Dear Reviewer 2, dear Nina,
many thanks for your review that helped a lot to improve our manuscript! We find the detailed comments very useful and give below detailed answers and our plans for the revised version.
Your review was definitely very labor intensive and you have put a lot of time in it, for which we are very grateful for your effort!
Below you will find our detailed response to your comments. In some cases Reviewer 1 raised a similar point and we then refer also to our answer to Reviewer 1. In some lines you will find a short 'Done.' only, which means that we incorporated it right away in the revised version, but this is for grammar, typos etc only, all other things have detailed answers.
We again want to thank for your help to improve our manuscript!
Best wishes,
Angelika and all co-authors

Major comments

The **introduction** is not well structured and not concise (for detailed comment, pls see below). It is essential to better explain the configurational change at 79 NG, and why this motivates to study a regime change.

We understand that the reviewer wants to have a better structured and more concise introduction. In addition there were numerous requests to include an in depth introduction of fracture mechanical topics into the introduction. We will present both in the revised version of the manuscript.

The section **Transition in calving regime** needs to better streamlined and organized, too, and both figures and text need to be improved (for detailed comments, pls see below). Hydrofracture as a potential link between supraglacial melt features and crack propagation is dismissed, however, with insufficient arguments. The fracture mechanical context employed to describe crack formation lacks context necessary to follow the argumentation. It is essential to rework this section so that the wealth of useful information can be correctly understood.

We elaborate more on hydrofracture in the revised version and this can now already be found in the answers to the comments of Reviewer 1.

The **Impact** session suffers from two drawbacks. First, there is repetition as some information is already given in Appendix B and can be removed from the impact session. At the same time, Appendix B is incomplete and would benefit from additional information. Second, the text in the impact section is lacking important detail concerning the justification of model choice, description of results and metrics used, and that needs to be added (for detailed comments, pls see below) to allow for a better understanding of the results presented.

We will present a thorough updated version of the modeling part and our answers to the detailed comments below give more information on what we will present in the revised version. We will also include suggested modeling experiments.

The **Discussion** section appears incomplete and needs to be expanded, for suggestions pls see the detailed comments.

We are grateful for the detailed comments below, which we have answered all below.

The **Conclusions** reflect the manuscript in its present form, and may need to be revised slightly during the revision.

The **Appendices** appear to replace a full-fledged "Methods" section, which I found a bit surprising for this type of manuscript. Appendix A and B are introduced in the main text at appropriate locations, however, Appendix C and D are not, and actually should not, either, because they belong
to Appendix A. Pls see detailed comments below. Almost all figures need improvements, and Appendix B lacks necessary detail describing the diagnostic model setup.

We are restructuring the appendix in the revised version, so that all methods and figures related to observations appear in Appendix A and have then only Appendix B. We will also move the figures accordingly into the newly structured Appendix A.
The figures will be updated and we will incorporate all the detailed comments given below - also taking Reviewer 1's suggestions into account.

Detailed comments (text from the manus is given in italics) Abstract
Line 9: *"are a precursor"* -> may be a precursor
Done.

Section 1 - Introduction
Lines 18-19: provide names of the three floating tongues left. Return to these in the Discussion, see my comment later on.
The revised version will contain the names and we will refer to them in the discussion.

Line 21: I understand ice rises as features at the surface of an ice mass that are caused by iceflow over a bathymetric high ate the seafloor. Could the text *"Its ice front is still in contact with ice rises..."* be reformulated along the lines of e.g.: Parts of the front are grounded on ice rises, acting as pinning points, ... also because later (eg. in line 24 and in Fig.1) the ice rises are referred to as pinning point.
Thank you, this is a very good suggestion that is incorporated in the revised version.

Line 23: "*Calving and basal melting*..." References for this should be added. Also, basal melting is barely addressed later in the manuscript, so it should be mentioned that the focus here is on calving only.
We will include 'This study is focusing on calving only.' in the revised version of the manuscript.

Line 24: A ref is needed re "*calving ... is often governed by the existence of ice rises"*, and a more nuanced formulation is needed as well. Yes, ice rises could be related to calving if they are in the frontal zone of an ice tongue. But I do not see how ice rises that are far from the frontal zone should be related to calving? Pls clarify.
Indeed ice rises at the calving front are directly affecting calving. Ice rises far away from the calving front have an indirect effect, as they lead to damage, which may then influence calving. This has not yet been studied intensively. We will include two references in the revised version, Thomas et al. 1979 and Wang et al. 2022.

Lines 24-25: "*When the ice mass moves passes by ...*" -> When the ice mass moves past ...
Done.

Line 25: " *... rifts (cracks that are separating the ice entirely...."* -> rifts (cracks that penetrate through the entire ice thickness)
Done.

Line 26: "*which grow laterally into the floating ice sheet*" -> which may also propagate laterally.
We suggest to include 'typically' instead of 'may'.

Line 27: "*This calving style can be found at many locations....*" Pls provide references.
It is actually non trivial to find references to this calving style for Greenland, the calving fronts of Petermann Glacier and Ryder Glacier are not pinned, and no reference can be found for the northern calving front of Zacharias Isbræ prior to the recent changes. Many examples can be found at Antarctic ice shelves and outlet glaciers, and we will refer to this work in the revised version of the manuscript:

BERGER, S., FAVIER, L., DREWS, R., DERWAEL, J., & PATTYN, F. (2016). The control of an uncharted pinning point on the flow of an Antarctic ice shelf. *Journal of Glaciology, 62*(231), 37-45. doi:10.1017/jog.2016.7

Lines 27-28: "*Tongue-type calving is a normal process and is very distinct from break-up of disintegration events"* -> Shorten to: Tongue-type calving is very distinct from disintegration events
Done.

Line 29: "During these events, a large part is shattered..." -> During these events, a large part of the floating tongue is shattered.
Done.

Lines 30-32: In addition to modern disintegration, there is also evidence of similar ice shelf collapses in the past that should be mentioned, see eg. Jakobsson, M, et al. (2011). *Geological Record of Ice Shelf Break-up and Grounding Line Retreat, Pine Island Bay, West Antarctica*. Geology 39(7), pp. 691- 694, doi:10.1130/G32153.1
Many thanks for keeping an 'paleo-eye' on the introduction! We will cite the mentioned paper in the revised version.

Line 35: Insert space before "(ZI)"
Done.

Lines 39-44: Several issues here.
1/ This info would fit better in the context of the description of 79 NG that is initiated in line 19 ff.
Many thanks, this is indeed a very good suggestion that we follow. The new text will read as follows:

The largest floating tongue in Greenland is the 79NG (~70 km length and ~20 km width), draining an ice sheet area of 6.28% containing an ice volume of 0.58 m sea level equivalent SLE (Krieger et al., 2020). The floating tongue of 79NG has two calving fronts (see Fig. 1a), one in the north towards the Djimphna Sound (earlier this part of the 79NG was named Spalte Glacier) and one eastern calving front. The latest calving event at the northern front was taking place in 2020, still in the same style as in the 1980's, with one lateral rift growing and widening over numerous years.

Parts of the eastern calving front are grounded on ice rises, acting as pinning points (blue areas in Fig. 1a, denoted with IR in Fig. 1b) and it is hence an ideal location to understand the impact of ice rises on stabilising the ice front. The floating ice has an ice thickness of about 80-100 m in the vicinity of the calving front. Variations of the eastern calving front position in the past have not gone beyond an imaginary line between these pinning points (Khan et al., 2014) until 2013.

2/ Also, some basic numbers (length and thickness of floating tongue, depth at grounding line etc) would be useful to have upfront around lines 19 ff.

Numbers will be included in the revised version. Depth at the grounding line seems to us not very useful in a study that focuses on the calving front.
3/ line 41: remove "act as pinning points", has been said above (eg. line 24).
Yes, very good point and will be changed accordingly in the revised version.

4/ "*.... Calving event ... in 2020, still in the same style as in the 1980's*": this is not helpful because the "1980's style" isn't described (yet).
We rephrase this in the revised version: 'The latest calving event at the northern front was taking place in 2020, 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.'
5/ Incorrect grammatical reference in "*Changes of calving front ... style... have not gone beyond the line of these pinning point*" (how can a style go beyond a line?)
Yes, that is correct. We will rephrase this to 'have not gone beyond an imaginary line between these pinning points'
6/ "Line of pinning points" should be reformulated.
This has been rephrased to 'Variations of the eastern calving front position in the past have not gone beyond an imaginary line between these pinning points \citep{Khan2014} until 2013.'

7/ Incorrect tense: "A transition ... can...destabilize ... and eventually triggering " -> A transition ..can.. destabilize ... and eventually trigger.
Done.

Lines 45-50: Consider combining this paragraph with lines 33-38. Explain "*load situation*". Reformulate "*response ... in stresses ... of glaciers*".
Mention "Antarctica" in connection with Pine Island Glacier.
That Petermann is in Greenland is known once you introduce the glaciers name in the context of lines 18, see my comment above.
Done.

Lines 51-52: "*We leverage...of the floating tongue in the vicinity ..."* -> Here, we leverage ... of the floating tongue of 79 NG in the vicinity....
Done.

Line 55: *"... are complemented by numerous numerical "* -> complemented by numerical ....
Done.

Line 56: What do you mean by recent changes of 79NG configuration? From line 42 we know that something changed CE 2020, but not really what exactly. What exactly is the configurational change that motivates to study a regime change? This needs to be better explained, see my major comment.
We apologize, 'configuration' is a remnant from another draft sentence.

Line 60: Decide whether to refer to 79NGs tongue as a tongue or an ice shelf. This needs to be checked throughout the manuscript.
We have checked that throughout the manuscript and are now mentioning only floating tongue. However, we want to state here that it is well justified to use both in a text. While the floating parts of outlet glaciers in Greenland are normally called floating tongues, it is rather unusual to speak of over- or underbuttressed floating tongues. We also think that the readers are fully capable of dealing with both terms without becoming confused.

Figure 1: Several issues.
1/ Pls provide an overview map of Greenland with the location of 79 NG marked.
This has also been mentioned by Reviewer 1 and we will include this in the revised Fig. 1.
2/ Caption (a): "*IB marks an ice bridge (see text for details)."* -> add more precisely where in the text, eg. Section 2.

We will also highlight the area of the ice bridge in color in the revised version and rephrase the related sentences to make it more clear where the ice bridge is located. As we will add the former Fig C2 to Fig 1 as a third panel, we have also a better zoom into that area.

3/ Panel b: Pls provide some context for Fig 1b already in the introduction. Currently, the reader needs to wait for this until Section 2. Label (b) and scale bar are poorly visible. Consider rendering them in different color and at position with better contrast.
We will improve the figure as suggested. However, given that we move the introduction of fracture modes into the introduction already, we think that we can only afford a brief mentioning, as we otherwise overload the introduction.
4/ Panel b: Could cracks A-F be colored with individual colors? Explain in the caption what a stippled line vs a solid line means for the crack. Also, cracks are referred to as rifts in Fig 2. Decide on one terminology.
Indeed, obviously the cracks A-F could be colored with individual colors (six well distinguishable colors with each a good contrast to gray may be challenging), but we are not totally convinced if there is a lot of benefit from it. Reviewer 1 suggested including a legend with the date the cracks were formed, which we plan to do in the revised version.
Many thanks for mentioning that we missed to explain the dashed line in the caption! We will include that in the revised version. The dashed line denotes that the crack is not penetrating through the entire ice thickness, whereas all solid lines are rifts.

5/ Panel b: I guess IR1 and IR2 stand for IceRise1 and IceRise2. In panel a, they are referred to as pinning points. This should be homogenized, alternatively, say that you use the terms as synonyms. Consider adding the outline of the blue ice rises in panel a to panel b - may be useful for orientation. 6/ Add something about the supraglacial lakes.
We prefer to state that we use ice rise and pinning point as synonyms. One is the classification and one refers to its mechanical effect. We will state that in the text.
The reason why we did not add the outline into panel b is that it would overlay them and we find it useful to show their appearance.

Section 2 – Transition in calving regime
Figure 2: This figure shows changes in the frontal configuration 79NG. Pls consider modifying the figure such that configurations (outlines, prominent features) from 2010 and 2020 are overlain onto the 2000 configuration. This will make the comparison more visually straightforward. A central flowline could be added, as you refer to it later on (eg. line 72).
This is a very good suggestion and we will include that in the revised version of the manuscript.

Lines 64-74: Generally, the text here would benefit from streamlining and better organization. A simple guideline could be the chronology of events/calving front changes, from 1975 to present. Specifically, I have the following comments.
1/ Line 64: In this generality, I don't think this is correct.
One can eg. not say anything about changes in subaqueous calving based on calving front positions from optical satellite imagery. I think this needs to be reformulated.
We are not sure if we understand the reviewer correctly. Icebergs of all sizes are becoming afloat and we have never heard of icebergs being trapped underneath a floating tongue. Therefore all icebergs are visible in optical satellite imagery eventually. If fragments broken off from tidewater glacier calving fronts become larger or smaller or if formation of larger icebergs like at Petermann Glacier has changed can all be investigated using optical satellite imagery. It could well also be investigated by radar satellite imagery, therefore we will rephrase the sentence in the revised version and leave out 'optical'.

2/ Ensure easy matching between text and abbreviations used in figures. Replace "*between the two ice rises*" -> between the two ice rises IR 1 and IR2.
We will change it in this sentence as suggested in the revised version.

3/ Lines 66-67: *"Calving is initiated by the lateral rifts":* It would be helpful to indicate the rifts in Fig 2. Pls see comment above re Fig 2. Length of rifts in 2020 need not be given in the text if rifts A and D are indicated in Fig 2.
It is a very good idea to indicate rifts A and D in Fig. 2c and we will do that in the revised version. We do not understand why the length should not be given as it is not a repetition, but we will follow the reviewers suggestion in the revised version.

4/ Line 70-71: "*From the mid 2010's onwards, the calving front situation has changed tremendously"* – How does that go together with line what is stated in line 43: "*calving ... took place in 2020, in the same style as in 1980*". Is the 2020 event an exception from the new, post 2010 state? Pls clarify.
In line 43 we discuss the northern calving front and not the eastern calving front.

Lines 75-91: These paragraphs link to Figs. 3 to 5 and focus of the floating tongue becoming ungrounded at a certain location. I have several comments.
1/ "*Based on our database*" -> Based on the data analysed here.
Done.
2/ "*between the two ice rises a small grounded spot..."* What you are saying is that there is another ice rise, at the location marked by a blue star, correct? The formulation is not clear. Why don't you refer to this grounded area as an ice rise? Does an ice rise have to be of a certain size? Other criteria? This is confusing, pls clarify.
There has never been a formal definition of what characteristics an ice rise has, but grounded and ice flow moving around the ice rises rather than across, are the most referred to definitions. Given that we find that the location is not permanently grounded, we avoided calling it ice rise. As we have not high resolution velocity field (the one shown in Fig.6 is too coarse for it and there is no data available to produce any higher resolution velocity field) we can also not identify it as an ice rumple, which is normally characterized by ice flow being disturbed, but still moving across the grounded area. Therefore, we called it a grounded spot.

3/ "*Our assumption ..."* do you mean: our suggestion? To avoid repetition, you can replace "suggest" by "propose" in line 75.
We assume that the reviewer refers to line 76. We will rephrase this to 'Our inference is … '
4/ The text and **Fig. 3** don't go very well together. Starting point is 8 Aug 2013 (panels a and b). Pls add length scale to panel a and date to panel b, or combine them into one panel. Legend in panel b is m asl, pls add. Panel c has date 1 july 2013 so is showing a state prior to the one shown in panel a. When panel c is mentioned in line 80, no date is mentioned but it is natural to assume that it is a date after august 2013, which causes confusion. In line 80, reference is made to panel d, providing evidence for the ungrounding which is mentioned in line 76, however, without referring to panel d. Rather, because the sentence continues timewise with describing 2021 situation, there is risk that confusion arises (again) as to what actually is shown in panel d (obs scale is missing) and whether it is really necessary (from the caption to Fig. 3, it appears to provide geographical context only?). Also, it would be helpful if current Fig. 6d would be combined into current Fig. 3. Pls review Fig. 3 and caption and harmonize with the text.
Reviewer 1 also suggested adding acquisition date to panel b and scale to panel d, which we will do. Please see our answer to Reviewer 1 why we do not add a length scale to panel a. We will also change (m) to (m asl) in the revised version, thanks for the suggestion. We will also add more information into the

caption to avoid any misunderstanding. Fig 6d has a wider coverage and if we incorporate it into Fig 3, there would only be very few velocity vectors.
Because the time of acquisition of panel a and b is the same, we have decided that they shall be next to each other and with four panels, we cannot do that by arranging it differently.
As the Fig 3c and d have both dates given in the panel, it is unclear where the confusion about dates arises from. We begin this paragraph with briefly summarizing the important information (line 75 and 76) and then present the details. This is why panel d is not mentioned in line 76. We guess that we can solve this by ending the sentence in line 80 after the reference to Fig 3c,d and start a new one with 'Furthermore, ALS data [...]' .
To summarize, we will do our best to harmonize the figure and text in the revised version.

5/ Line 81: "*Since 2013 such dome-like structure...*" -> Since 2013 such a dome-like....
Done.

6/Line 82: "*An alteration...*": Wouldn't that fit better in the discussion section? Alongside an explanation why?
We think that this shall remain in the presentation of the results, as the discussion section is taking a different perspective, comparing the situation at 79NG to other locations like Jakobshavn Isbrae and Wilkins Ice Shelf.
7/ Line 83: Grammar: become –> became.
Done.
8/ Line 84: "*Ungrounding can be a result from two instances:*" Ungrounding can results from at least two processes.
We wrote 'Ungrounding can be a result from two instances: (i) thinner ice approaching the shallow bathymetry or (ii) thinning of the ice locally.' and we do not see more than these two possibilities. Either thinner ice is advected or the ice thins locally. Please advise which other possibilities are given. The sentence is not stating what may cause local or non-local thinning.
And yes, indeed, at the ice rises, a lowering of ice tongue surface elevation must come from surface melt or dynamic thinning. However, at floating locations, even basal melt should reduce to surface elevation lowering because reduced buoyant forces cannot sustain the same ice thickness above the waterline?
We are not sure what the critical point is. Please see the answer above, potentially this leads already to clarification.
You have mentioned basal melt in line 23 but don't seem to pick up on it any further? Pls clarify. There are some remarks later in the discussion re suspected low basal melt rates where the issue could be discussed nicely in appropriate detail.
Basal melt rates are picked up in the last paragraph of the discussion section and the conclusion. As we can exclude this being the reason for thinning, we have not had a motivation to go into more detail.

9/ **Fig 4.** Please provide an inset to the current figure showing where the profiles are located.
Fig. C2 does show the location of the profile. We will mention in the revised version in the caption where to find the location of the profiles.
The calving front position of 2013 is nowhere shown, and the 2021 position could be inferred from Fig 1.a, but it gets cumbersome if this becomes a to-do for the reader. Why are supraglacial lakes included in the figure? If they were introduced in Fig 1, it would be easier to see a red thread (since lakes are mentioned later, too). What about lakes in 2013? Pls provide sufficient information regarding Fig. 4. Caption: Pls check grammar. A location cannot correspond to a Figure.
It is unclear to us why the reviewer refers to the calving front position here and we imagine that there is a misunderstanding. This profile (as shown in Fig. C2) is an exact repeat profile, flown in 2013 and 2021. The calving front position is obviously different in both years, but this is not the point we discuss here. This figure

is about thinning - and lowering of the surface. We assume that we confused the reviewer by the caption that has stated 'Thinning along the calving front … ' and we are rephrasing this to 'Thinning along a profile in the vicinity of the calving front …' When looking with the aim of analyzing thinning on this figure, two features may confuse: the rift and lakes. As the surface of the floating tongue is densely covered with small lakes, a profile may by chance cross a lake in year 1 and not cross lakes in year 2. This is why we mark these features to make it easier to understand. We think that the reader will also benefit here from moving Fig. C2 into Fig. 1 in the revised version, as the location of the profile is then already introduced when coming to this figure in the manuscript.
The calving front position of 2013 is shown in Fig 6d.

10/ Lines 89-91: Could this be better placed in line 82, before "*we conclude*"?
This is a very good suggestion and will be followed in the revised version.

11/ Fig. 5. Pls add lines showing the approximate location of the underside of the floating tongue, and the seafloor. Same applies to Figs. C1 (a-c).
We will insert arrows denoting the location of the ice base. As this is a radargram, the seafloor is not observed - ocean water is not transparent to radar waves.

Line 92: "For the sake of ..." This is repetition from Fig.1 and can be replaced by a ref to the Fig instead.
Actually, this is the location where the rifts A-F are first discussed. We can shorten the sentence by leaving out 'For the sake of', but it would be odd not to introduce the naming of the rifts.

Line 95: *"lower ice rise"* Do you mean "southern"?
Done.

Line 98: "*unification"* -> do you mean that the northern and southern branch of crack E joined? Pls Reformulate.
We reformulated that sentence to
'Although optical data show in 2021 that the northern and southern branches are close to join (shown in high resolution in Fig.\,\ref{fig:crackE-optical} marked by a red arrow), …'

Lines 103-106: "*None of the cracks is a hydrofracture"*. How can you be sure? This is quite a strong statement, and the simple mention *"we did not find evidence"* is in this brevity not a satisfactory argument. What did you do to arrive at you conclusion?
We agree with both reviewers that we did not provide a good explanation for our conclusion that none of the cracks is a hydrofracture.
We can exclude Crack D easily to be a hydrofracture because it arises before the melt season. As the melt ponds at the 79NG were small and shallow at the time the cracks appeared, the water amount was not large enough to initiate hydrofracture. Furthermore hydrofracture is a secondary fracture process, because crevasses must exist beforehand. As cracks did not follow remnants of historic crevasses but correspond to higher stresses we conclude that exceeding the material strength is the reason for most of the cracks.
We will elaborate our conclusions in the revised version to give a satisfactory argumentation.

You have bothered to indicate lakes in Fig. 4, why? "either from surface melt of facilitation supraglacial lake drainage": Even the latter are related to surface melt. Pls reformulate and check grammar, and also add a ref to at least Fig. 1b.
This has been answered above under 9/ **Fig 4.**

Line 108: *"at 79 NG's interesting part ...."* -> remove interesting and reformulate sentence.
Yes, indeed, this is not well formulated and will be rephrased in the revised version.

Lines 109 - 124: Several issues here. 1/ line 109-111: Provide a reference. Why mention mode III – it is not relevant for the following?
We include a reference to a text book in the revised version. As there are only three crack modes (and not a large number), we find it useful to introduce all three.

2/ Lines 111-112 and Fig 6: Explain briefly how shear and tensile stresses are related to mode I and mode II, and to first and second principal normal stress and maximum stress mentioned only in the caption to Fig. 6. Otherwise, this part and the following discussion cannot be understood.
We find this indeed a very useful point and have restructured our introduction and include a larger text part on general introduction to fracture mechanics. This is certainly useful for the reader and we are happy to do so. So, these sentences will be moved to the introduction where we will provide an overview of fracture mechanics including the different modes and their correlated stresses. Therefore we will expand this text passage in the revised version (see also our answer to comment of RC1).

3/ Why do ice rises induce shear stress and why does tensile stresses increase downstream of the ice rise? How is this related to velocities? This must not be left to the reader to find out but needs to be explained.
Yes, indeed it is absolutely worth it to elaborate more on this and we will do that in the revised version.

4/ Line 114. Grammar and terminology. What is an unstable crack and can a crack (a void) detach an ice berg? The latter is detached from the tongue and crack propagation accelerates – is that what you mean? Reformulate.
Many thanks for pointing us to this! Indeed the usage of unstable can become entirely misleading here. We intended to say that the crack propagates, but we did not intend to say if the crack propagation is stable or unstable. We have rephrased this for the revised version.
'The floating tongue downstream the ice rises is incised laterally on both sides and eventually one of those initial cracks is reaching a critical limit and propagates further, disrupting the entire ice vertically as well as horizontally and leading to the detachment of an iceberg.'

5/ Line 117 ff: *"Velocity field of 2014-2016"* Why a two-year averaged one? From where are these stresses? From ISSM? This needs to be explained.
The stresses are an output from ISSM. They correspond to the inversion state with the 2014-2016 winter velocity field as target. We will explain that in the revised version
This is a winter velocity field, meaning only scenes with frozen surface conditions are considered, as also explained in the Appendix for the inversion.
Please also see our answer below to line 315.

Line 129: "descend" -> decreases.
There is no mention of 'descend' in Line 129 or adjacent lines.

6/ Fig 6. Add to the caption that the scale in panel c is valid even for panels a and b. I think that would be helpful.
Many thanks for mentioning this. It will be added to the caption in the revised version.
Consider having panel d in Fig 3 instead, see my earlier comment.
There are only two data points in the area covered by Fig. 3, which is why we suggest leaving it here.

Pls show the floating tongue in panel d a lighter grey so a better contrast to the arrows and the star is achieved.
We are afraid that it would then be difficult to separate it well from the ocean mask.

7/ Did you provide evidence for your statement that (line 121) *"Loss of contact to the grounded spot is leading to an increase in main flow"*? If yes, please refer to it, if not, pls add and/or explain. Pls also check grammar in lines 121- 124, there is a mix of tenses that should be checked for correct use.
Fair point - we have checked the entire database at TU Dresden and have to conclude that we cannot present evidence for it. We will delete this statement in the manuscript accordingly.

Lines 125 – 140: Here cracks and supraglacial water and fatigue failure is discussed. Several issues:
1/"River" – has nowhere been introduced. You should mention (eg. in the context of Fig 1b) that supraglacial lakes can be connected by supraglacial streams. Is it "stream" or "river"? There is a lot of literature on supraglacial hydrology at the Greenland Ice Sheet so it should be easy to find the correct terminology and add a couple of references.
We conducted a literature search, but none of the studies we found is giving a definition for the width of a river or stream, which we understand being prone to resolution in the available imagery. In our text, the main info is, that the surface water body is not drained. If it is a river, a stream, a lake or a small pond is secondary from our perspective. In the papers we read, the term river is also not introduced. It is likely assumed that the term river is known to readers, which we assume here, too.

2/ *"de-watered*" -> drained. Also in Fig. D2.
Done.
3/ Add a line showing the base of the floating ice tongue to Figs 5, D1. Otherwise it is hard to see that the crack is not intersecting the entire ice thickness.
We will add arrows marking the ice base.

4/ *"Forming a non-intersecting crack".* I don't understand the terminology chosen. Crack E is rendered as a stippled line in Fig 1b, with individual ends propagating towards each other. In Fig. D2 their expected joining location is highlighted (btw how did you predict this location? And pls add a north arrow to Fig. D2) So why "non-intersecting"? And how is that linking to the fatigue fracture? I understand the latter, but not how the connection to the not-yet joined crack is made.
We have rephrased this to 'vertically non-intersecting crack', which describes the situation accurately. The caption of Fig. D2 is rephrased to 'The red arrow points to the location of the crack tips at the surface.'. We will add a north arrow in the revised version.

5/ Maybe it would be better to group this part as section on supgraglacial meltwater features and cracks (lines 125-133) and a section on oceanic forcing of crack propagation (lines 133-140)?
We have discussed that lengthy, but given the fact that meltwater features have no effect on the fracture formation, we do not want to include a section, as it would be misleading. We have rephrased the text where it was mentioned before to make it more clear.

6/ Pls add missing information to the caption of Fig. 7. What is ranging between -pi and pi (if the symbol is pi?) Pls add a pointer to cracks A and E.
The phase is ranging between +-pi - it is an interferogram. Yes, the symbol is pi and we try to change the font. We will add a pointer to both cracks.

7/ Line 137: *"The ice plate"* do you mean the floating tongue? I don't fully understand why you need to argue that grounded and floating ice are connected at the ice rises (lines 136-139). Because you have fractures around the ice rises? Isnt it sufficient that the tides work on the floating part? Pls clarify.
As above, we will change the terminology 'ice plate' for clarification in the revised version.
To understand the stress situation as well as the kinematics it is necessary to describe the boundary conditions.

Lines 141-143: This is a bit unconnected, pls consider moving, providing additional context, or removing.
This is a good suggestion. We will move this further up, before crack E and the chaotic zone are discussed.

Line 145: *"detaching...."* -> reformulate eg as: will detach about 20 km^2 of ice but leave the ice bridge (IB, Fig. 1a) unaffected.
This will be incorporated in the revised version as following: We anticipate that calving along the cracks D–F–C–A detaching about 20 km^2 of ice but leave the ice bridge (IB, Fig. 1a; area ~ 55 km^2) unaffected.
Reviewer 1 had an additional useful comments to this text block. As a consequence we have restructured this paragraph and give more information on that in the answer to RC1.

Fig 8 and caption. Use either chaos zone or chaotic zone, it is not homogenous in the text. What does the red line denote? Add north arrow. Panel d shows locations for which no hinge zone is visible in interferograms. Are the latter shown? What is the meaning of the easternmost interferogram location – it is in the ocean?
We will add a north arrow. It is unclear what with 'What is the meaning of the easternmost interferogram location - it is in the ocean?' is meant. If this refers to the right pink dot, it shall be noted that the blue line marks the shore line and the pink point is located on the shore line.

Lines 152-157. Consider providing this information as a diagram (staples, or similar). This provides a more attractive means of conveying the info than in repetitive text.
We understand that repetitive text is not very appealing, but we give only four time snapshots here, which does not make a very attractive figure. In addition, the manuscript already has a large number of figures. So, we suggest leaving it as text.

Line 158-159: Add info on WIS IB to line 147 instead (and remove here).
We suggest leaving the text at its current position, as the lines 157-159 are giving the numbers for the ice bridge on 79NG, which is not discussed at line 147 yet.

Line 159-161: Lambert Land has not been introduced. Perhaps just remove? Sufficient that two glaciers drain into the 79 NG from the south? Do you show evidence of the bulging zone?
We agree - the revised version will contain 'from the south' instead of Lambert Land.

Section 3 Impact – response of the 79 NG instability of the calving front
Lines 166-167: "*We attempt ...discharge"* -> This is repetition from line 163. Shorten and mention ISSM directly, eg: We address this question using ISSM (add ref to App. B). Then: continue to describe the three experiments (line 170 onward).
Done. This part is rewritten accordingly.

Line 167-170: Repetition from Appendix B. Remove.

We do not fully agree with the reviewer to remove this part from the main text although it is a repetition from the Appendix. We think running an inversion is an important information to understand the initialisation state without reading the technical part of the Appendix. However, we deleted some parts to make the text better readable. Together with the comment above, this part reads: “As NEGIS is a fast flowing ice stream and drains a large area of the GrIS (17.23%, Krieger et al., 2020) it has the potential to
contribute to sea level rise by an increased ice discharge, once the boundary conditions, such as its calving front, are changing. We address this question, how future large calving events or even a large disintegration event will modify the ice discharge, by using the Ice-sheet and Sea-level System model (ISSM, Larour et al., 2012). First, we initialise the model using observational data of surface and basal topography as a target to determine the initial conditions by a running a joint inversion for the basal
friction coefficient and rigidity of the ice (see Appendix B).”

Lines 170-178: Here, the three experiments should be described, as well as model output variables and metrics used to derive conclusions, eg. the buttressing parameter that is so far only mentioned in the caption to Fig. 9.
We agree, the revised version will contain a better description of the experiments and the buttressing parameter.
Pls justify why you run diagnostic experiments only when the temporal evolution of the changes at 79 NG clearly are in the overall focus.
We are not intending here to simulate the retreat itself, but the long term impact it will have. To this end a diagnostic simulation of the new state is sufficient. But we fully understand that retreat simulations are very interesting and we are looking forward to go into that direction in a future study.

Pls also explain why a Blatter-Pattyn (BP) model was favored over a Full Stokes (FS) model, when in a recent study of the NEGIS, , ’considerable differences at the grounding line’ were found and that ’results from non-FS models should therefore be viewed with caution’ (tc.copernicus.org/articles/16/1675/2022/). Maybe the error is systematic and so not so important in terms of % increases in discharge, which is the main takeaway in the following. But, I would suggest that either running an FS simulation as a comparison or including a discussion of the potential errors / motivation of the model set-up is necessary.
Done. We will either include a FS simulation or discuss the potential errors as suggested. See also answer to major commnet by RC1.
Moreover: 1/ Line 171: Remove number concerning grounding line flux for the init run.
Done
2/line 175: “Bottleneck”: Do you mean in term of the fjord geometry narrowing? Pls explain better.
We have rephrased this into ‘[...]in which we assume that the calving front retreats up to a point where the fjord geometry is narrowing (denoted southern and northern bottleneck (SBN and NBN), respectively, in Fig. [...]’.

3/ Line 176: Incorrect Fig ref, needs to be corrected to Fig 9f.
Done.
4/ Fig 9: parts of the floating tongue that are removed in the two perturbation runs should be rendered hatched (or otherwise highlighted) in panels b,c,e,f.
Done.
All panels can be cropped to instead focus on the relevant model domain.
As mentioned above, we do not crop the images as the new experiment “collapse” shows speed-up far upstream the grounding line. Sure, Figures could (d), (e) and (f) could be cropped, but we would like to keep the same figure extension for all figures of the modelling results.
Insert a frame showing the extent of eg. Fig 1a in Fig 9a.
Done.

Lines 179-185: Several issues. 1/ Explain buttressed vs overbuttressed. The Borstadt parameter ranges between 0 and 1 according to the caption to Fig 9. So why is the scale in Fig 9d,e,f from 0 to 2?
Maybe the caption is misleading. The Borstad buttressing parameter ranges from 0 to >1. A value of > 1 corresponds to overbuttressed ice shelf. An overbuttresed ice shelf means that the longitudinal stresses are negative, i.e. compressive. We added this to the text and clarified the figure caption.
2/ Move grounding line flux for init here.
Done
Consider comparing your findings to the other modelling study which you cite in the Introduction wrt grounding line fluxes (de Rydt et al., 2021).
We actually find it more useful to compare it with the changes in grounding line presented in Mouginot et al. (2019) as that study is presenting values for the same glacier. The revised version will include this.

3/ Velocities are not discussed at all, this needs to be added. Especially since velocities and their changes after ice tongue breakups are mentioned several times in the Introduction.
Done. See answer to RC1 line 179.
Modelled results are presented at the ice surface only, correct? What about their distribution in the vertical which is also a direction in which the cracks evolve.
Indeed, we presented surface velocities in the figures, but not in Fig 9f that is discussed here. Of course we simulate the 3D velocity field. Two things to clarify here: (i) the rifts evolve in horizontal direction, but through the entire vertical direction. (ii) the discussion in like 179-185 is about buttressing and grounding line flux and not velocities at the calving front.
4/ Panel 9a shows a modelled region southeast of 79 NG, in all other panels, this is not shown. As it is not relevant, I suggest to remove it in panel a, and also in Fig B1. If not, pls motivate and explain why it is kept.
First of all, all panels show the same region (also Figure B1). Since the other panels show Zeros of velocity differences and the buttressing parameter there is nothing to show. So, generally we would agree with the reviewer to crop the images. Since RC1 requested a third experiment, a full collapse of the floating tongue, we do not crop the images. This experiment shows large velocity differences upstream the grounding line which would be hidden once we crop the image.

Section 4 – Discussion
Line 187: “First...” does this opening refer to lines 190-210? One does expect a “Second”, which would perhaps be the discussion of the modelled ISSM results in terms of experiment design (lines 213-214), as well as a “Third” which would be changes in regional climate and their potential impact on 79 NG (lines 215-220)? And what about adding a ”Forth”, see below. Re “first”, my specific questions are:
We have rephrased this for the revised version to ‘We start by comparing…’
1/ line 194: provide the year during which this mass loss occurred.
We guess you mean before the calving front settled. Because it is actually not a mass loss as it is a floating ice body. We will incorporate the year in the revised version.
2/ Line 200: For consistency, star a new paragraph when ZI is described.

Regarding “Second”, the discussion should be extended to include more nuanced reflections on the limitations of diagnostic simulations and to offer an explanation as to why prognostic simulations were not address here. Also, it would be useful to include reflections on whether or not a crack propagation (instead of removing parts of the floating tongue based on observational evidence and some extrapolation) could be captured in a prognostic setup? Also, are results from similar numerical simulations regarding the buttressing of JI, ZI and WIS available in the literature? This would be an interesting comparison.

Likewise, the grounding line fluxes modelled here could be placed in observational (and modelling, if available) context, which would provide a broader picture with regards to future expected changes at 79 NG.
This is a very good suggestion and we will include a comparison with the values in Mouginot et al. (2019) in the revised version.
If no additional FS simulations are run for comparison (see my comment above) a discussion concerning the potential shortcomings of the BP model simulations needs to be included.
Please see answer above
Regarding “Third”, why is the impact of expected future climatic change on 79 NG in a rapidly changing Arctic not discussed, especially with a focus on supraglacial melt features that have been mention a number of times in the manuscript?
We do discuss the changes in air temperature in line 215-219. It is actually the paragraph in which we also discuss why increase in air temperature and not basal melting is the cause for reduction in ice thickness.
Regarding “Fourth”: in the introduction it is mentioned that 79 NG is one of three remaining ice tongues at the northern Greenland margin. I suggest adding a discussion focusing on a comparison with the other two, Ryder and Petermann. That is at least as relevant as comparing to ZI, WIS and JI.
This is a very good suggestion and we are happy to do so in the revised version.

Section 5 – Conclusions
Line 234. I recommend to weaken this to “may be at the onset...”
We discussed that in depth and came to the conclusion that we do not agree to include ‘may’ in that sentence. This sentence says that our findings ***‘indicate*** that 79NG is at the onset of a transition from stability to instability’, which is a moderate statement from our perspective.

Appendix A: Data
A1 is very well written. But I find the figures not as helpful as they could be because of a lack of overall structure. So, before setting out with A1, pls consider presenting an overview figure like the present Fig. C2 (or a variant thereof, perhaps with more zoom into the relevant region?) as a main orientation figure for the Appendix, before you present data along the various profiles in subsequent figures and dive into the various appendices A1, 2, 3 etc.
We also got other comments about Fig. C2 being better presented in the introduction already and agree to move it further up. We will then also zoom more into the relevant area, but leave the southern extent of the figure as is, because we want to mark the ice bridge in that new figure then, too.

Minor changes: Line 260: “*was formed which is shown in Fig 7”* -> was created.
Done.
Line 247: “*Furthermore, we apply”* -> Furthermore, we use.
Done.
Line 257: *“Here we apply SAR interfermetry”* -> Here we apply InSAR.
Will be added in brackets in the revised version, see comments RC1.

A2 is very well written. My major question is: Why is there a separate Appendix C with Figs. C1 and C2, and a separate Appendix D with Fig, D1 which all clearly belong to A2? Here in A2, you should continue to present Fig C1 (C2 should be taken care of at the start of the appendix, see above), and
then present Fig. D1.

Reviewer 1 also suggested to rearrange the appendix and we find both reviewers suggestions very good. Our plan is to have two sections in the appendix, one on data and one with modelling. We will then also present all figures in Appendix A (the data appendix).

Line 272. What is the thickness of the floating tongue? I suggest to include that as base info somewhere in the introduction, see one of my comments above.

This is a very good suggestion and we incorporate this in the revised version.

Figure C2: Please add map showing location, orientation and length of the profiles in panels a, b c.

Figure C2 has only one panel.

Alternatively, perhaps introduce a new overview figure where all profile locations are given, see my comments below in relation to the Appendix.

Fig. C2 is the figure in which all profile locations are given.

Pls specify whether the same equipment was used in 2013 (data in panel a), or add a ref where the data description can be found.

The section A4 explicitly states that the laser scanner was the same instrument. Fig. C1 explicitly states which data is used in the different panels. We can, however, add a legend, in which we can list the data for the different profiles.

Abbreviation EMR has not been introduced.

We recognised that we did not only not introduce the abbreviation, we entirely missed to introduce this radar instrument! The revised version will contain a paragraph on this radar in Chapter A2. Many thanks for pointing this out, we would have missed that entirely and are very grateful for the careful reading!

We added in A2:

In addition and to compare to the more recent UWB system (Figure C1) we used data from the Electromagnetic Reflection System (EMR). The EMR is an airborne radio-echo sounding system used to map ice thicknesses and internal layering of glaciers, ice sheets and ice shelves. The system is capable to penetrate 4000 m thick ice. It was designed and built by AWI in cooperation with Aerodata Flugmeßtechnik GmbH, Technical University Hamburg-Harburg and the Deutsches Zentrum für Luft- und Raumfahrt e.V. (DLR). The radar signal is a 150 MHz burst with a signal length of 60 ns or 600 ns. The maximum performance is 1.6 kW with a sensitvity of 190 dB (Nixdorf et a

Line 268: “antenna” -> antennas.

Done.

A5: Line 307 *“mosais”* -> mosaic

Done.

Appendix B: ISSM model setup

The ISSM model setup for the diagnostic runs in Section 3 is insufficiently described. Pls provide a more complete description.

We apologize that the description was incomplete. In the revised version we envisage to provide a more detailed description.

Specific comments/questions:
Fig. B1: add location of 79 NG to the figure.
Done
Consider cropping to the relevant area, cf. comments in the context of Fig. 9.
We do not crop the images because of the new ‘collapse’ experiment which shows large velocity differences upstream the grounding line. See answer above to Line 179-185 /4.
Line 315: Why are only winter surface ice velocities used for the inversion?
The offset tracking methods have issues in summer as coherence is easily lost by the changing surface conditions. This leads to bigger areas not covered with data and also noisy velocity fields. Therefore we (and other studies, too) rely on ‘winter velocity fields’, so fields outside the melt period.
Line 316 *”Since the surface velocity field have*” -> Since the surface velocity field has .
Done
Equation B3: What is the meaning of the plus side in front of gamma_1?
Done. The plus sign is a typo and is removed.