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.

Specific Comments:

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

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)

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.

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

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

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.

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.

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.

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.

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.

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

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?

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.

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?

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.

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

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.

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.

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?

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?

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.

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.

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.

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

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