

Dear Reviewer 1,

many thanks for your very useful review. We will follow the vast majority of your points in the revised version and give detailed answers to your comments below. We are very grateful that you have put a lot of effort - and time - into giving us recommendations.

Some of your points were also raised by Reviewer 2 and we thus refer occasionally also to our answer to those answers. You will find below 'Done.' as an answer to some of your points. In that case we have directly included your suggestion into the manuscript and elaborate more on it, but this is more for grammar, typos or smaller issues. All other things are answered in detail.

Again, many thanks for helping to improve our manuscript!

Best wishes,

Angelika and all co-authors

I read the paper in the order it is written and at several points found it difficult to understand the results presented because the methods are hidden in the appendices. I would suggest moving the methods into the main text, either as a dedicated methods section or as subsections to your observational results and modelling results sections. If the methods are not moved to the main text, at the very least there needs to be better signposting, or summary methods sentences at the start of the results sections to make it clear what data/experiments were used to make statements in the results. I've included some suggestions in the specific comments below. The paragraph at the beginning of Section 3 is very clear, so perhaps a similar paragraph at the beginning of Section 2 (stating the type/date range of imagery used) would be sufficient.

We have thought lengthy about including the methods in the main text while we wrote the original version and did it now again. It is the number of different datasets and with those methods why we have put it into the appendix. We wanted to tell the story from the past situation, the current changes to the impact in the main part, as we thought readers with very different own background may read it and a modeler might find the details on n instruments/dataset distracting and a scientist from observational glaciology to be distracted by the details on the modeling. If we would use only with up to ~three different data sources, a method section would be our choice, but with this larger number, we still want to keep it in the appendix.

The modelling experiments are well thought out and the second experiment was a useful addition to the first that just removed the region that appears to be about to collapse. However, I did wonder if a third experiment would be interesting, where you remove the entire ice tongue to see what the total/maximum impact of ice tongue loss would be. This would be particularly interesting given that you show that the regions close to the grounding line appear highly buttressed.

Yes, we fully agree that this is an additional interesting experiment and we have completed the simulation and will include them in the revised version. Currently, we are trying to increase the mesh resolution in order to also incorporate Reviewer 2's suggestions. This will also then lead to an overhaul of the figure.

Specific comments

Line 3: I think the link to sea level rise in the future due to ice tongue disintegration could be more convincing here

We agree that this can be improved and will present a better sentence in the revised version.

Line 5: Specify what 'recent changes'

Very good suggestion and will be included in the revised version.

Line 6: Perhaps state when these cracks progress further upstream compared to 2010.

We will include 'since 2016' in the revised version.

Line 17: Rephrase 'since more than a decade', perhaps 'In the last decade, mass loss has reached northern Greenland...'

Many thanks, this is definitely the better wording and we will include this into the revised version.

Line 21: Would be useful to refer to Figure 1 when you state the ice front is in contact with the ice rises. Very good suggestion and will be included in the revised version.

The second half of this sentence is a little vague, can the statement be more convincing, along the lines of 'the impact of ice rises on stabilising the ice front.

That is indeed a by far better formulation and will be included in the revised version.

Consider changing 'object' to 'location'

Yes, indeed location is by far more suitable and will be rephrased accordingly in the revised version.

Many thanks for reading so carefully and making so useful suggestions!

Line 23: You mention basal melting as a mass loss mechanism but do not then discuss it elsewhere in the Introduction. While I appreciate the focus is on calving, I think at least a sentence on melt rates beneath the ice tongue would be useful, making reference to previous work e.g. Wilson et al., 2017 We apologize that we mixed up the citation of Wilson's work. It should actually have been Wilson et al. 2017 and we cited Wilson and Straneo (2015) in line 220. The revised version is correcting this to Wilson et al., 2017.

Line 26: I think floating ice sheet should instead be 'ice shelf/tongue'

This is indeed correct and is reworded accordingly.

Lines 27-31: Given that a number of ice tongues in northern Greenland have recently disintegrated (ZI), or calved large tabular icebergs (Petermann Gl.), it would be good to include some context for the calving style you are referring to here, and how this compares/contrasts with calving elsewhere in Greenland from floating ice shelves/ice tongues.

This is also mentioned by Reviewer 2 and we discuss that in more detail below. We agree that it is important to mention other examples. The revised version will contain references to examples of calving fronts with pinning points from Antarctica, as the floating tongues mentioned here are not exhibiting any pinned calving fronts. We will also add text to compare that to floating tongues.

line 35: Given that Zacharias Isbrae is often also referred to as Zachariae Isstrom it would be useful to include both names here. Also state the time period (decade) over which the ice tongue was lost.

That is a good suggestion, many thanks. It will be included in the revised version as suggested.

Line 39: Add in the size/length of the ice tongue, either here of line 20 where you state it is the largest ice tongue.

We have decided to include it in line 20. Many thanks for pointing out that this was missing!

Line 42: Re-state what style of calving that was. Also where is the evidence for the style of calving in the 1980s, from satellite imagery? that was examined as part of this study? It needs a reference if not.

This is a good suggestion, in particular as this is no tongue-style calving. We will incorporate 'still in the same style as in the 1980's, with one lateral rift growing and widening over numerous years, similar to calving at Petermann Glacier.' in the revised version.

Lines 47-50: Given that you calculate buttressing, and discuss this when conducting your modelling experiments, it would be nice to include a sentence here to state how it impacts upstream flow.

We will include: “ A retreat of the floating tongue might imply a reduction of the buttressing exerted on the upstream part and perhaps lead to increased ice discharge.”

Lines 51-54: Before launching into the summary of the methods and imagery used, I think there should be a clearer statement of the research aim and justification.

This is indeed a very good suggestion. We will add here a paragraph doing exactly that.

It would also be useful to state the time periods over which the study is conducted, e.g. datasets used for xx to xx

This has also been suggested by the second reviewer and we will include a table in the Appendix in which we list the datasets and the respective time period.

Figure 1: Most people may know where in Greenland 79NG is, but I think an inset map of Greenland and a box/marker showing the study region would be useful. This inset map could also include the modelling domain used too. Also, include the time period of the crack formations A-F so it is clear when the 'oldest' and 'newest' refer to. Consider adding 'IR1' and 'IR2' labels to panel a) as well.

We agree, the overview figure is needed. We discussed if we could also fit the modeling part into it, but it seems to us to become too crowded, therefore, we suggest having another inset figure in the modeling section.

The suggestion to have a legend with A-F and the respective years is very good and we will definitely include this in the revised version!

Figure 2: It's quite difficult to see the calving fronts in panels (a) and (b), can you increase the contrast of these images? and perhaps also digitise the calving fronts so that it is obvious where they are.

Unfortunately, even with higher contrast the reflectance of the snow on sea ice and on the floating tongue is very similar. What we suggest doing is to add a thin colored, dashed line, marking the calving front position. With the dashed style the front can still be visible in the image, but is highlighted.

Line 55: Given that the material on the modelling simulations is all in the appendix, I think a clear summary sentence on the experimental design and purpose of these experiments is needed here.

Done. The sentence is rewritten to “The observational datasets are complemented by numerical modeling simulations which aim to assess the instantaneous velocity response to a retreating floating tongue (Section 3 and Appendix B)”.

Lines 68-69: It is not clear to me where the evidence for the statement that the tongue type calving style has been present for 28 years. It was also not clear from Appendix A1 what the date range of imagery that was used in the study. Did you examine imagery from 1975 in order to make this statement? Please clarify, both here, and in Appendix A1 (see later comment on this).

Yes, we did examine the Landsat 2 image from 1975-08-26 for the statement, but we decided to not include that in a figure, as this was also part of other studies. We suggest moving the sentence ‘This is in line with the findings of Khan et al. (2014).’ to line 69, as it is a very good reference for it.

Line 71: Can you instead provide a summary/overarching statement about how the the calving front has changed, instead of using ‘tremendously’

You are right: the sentence is somewhat misplaced here, as it is in the middle of the discussion of what changed and how it changed. We suggest removing it entirely in the revised version.

Figure 3: Some minor suggestions: add scale bars to (a) and (d), or state in the caption they have the same scales as (b) and (c). (b) state the time period of the surface elevation data.

We will add a scale bar to (d), however, the optical image from the onboard camera cannot be perfectly georeferenced, as it was our old system. We have tried to scale it similar to panel (b), but no exact scale can be given. We will state the date of the laser scanner DEM in the revised version - many thanks for pointing this out!

Change to 'photo from the Canon camera'

Done.

Line 76: As mentioned above, where does the evidence for tongue-type calving in 1975 come from?

From the Landsat 2 scene acquired on 1975-08-26.

Figure 4: It would be useful to show this track on a study figure or inset map so it is clear where it is located.

This track is shown in Fig. C2, but we forgot to mention it in the caption, which we will do in the revised version.

Line 82: Restate here when it was formerly grounded and when 'recent past' refers to.

We rephrase this sentence to 'we infer that a grounded spot existed at least for four decades and became afloat early 2014.'

Line 87: State the tidal range here, including references, or refer to the Appendix section where it is mentioned.

We added the sentence: , expected to be in the range of 1 m based on measurements of Reeh et al. (2000), Christmann et al. (2021) and FES2014b ocean tide model (Lyard et al., 2006),

Line 89: Consider making Fig.C2 a panel of Fig.1, it would be nice to see the location of the profiles in the main text.

In the meantime we are convinced that this is a good suggestion! Many thanks for pointing this out. We are planning this as Fig 1c and also take Reviewer 2's comments into account.

Line 89: The second half of this sentence needs rephrasing for clarity.

The sentence will be rephrased to 'Interestingly enough, ice penetrating radar shows in 2021 at the location that was formerly grounded still thinner ice (see Fig.\*, location of profiles shown in Fig.\*).' Figure numbers will change in the revised version.

Line 90, Figure 5: As you talk about radar profiles for 2021 and 2013 in this section, I think it would be useful if both appear in the main text rather than the appendix. Why not add a second panel to Fig.5 with the 2013 radargram?

Yes, this is indeed a very good idea. We will prepare a figure with two panels for the revised version.

Line 94: Can you include a measurement of how much crack A has widened?

In principle, this is a good suggestion, but the motion of the floating tongue is pushing the central part towards crack A. However, we think it is definitely worth it to give the reader an impression of how much the rifts changed and we suggest including the change in width for crack D instead. This will be included in the revised version.

Line 96: I don't find 'intersect the ice entirely' to be particularly clear, consider rephrasing, perhaps 'propagate through the full ice depth'

Done.

Line 99: see previous comment related to 'intersect the ice shelf'

We will rephrase this to '.. there is no indication that they are rifts that propagate through the full ice depth...'

Line 106: What evidence is this statement based on? the timing of lake formation/drainage did not coincide with changes in the cracks? Please expand on this.

Also Reviewer 2 has mentioned that we shall elaborate more on the role of hydrofracture and we do understand that this needs further discussion. We will definitely do this in the revised version and are actually happy to expand the text with a more thorough discussion of that topic..

*None of the cracks is a hydrofracture. Hydrofractures are basically crevasses (preexisting fractures) filled with water, either from surface melt draining into them as on Antarctic ice shelves (e.g. Scambos et al. 2000) or are facilitating supraglacial lake drainage (Das et al. 2008 ,Chudley et al. 2019). Crack A-D and F are newly formed cracks that grow vertically and horizontally at once - they are rifts. As an example, Crack D was initiated in March when no melt water was available at all. Also, the newly formed cracks do not follow the remnants of old crevasses, which hydrofractures would have done. Crack E is the only crack that has a potential for becoming a hydrofracture. Formed in 2019 it has so far survived three years without propagating vertically through. Although the floating tongue at the calving front is densely covered with melt ponds in summer, the lakes are small and shallow so the stress due to the water filling the crack has not yet been large enough to initiate hydrofracture. Hence, we conclude that the rifts are not hydrofractures but initiated by stresses (not due to water pressure) exceeding the material strength.*

Line 108: State why this eastern part is 'interesting'

This was also mentioned by the second reviewer and will be rephrased in the revised version of the manuscript.

Lines 110-113: I wonder if these sentences on crack modes would be better placed in the introduction when you first use the term 'tongue-type calving style'

Yes, very good point! We will move these sentences to the introduction where we add an overview of fracture mechanics and explain the different modes and correlated stresses. We hope this will give the reader a better understanding of the terminology. This was also suggested by Reviewer 2.

Lines 127-128: Rephrase these sentences for clarity.

Indeed, the reviewer is absolutely right in requesting a clearer text. We have rephrased this text to:  
'Crack arrest is not coinciding with rivers or lakes, as we find evidence for cracks to propagate across meandering rivers and lakes, and propagating further. One may wonder why rivers and lakes are not disturbing crack propagation.

*In comparison with the ice thickness of about 80m an 1-2m deep river or 3-4m (based on ALS data) deep lake is still a minor change in thickness. From our perspective, this surface topography can be seen as a surface roughness, but without an additional stress concentration, a surface roughness alone, is not controlling the propagation of a crack.'*

Line 145: Explain the characteristics of a 'kind of bridge'. I appreciate you do this on line 148, but I think it would be better to explain the ice bridge at 79NG first, and then refer to the Wilkins Ice Shelf.

This is a good suggestion. We will change the order in the revised version by first describing the ice bridge at 79NG and then going into the example of the Wilkins Ice Shelf - and it actually reads now much better! Thanks for suggesting this!

Line 155: Restate the time over which the calving style changed.

Yes, this is indeed very useful information to the reader. We will include that in the revised version.

Line 158: I'm not sure how useful this comparison to Wilkins Ice Shelf is, given that they have different settings, is there anything to suggest they would be similar in size? Perhaps make it clear why this comparison is necessary. Also, refer to a Figure or the location of glacier draining into the ice bridge from the south on Line 160.

We think it makes sense to mention all commonalities as well as differences of the two ice bridges to gain a detailed understanding and provide an explanation why the 79NG ice bridge is stable although it is narrower than the WIS bridge. We will elaborate that in the revised version and also include the reviewer's suggestion and refer to an adequate figure.

Line 170: Given that the methods are in the Appendices, please add a sentence to the main text that presents how your model replicates observations, e.g. the misfit between observed and modelled velocities after the inversion or how the grounding line flux calculated by the model compares to published estimates of grounding line flux/ice discharge.

We added a RMS between observed and modeled surface velocity; we differentiate between the grounded and floating part. We don't think that a comparison between simulated grounding line fluxes and other cited values is a good measure of how the model replicates the observations because it is strongly dependent on the location and length of the grounding line or flux gate.

Line 175: It seems that the impact of removing the ice tongue from the ice rises, and even removing it half way up the fjord, was relatively small, <10% increase in ice discharge. Therefore I'm wondering why you stopped there, and didn't follow up with a third potentially 'high-end' scenario in which you remove the entire ice tongue. This would be particularly interesting as it seems from your buttressing maps that the region closest to the grounding line is highly buttressed. This would be complementary to the impact of removing sections of ice tongues/ice shelves close to the grounding line in other regions (e.g. Petermann Glacier Hill et al., 2018 and Larsen C (Mitcham et al., 2022)).

Done. See answer above in the preamble.

Line 179: Again, as the methods are hidden in the Appendix (see major comment), there needs to be summary statements or better signposting in the main text. Here for example, it would be better to say something along the lines of 'We calculated ice shelf buttressing following the method presented in (ref) and found that...'

Done. We agree with the reviewer that this part needs improvements. We have extensively rewritten this part in order to better guide the reader through the experiments and findings etc. See also reply to RC2 179-185.

Line 180: Explain the term 'overbuttressed'

Done. See answer to RC2 Line 179-185.

Lines 193-194: Re-state the 14 month period that this 15% area was lost, and the decade over which it as remained in this 'intermediate state'.

We understand that the actual date is what the reviewer asks us to include in the text and we will do so in the revised version.

Figure 9: (a) It looks like the velocities near the northern part of the calving front go off the bottom of the scale? Are they zero here? or excluded from your domain? In (b) can you somehow highlight (e.g. polygon, shading, arrow) the region that has been removed during the experiment. Same for (c). Then refer to this Figure/panels when presenting the experiments in the main text.

Panels (d-f): white is not a good colour for the highly buttressed regions given that the background is white, consider changing the color bar.

Done, we used an improved colormap

In the caption, state what a value of 2 represents.

Done. We update the figure caption.

Lines 201-202: Is there a reference for this statement that the tongue of ZI was highly heterogeneous in the 1980s.

Done. We have added reference to Khan et al., 2014, which studied ZI using aerial photos from 1978 and Thomas et al. 2009, which studied ZI using NASAs ATM data from 1994-2001.

Line 204: It is a little bit confusing to refer to locations (e.g. 'northeast part') of ZI's ice tongue without referring to a figure. Consider labelling these locations on existing figure or creating a new figure/panel on existing figure (e.g. Fig.1).

This study is focussing on 79NG, this is why we do not want to introduce figures on ZI's ice tongue and it is far outside the range of Fig. 1. However, the new references included in the revised version provide context for these statements and provide a useful discussion for readers.

Line 213: Can an 8% increase in ice discharge be considered a 'significant contribution to sea level rise'? If so, some additional context/justification is needed. Consider referring to other examples of observed or modelled ice tongue calving/collapse elsewhere in Greenland and Antarctica.

We understand the reviewers point and will delete significantly in the revised version.

Line 213: Change 'is already' to 'would' because the calving in Fig.9c,f has not happened yet.

Indeed, this is the correct formulation and is included in the revised version.

Line 217: What about air temperature changes between 1990 and 2020?

This is somewhat unclear to us. In line 215-217 we explicitly write about the changes in air temperature and refer to Zhang et al., 2022.

Also in the following sentence, make a clearer link between thinning at the calving front and the potential for collapse in the near-future.

We have evolved this link throughout the manuscript, from presenting evidence for thinning, ungrounding and subsequent change in rift formation. It is unclear how the entire route shall be summarized again in one sentence. These lines 215-220 are providing the background to assign the thinning to surface melting and exclude basal melting being a driver here.

Line 217-218: I think it would be useful to expand on this discussion about thinning of the ice tongue near the calving front into the context of the rest of the ice tongue, i.e. the oceanic forcing is important for high rates of basal melting elsewhere on the ice tongue and thus responsible for the observed thinning (e.g. Mayer et al., 2018).

It is correct that oceanic forcing is important for high basal melt rates and thinning of the tongue, but the warm water sits at the lower part of the water column and is thus not in contact with the ice at the calving front. To discuss oceanic forcing elsewhere along the floating tongue would thus not give any additional information on the scenario at the calving front.

Line 227: Can you obtain measurements of basal melt rates to clarify this statement 'presumably small' Measurements of basal melt rates require two expedition minimum, which is nothing we can achieve easily in this remote area. In addition, we would not be able to obtain past basal melt rates, which would be needed in the context of this manuscript. In addition, we refer in line 220 to the remote sensing based dataset of Wilson and Straneo (2015).

Line 243: You use a number of different satellite sensors and I think it would be useful to include a table somewhere of the dates and sensors used. Also include the full time period for which your study covers. This is a very good suggestion - we will add that to the appendix.

Line 278: Why not include Figure C2 in Appendix A? Same for other figures in Appendix C and D. This is a very good suggestion! We will incorporate the figures in the respective subchapters of Appendix A, so that in the revised version we have all figures from observational data in Appendix A and have only Appendix B with modelling.

Line 284: 'usually better' by how much?  
Changed to: less than

Line 311: It looks like the resolution is also refined based on distance to the grounding line and calving front? If this is the case it would be worth mentioning.  
Done. You are right, the description of how the mesh is generated is incomplete and we have rewritten this part to: "Model calculations are performed on an unstructured finite element grid with a varying horizontal resolution between (Fig. B1). Over fast flowing ice (i.e. >300 m/a) we employ a resolution of 1000 m. Around the grounding line we refine the mesh up to 500 m. The domain is vertically extruded with 15 layers refined to the base."

Lines 329-330: Is it a common approach to assume the ice rigidity is constant over the grounded areas? Perhaps refer to other studies using this approach/temperature value.  
We agree with the reviewer that the constant ice rigidity is a very crude approach for the grounded part. A better approach would be to use a depth-dependent ice rigidity from other products, like thermal spin-ups over a glacial cycle. However such an assumption is very common when running an inversion for the basal friction coefficient

Line 338: Rather than state excellent agreement, can you instead include the final value of the cost function, or some measure of the misfit between observed and modelled velocities.  
Done. See comment to Line 170. We also provide the RMS values here.

Technical comments

Line 117: Change 'figure' to 'Fig 6c'  
Done.

Line 125: Add 'the' before 'direction'  
Done.



Line 130: 'at the end' instead of 'in the end'

Done.

Line 136: 'Remarkably there are'

Done.

Line 156: Delete 'with changing'

Done.

Figure 6: (d), there are no arrows shown in light yellow (<10) so this could be removed from the legend.

This is a good suggestion, we will do this in the revised version of the manuscript.

Line 188: Change 'featured' to 'features'

Done.

Line 307: 'mosaic' spelling

Done.