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”

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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 manuscript and their constructive comments which will improve the revised version of the manuscript. We respond to each of their specific comments below. We will address the main concerns by rewriting the methods section and expanding the modeling work. We will improve the methods section by integrating some of the material currently relegated to the Supplementary material, so the reader gets a fuller picture of, for example, the rift measurement algorithm, without having to refer separately to Supplementary material. We will also take care to address the specific aspects of the methods sections which were highlighted as unnecessary, unclear or lacking in sufficient depth. To properly address the concerns raised by both reviewers, we will expand the modeling to include an analysis of changes in the stress field.

Specific Comments:

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

There was some discussion of this point between the co-authors prior to manuscript submission. The manuscript deals mostly with the validation of the rift width measurement algorithm, and we wanted to make it clear that it would be applied more widely than just Halloween Crack. However, it is probably not necessary to mention this in the abstract. We will remove it from the abstract and if mentioning it at all, do so in a more appropriate place such as the conclusions.

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)

We agree that the current wording of this sentence implies any loss of ice shelf thickness/area will reduce buttressing and increase ice discharge, whereas in reality it is possible to thin/lose ice that is not providing buttressing to grounded ice. We will rewrite this sentence to make it clear that thinning/loss of ice ***providing buttressing*** *results in increased discharge of grounded ice. Thanks also for pointing us towards Reese et al., (2018).*

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.

We will rewrite/restructure this section to remove the repetition on lines L19 and L47.

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

We agree that we should provide a brief description of the seaward-landward offset here, so it is not necessary for the reader to refer to Walker and Gardner, (2019).

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, for example, McDonald Ice Rumples and propagate solely upstream, but originated ~15 km from MIR and propagated both towards and away from the ice rumple. "Both directions" refers to the 'post-propagation' location of the rift tips. However, it would be better, as suggested, to include additional information as to which directions the rift was propagating in.

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 will shorten this section by removing any surplus information about the ICESat-2 platform/instrument/processing/data products etc. leaving only information that is necessary to understand and interpret the work carried out.

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). It is highlighted above the other studies using ICESat-2 to study rifts/fractures because it shares similar aims (producing an Antarctic Rift Catalog versus mapping fracture features on Amery Ice Shelf), but employs a different data processing technique. To avoid the reader making the assumption that our method is based on/related to the work of Wang et al., (2021) we will remove L 112-115, and the interested reader can refer to the Wang et al., (2021) manuscript.

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.

The filtering is necessary to remove sections of data with erroneous height measurements or data gaps resulting from (we think) the presence of clouds, and problems with either the geoid or tide corrections applied to the ATL06 product. We found the ATL06 quality flags to be incompatible with our methodology, as (apparently) valid height measurements in the area of the rift were often flagged as low quality (possibly a result of topographic variation over short distances at the rift wall and within the mélange?), though we do skip rift detections containing a *large proportion of points flagged as low quality. We therefore developed the filtering employed based on our own testing at a number of rifts in the region around Brunt Ice Shelf. We will clarify here that these filters were based on our own testing.*

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 and too much is relegated to the Supplementary material. As noted by the reviewer, the methodology is a key novelty of the work presented in this manuscript. We will therefore rewrite and expand the description of the rift measurement algorithm, ensuring that we properly address things which either or both reviewers found unclear, as well as integrating material currently relegated to the Supplementary material.

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 detachment of these blocks from one or both of the rift walls. It is most appropriate to use the "opening width" for the comparison with GNSS, as the blocks tend to remain in situ due to remnant connections to the rift wall(s) and/or the presence of ice mélange. This is currently detailed in Supplementary Text S3 Merging and Supplementary Figures S4 and S5. We point to Supplementary Figure S4 in the main text on L 132. However, the distinction is fairly important, and so that it is not necessary for the reader to go to the supplement, we will introduce and explain the importance of the difference here in the text, and investigate including Figure S4 as an additional subfigure of Figure 3.

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 acronym is introduced prior to this point in the figure caption of Figure 1, but not in the main text. We will ensure that the acronym is specified on the first mention of 'reference ground track' once other alterations to the text/format of the first sections of the manuscript are complete.

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 will include the pixel sizes (m) for the Landsat and WorldView optical satellite images here, so the reader can better understand the assumed uncertainty in rift digitization.

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 will add more detail to the modeling methods section to elucidate on the inputs and parameter choices used in the models, and point the reader to the Jupyter Notebooks which are included in the 'Model code and software assets'.

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 acknowledge that insufficient details are given in the current text, and will either incorporate details into the methods section of the main manuscript, or include further details in the supplement as the reviewer suggests.

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.

Halloween Crack and smaller fractures near McDonald ice Rumples were delineated using Landsat-8/9 imagery. They are treated as holes in the mesh with Neumann-type boundary conditions. We will include these details in the text of the revised manuscript.

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". This 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 rift appears as a pair of troughs in the ICESat-2 data; one broad, one narrow. The narrow section of rift 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. If the beam is located further from the point of connection of a block with the rift wall, the narrow component will be wider (as a result of the rotation of the block about the connection point as the rift opens, e.g. Supplementary Figure S5), and it is more likely that the algorithm will successfully measure the width of the both parts of the rift. "In one part" here refers to the narrow component of the rift where it is bisected by a block/iceberg and *the geometry is such that that part is narrow. We will improve the wording of this section to make it clear that we are referring to sections of the rift, along the same RGT, that are bisected by a block/iceberg. We will also expand to explain that this underestimation occurs when the beam is close to the connection point of a block/former connection point of a recently detached iceberg, and the trough in the ICESat-2 data is narrow.*

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.

Yes, we use the 6 bold RGTs for the validation, and discuss them in the text (L 227-230), but we do not make clear why they are bold in the table caption. We will expand the table caption to state that these are the 6 RGTs we use in the validation, and reword to "in one part" to make it clearer, as discussed above.

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 will move the legends and the subfigure letters of Figures 4 and 5 to the positions they are in in Figure 6. This will improve the clarity of Figure 5, and the consistency in layout between the three graphical figures.

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 modeling is to test the hypothesis that the (variability in the) opening rate of the Halloween Crack can be explained by changes in ice shelf geometry. (There is a small advantage to using the modeled rather than feature tracked velocity field to calculate the opening rate - the absence of noise, particularly around the sharp discontinuity in velocity at the rift). It is true that the modeled opening rates are a result of the input observational data, but it is interesting to assess whether the parameters required to replicate the observed behavior are physically realistic. We do investigate the fluidity field, finding increased fluidity in the grounding zone of the ice shelf that we suggest represents the opening of fractures in this area as the ice shelf accelerates in response to the reduced buttressing provided by McDonald Ice Rumples following the calving from North Rift. We take on board the suggestion that expanding *the modeling work to include an analysis of the stress field would provide further insights, and we will conduct this work prior to submission of the revised version of the manuscript.*

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 will make it clear in the revised manuscript that the velocity increase and redirection is seen in the feature tracked velocity field (shown in Supplementary figure 16 g-k) and is replicated in the outputs from the inverse models.

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.

We will review the literature and provide references for the statements “Many studies have looked at grounded ice after ice shelf calving” (including some referenced around L 21) and “few if any have looked at the details of ice shelf flow and fracture in the immediate post-calving period”. As pointed out by the reviewer, we can then strengthen this statement to say either “few have looked at the details…(references)” or “none have looked at the details…”

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.

Both reviewers suggest that in its current form the modeling is of limited value, and suggest ways it could be extended to provide more insight. We agree that extending the modeling to analyze changes in glaciological stresses would be a worthwhile extension that we will undertake for the next version of 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 can include the benefits of ICESat-2 measurements of rifts in the rewritten section 3.1 ICESat-2 Data and Rift Catalog.

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, which we will correct in the next version of the manuscript.

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