundary conditions applied? This needs to be clearer.

We have clarified here that we digitised the extent of Halloween Crack, North Rift and other small fractures using contemporaneous optical satellite imagery, which are then treated in the ice flow model as holes in the mesh with Neumann-type boundary conditions.

L221: Not sure what you mean by 'in one part' here?

This refers to the situation where an iceberg or semi-detached block bisects the rift and it is necessary to combine two measurements to calculate the "opening width". The problem seems to occur when the beam is very close to the point where a semi-detached block maintains limited connection to a rift wall, or where an iceberg is very close to one rift wall. In this situation the rifts appears a s a pair of troughs in the ICESat-2 data; one broad, one narrow. The narrow section of the will contain few elevation points. If these points are flagged as low quality, the detection will be skipped. Because the narrow component of the rift is discarded, the "opening width" is underestimated by ~100 m. We have tried to reword this section without going into too much detail, avoiding the confusing "one part" terminology.

Table 1: Do the bold entries signify the RGTs used for validation? This needs to be made clear in the caption. The same clarification relating to 'in one part' applies to this caption as well.

The 6 bold RGTs in Table 1 are those used for validation. We have rewritten the table caption to specify this, as well as improving the description of the situation where the rift is bisected by an iceberg/peninsula/bridge, changing "in one part" to "the narrow trough" of a bisected rift.

Figure 5: The legends in some of these plots cover the data points and error bars. It would be better to position them in the NW corners.

We thank the reviewer for this suggestion and have altered the 3 graph figures (now figures 5, 6, 7) accordingly. This improves their clarity and ensure a consistent layout throughout.

L271-283: This is where I would like some more clarity on the modelling approach. As I understand it you have solved an inverse problem so that the ice sheet model velocities replicate a pre-calving observed velocity field. Why not directly use the ice velocity field to calculate the opening rates? I don't see the need for the ice flow model when only using its inverse capabilities (unless you were looking to analyse the stress field or fluidity field – but here you only look at the modelled velocity components). I can see the use of the diagnostic experiments that you present, but not the use of the outputs from an inversion to compare with observed opening rates. You also state towards the end of this section that the inverse models replicate the general pattern of opening rates – but is this not just because they were tuned to do exactly that by inverting with snapshot velocity fields from 'pre', 'during' and 'post' calving observational data?

The primary reason we perform the modelling is to test the hypothesis that the (variability in the) opening rate of Halloween can be explained by changes in ice shelf geometry. However, it is true that the output opening rates are a result of the input observational data (albeit without the noise present in the feature tracked velocity maps). We have taken onboard the suggestion of both reviewers to expand the modelling to include glaciological stress fields. We have re-written large parts of the results and discussion of the ice flow modelling, and now focus on changes in the glaciological stresses resulting from the calving from North Rift, and its impact on the opening rate of Halloween Crack.

L295: But this 'ice flow speed increase' is not a result from the ice flow model evolving. The speeds in the model following an inversion were determined by the three different velocity fields you used as inputs. Again, I can't see the benefit of using an ice flow model in this way over the velocity observations themselves?

We have added a note that the velocity increase is seen in both feature tracked and modelled ice flow fields.

L349: I feel that both of these statements need supporting references. In particular it would be good to reference those that have looked at ice shelf flow immediately post calving, and if there really are none then to state that with confidence.

This statement has been removed as a result of the significant re-writing and re-framing of this sections of the manuscript.

L359: Could you analyse the changes in the glaciological stresses produced from the inverse modelling and confirm this (along the lines of (De Rydt et al., 2019))? This would be a good use of the inverse modelling you have carried out.

We thank both reviewers for their suggestions on how to make best use of the ice flow modelling performed. We have taken this onboard and expanded the modelling to investigate the glaciological stresses in the ice shelf during the pre-calving, around calving and after calving time periods. The details of this processing can largely be found in the supplementary material. The resulting stress field are included in the modeling figure (now figure 8), with less relevant outputs from the modelling relegated to figures in the supplementary material. We have re-written/expanded large parts of the ice flow modelling *section of the methods, results, discussion and conclusions. We now discuss the changes in the behaviour of Halloween Crack from the perspective of the changing ice shelf stress field, as well as discussing the need to incorporate shear stresses into ice shelf calving law. The reviewers rightly highlighted the ice flow modelling component as the major weakness of the first iteration of the manuscript, and we believe that the additional modelling and re-writes have significantly improved the manuscript.*

L456-459: You introduce some really good points about the benefits of your ICESat-2 rift measuring algorithm here which were not mentioned earlier in the text. I would highlight these points when introducing the methods you used.

We now mention the year-round observational capacity (limited on only by clouds) and the ability to compliment optical satellite imagery in the relevant subsection of the methods section

Technical Corrections:

L6: Insert a comma after "North Rift"

L75: "velocity ~~on~~ **of** the opening…."

L167: "… the response of **the** wider…"

L184: The 'ij' on the first \tau should be subscript

Thank you to the reviewer for highlighting these spelling/grammar/formatting mistakes!

References

De Rydt, J., Gudmundsson, G. H., Nagler, T., & Wuite, J. (2019). Calving cycle of the Brunt Ice Shelf, Antarctica, driven by changes in ice-shelf geometry. The Cryosphere, 13, 2771–2787. https://doi.org/10.5194/tc-13-2771-2019

Fürst, J. J., Durand, G., Gillet-Chaulet, F., Tavard, L., Rankl, M., Braun, M., & Gagliardini, O. (2016). The safety band of Antarctic ice shelves. Nature Climate Change, 6(5), 479–482. https://doi.org/10.1038/nclimate2912

Reese, R., Gudmundsson, G. H., Levermann, A., & Winkelmann, R. (2018). The far reach of ice-shelf thinning in Antarctica. Nature Climate Change, 8(1), 53–57. https://doi.org/10.1038/s41558- 017-0020-x

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 has significantly improved the manuscript. The specific comments are addressed below. The main concern raised is the limited value of the modeling, with the reviewer suggesting this was “a missed opportunity to explain rift propagation through space and time” and that analysis of principal stresses would provide deeper insight. We therefore expanded the ice flow modeling to include an analysis of principal stress orientation. We have significantly rewritten the description of the ice flow modeling in the methods section and expanded the relevant sections in the supplementary material. We have also rewritten the sections of the results and discussion pertaining to the modeling, discussing, as suggested, the rifting in terms of orientation compared to the principal stress field. We also include a new section discussing these results in terms of ice shelf calving laws, suggesting an integration of shear stress is *necessary. As a result of this new framing of the manuscript, we have changed the title and rewritten the abstract to increase the weight given to the modeling aspects of the study. We concur with the reviewer's view that the expansion of the modeling provides deeper insight to the rifting and calving process, and significantly improves the manuscript.*

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.

We have expanded the list of factors influencing rift propagation to include heterogeneous ice properties and the presence of marine ice and added citations to 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.

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.

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 have expanded the sentence here to clarify that we calculate a 10 km running mean surface. This surface approximates the ice shelf surface across flat portions of the ice shelf, but is above the rift walls and sea/mélange within a rift because the rift width is significantly below 1km (except in the case of very large rifts or where the ICESat-2 track is largely aligned with the rift. This is not the case for Halloween Crack, and this method for rift detection will be replaced with one based on Machine Learning at a later stage of the project), thus the running mean surface is above the surface observed by ICESat-2. We found that by setting a threshold such that any ICESat-2 point with an elevation less than half of the corresponding running mean height was sufficient to identify the Halloween Crack.

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.

To improve the readability of Figure 2 we have split it into 3 figures, 2 in the main manuscript (figures 2 and 3) and 1 in the supplement (Figure S2). Figure 2 now comprises 4 subfigures and includes a larger version of the inset, as well as the ice fronts from before North Rift calving, after North Rift calving and after Chasm 1 calving (and the corresponding previous ice front positions). There are now only 2 spatial scales in this figure and scale bars are included in each subfigure. Figure 3 shows the ice front development from Landsat/Copernicus satellite imagery and an aerial photograph. The scale of the aerial photograph is unknown; a scalebar is included in subfigure (i) which is valid for all satellite image subfigures. The development of Chasm 1 and eventual calving is now in the supplement and the previous 'blue' and 'red' areas are 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 (now 4) 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 have clarified in the figure caption and in the main text that this rift axis is manually defined. An automated delineation of the large-scale rift axis will be a goal later in the project.

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

We have added a sentence here to discuss the origin of blocks which bisect the rift, pointing the reader to figures in the supplement. We also give further detail on "wall-to-wall width" *versus "opening width" later in the paragraph, again with reference to figures in the supplement.*

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 could not find this explained in the Gardner et al., (2020), so we added a sentence stating that this velocity field, and hence the correction based on it is subject to errors arising from the projection of the feature tracked velocity field down the steepest slope of the DEM used in their processing.

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 cut down the introductory paragraph to a minimum and moved the bulk of the paragraph to the methods section.

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. We have included a further mention of the similarity in locations of the two rifts in the conclusions section of the manuscript, and mention the similarity in calving events through time in the re-written 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 have expanded 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"

We have also reworded the first sentence of the "Rift Measurement Algorithm Performance" section to remove the 375 vs 380 confusion (380 total measurements of which 5 were erroneous, leaving 375 measurements.

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 will look into it when we expand to continent-wide scale, where will we have far more measurements which will enable us to draw a stronger conclusion.

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.

We have removed the unnecessary ICESat-2 launch data dashed line from figures (now) 5, 6 and 7.

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 16. 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. We discuss this in the discussion of the intercomparison of opening rates.

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 the extended modeling suggested by both reviewers provides evidence that the opening rate of Halloween Crack is determined by the ice shelf geometry and the degree of pinning of the ice shelf at McDonald Ice Rumples through its influence on the glaciological stresses and the ice shelf flow field.

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

We trialed black outlines to points prior to original submission of the manuscript. Larger points did not improve clarity due to increased overlap. We do not believe that the distinction between the different satellites is of particular importance, so we have not made changes to point size or type.

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 the descriptions of the modeling carried out would be more appropriate to the methods section. We have removed these paragraphs and incorporated them either into an expanded and rewritten Ice Flow Model subsection in the methods sections or the supplementary material.

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 tried to reach a balance between including as many of the core results figures and important methodology in the main manuscript as possible, whilst not including too much such that figures have too many subfigures and become too small. Since the previous version of the manuscript we have added a further figure and expanded the methods, results and discussion sections on the ice flow modelling. This unfortunately means that some figures which are important in understanding particular points - for example the figures on the influence of rift meanders and the origin of blocks bisecting the rift – must remain in the supplement.