Dear Editor, dear Jan,

Many thanks for the very, very helpful suggestions!

Before presenting below a point-2-point answer, we want to mention that we recognised that we did not include the new simulation results in the conclusion and abstract. Apologies for that! The new version has now an update of the numbers in abstract and conclusion.

We give detailed answers to all points below and prepared a new version of the manuscript with tracked changes.

Best wishes,
Angelika and all co-authors

LINE-BY-LINE COMMENTS:

(all line numbers refer to the manuscript with tracked changes)

L1 replace ‘so far’ by the timeframe of available observations
Done

L1-2 The first sentence of the abstract talks about changes to the ice tongue of 79N, whereas the second sentence compares this to changes to the grounded ice of ZI. This seems like comparing apples and oranges. The third sentence then jumps back to 79N. Is the comment about ZI needed here?
Right, it is indeed not needed and we deleted it for the new version of the manuscript.

L4 ‘will’ lead to sea level rise, or ‘is likely’ to contribute to future sea level rise?
We have rephrased it to likely.

L5 employed
Done
L6 add comma: …calving front, from…
Done
L7 unclear what is meant by ‘normal’ tongue-type calving. Is this commonly used jargon?
Correct, there is no normal or abnormal tongue-type calving, only tongue-type calving. We have deleted normal for the new version of the manuscript.

L8 maybe replace ‘increase’ by ‘expansion’
Done.

L8 the description of the melange and ice bridge is difficult to comprehend without the help of Fig1 and 8.

Could you reformulate to say that damage upstream of the main calving front has created areas of open water and an ice-berg melange?
Very good suggestion. We incorporated this into the text as follows: The calving front area is further weakened by an area upstream of the main calving front that consists of open water and an ice melange that has substantial expanded, leading to the formation of a narrow ice bridge.

L11 maybe say ‘future ice-front retreat and complete ice-shelf disintegration’ rather than ‘break-up or disintegration’, which sound identical.
Done.
L11 ‘discharge of grounded ice’ instead of ‘ice discharge’
Done.
L12 add …’south-eastern area of the ice shelf…’ for clarity
Done.
L26 ‘was taking place’ or ‘took place’?
‘took place’ is the correct description. Many thanks.

L26 The reference to calving style might need some further context. For example, can you say that since the first observations in the 1980s, all calving events have followed a similar pattern, following the extension of a lateral rift over several years?
Yes, that is a good suggestion. Indeed all calving events have followed a similar pattern. The new version is: All calving events still since the 1980's followed a similar pattern, with one lateral rift growing and widening over numerous years.

L31 ‘gone beyond’ in the upstream or downstream direction?
In upstream direction - changed for the new version of the manuscript.

L42-45 by ‘different mechanisms’ do you mean a ‘variety of mechanisms’, or do you mean that crack formation is controlled by different mechanisms compared to crack propagation? In the latter case, I think references are needed, and a clear distinction needs to be made between what mechanisms are responsible for crack formation, and what mechanisms are responsible for crack propagation (or deformation).
Very well spotted! We indeed meant a variety of mechanism. It is changed in the new version of the manuscript.

L46 and following: some of these concepts are explored in detail in https://tc.copernicus.org/articles/14/1673/2020/
It is correct that the reference proposed is exploring model I cracks, but many other references do too and at this stage in the text the only thing we want to do is to introduce the definition of mode I, II and III to the reader and therefore we refer to a textbook only.

L64 …‘are’ stabilizing... I would prefer: ‘are buttressing’
Done.
L65 …‘an’ increase…
Done.
L66 ‘most recent’ instead of ‘latest’?
Done.
Can you add a specific time instead of staying ‘in the past decade’?

We rephrased this to ‘since 2012’ and cited Khan et al. (2014).

L69 I think you use ‘stable’ to mean ‘unchanged’ throughout the manuscript. It might be good to clarify this somewhere, in order to avoid confusion with the term ‘stability’, which comes from dynamical systems theory and means something different: a steady system is stable if it returns to the same steady state following a small perturbation.

This is a very good point. We have incorporated in the introduction section this note: (we use stable as synonym to unchanged)

L71 ‘an’ ice discharge increase

Many thanks - and Petermann Glacier was not in capital letters, too. Both changed now.

L72 Why do you start the sentence with ‘However’, if the measurements and modelling results are consistent?

Correct, the use of ‘however’ was not appropriate and we have deleted it.

You will also need to include Hill et al. 2018 (https://doi.org/10.5194/tc-12-3907-2018) here.

Indeed! It is now included.

L73-82 These sentences sound rather fragmented. Can you reformat into a coherent paragraph?

We rephrased this paragraph and moved the part on Petermann Glacier to a paragraph below, which was already discussing Petermann Glacier. A reviewer asked us to introduce Petermann Glacier in this paragraph, but as we are focussing in that paragraph on observed disintegration events, it was there anyway not at the right location.

L80 ‘mandatory’ is a strong use of words; perhaps say ‘informative’ or ‘a first step towards’

Changed to ‘a first step towards’

L81-81 Do you have evidence from the published literature (or even anecdotal evidence) for this statement?

This is a very good point. It is indeed very difficult to find a suitable reference for this (which should be an inspiration for future modelling studies). One reference that presents some evidence is in Matsuoka et al (2015, https://doi.org/10.1016/j.earscirev.2015.09.004) which we cite in the new version of the manuscript. ‘The area of ice shelves in this region decreased by 6.8% between 1963 and 1997, mostly in regions without ice rises and rumples near the calving front (Kim et al., 2001). This observation supports the hypothesis that ice rises generally stabilize ice shelves.’

L83 what do you mean by ‘catastrophic’? I would remove this part of the sentence.

‘Catastrophic events’ is a well defined terminology (e.g. MacAyeal et al., 2003; Hulbe et al., 2004; Braun et al., 2009)

Also, by ‘change in load situation’, do you mean ‘change in ice-shelf geometry’?

No change in stress situation. This terminology is very often used in the field of mechanics.

L88 Do you mean ‘a moderate retreat of the ice front’ instead of ‘a moderate calving rate’? Pine Island is one of the fasted flowing Antarctic glaciers, and even a steady ice front location would require the glacier to have one of the highest calving rates on Earth.

This is correct and we have changed it to ‘moderate retreat of the ice front’.

L92 ‘Set this into the context of the stress regime’: can you be more precise? Are you linking changes in calving rate / calving flux / calving extent / … to changes in principle deviatoric stress / …?

We changed this to ‘and compare this to principal stresses prior to the crack formation’

L94 replace ‘destabilisation’ with ‘further retreat’

Done.

L95 ‘numerical perturbation experiments’ instead of ‘simulations’.

Done.

L117 I think you can already refer to IR1 and IR2 in line 112

Done.

L129 ‘ALS elevation data ARE lacking…’

Done.

L131-132 An ‘alteration between grounding and ungrounding’: this sentence confused me, because earlier on you say that this area has ungrounded. Do you mean ‘episodic’ grounding and ungrounding?

Yes, we want to say, that the location is not switching back and forth from grounded to ungrounded to grounded again. We have rephrased it to: An episodic change between grounding and ungrounding seems to be rather unlikely based on our data.

L131-134 I would fold this into the discussion above, where you provide evidence from other sources. I don’t see why the radar data should be treated separately.

This is a very good suggestion. We have now first presented the observations and draw subsequently the conclusion.

L137 ‘…result from two processes’ instead of ‘…from two instances’

Done.

L139 Unless I’m looking at the wrong feature in Fig4 (distance along track ~2.4km) it appears to me that the difference in surface elevation is about 7 or 8 meters?

You are definitely looking at the right image, but we compare the area outside the grounded spot to infer the thinning. The grounded spot was a domelike feature and once moved the elevation change is 7-8m, but to get information about thinning the ungrounded spots are the one to look at.

L142 You suggest surface melting as a process. How about dynamic thinning?

We divided the causes into an advection process or local thinning. As there is no evidence of a velocity change of the floating tongue in that area, but we find massive surface melting to take place (see the surface elevation change at the ice rises, also mentioned in the text), local thinning is definitely taking place. This is our way to identify local thinning to be the cause.

L146 ‘In addition’ instead of ‘Next’

Done.

L156 ‘…is similar in summer 2022,…’

Done.

L166 when you refer to the stresses in the floating tongue, say that this will be discussed later.

Done.

L176 How did you estimate components of the stress tensor? From ISSM?

Exactly, we used the remote sensing velocity product to conduct an inversion in ISSM. We have added to the sentence that we do this by means of inverse modelling.

L176-180 Can you show the evolution of the stress field as the calving front & rifts develop between the 1990s and 2020s? So reproduce Fig 6 but for different snapshots in time? You might find evidence in the stress field that could help explain the changes in rift propagation direction (or ‘calving style’ as it is referred to in the paper) after 2014. The following work might be of interest in that context: https://tc.copernicus.org/articles/13/2771/2019/.

This is exactly the problem here: the coverage is not sufficient to have an inversion before / after the rift evolution. We discuss this also in more detail below. We would have wanted to go exactly the route as in that reference, but the data coverage is just too poor for that. The TerraSAR-X supersite was ‘just’ the grounding line area, which is repeatedly covered, but no such coverage is available of the ice front in those years.

L183 The propagation into an area with lower stresses might well lead to the arrest of the rift, although the

behaviour also depends on the spatially heterogeneous material properties (ice thickness, critical stress intensity etc.) so I think a more nuanced statement is needed here.
The change in critical stress intensity factor is very unlikely over this short spatial distance - there is no indication that the density is different, or other factors varying over this short distance. The ice thickness is higher closer to the ice rise and has then a constant ice surface elevation over the length of the crack in a TanDEM-X DEM. If the thickness difference would be the reason for crack arrestment, the crack should have been arrested earlier. Nevertheless, we do not have any measurement of what exactly lead to crack arrestment, therefore we rephrase it to 'which is likely the reason for why the crack stops propagating'.

L182-184 Inferences about the type of fracture are provided as facts, but some explanation that links back to the explanation of the different modes of fracture in L46 etc. would make it easier to understand this paragraph. E.g. why is crack D 'clearly' a mode I crack?
Indeed we only threw a half sentence into the air, which was not adequate - apologies for that! As we only have the stress field before the occurrence of crack A, we cannot draw any direct comparison to the principle stress fields. That crack D is a mode I crack thus comes from the form of the crack opening. This is visible in Fig1c and clearly shows that the crack faces are moving apart like a mode I crack would do. We will include this in the new version of the manuscript. We are indeed very grateful that you mentioned this, because our text was very incomplete.

L185 '…along SURFACE rivers or lakes…'
Done.
L198-205 I find this paragraph hard to follow. Do you infer evidence for tidal bending at the location of rift E from the density of fringes in the interferogram? If so, I would expect there to be tidal bending -and hence 'fatigue cracks' - in many other locations with a similarly strong phase gradient in the interferogram. Can you explain this in more detail in the manuscript please?
It is a very good idea to include more detail in the manuscript! If the tidal bending would be large, crack E would have already become critical. In the interferogram one does indeed see a small change in phase, but no real fringe belt. Fatigue cracks could indeed very well be found at other locations of cyclic loading on short time scales. As cracks are often formed upstream the grounding lines already, and hence the grounding zone is often a zone of crack propagation rather than initiation. It might be difficult to distinguish between old mode I cracks and new fatigue cracks, but is would be a very interesting study indeed.

L206 pre-existing
Done.
L218 'We further consider THE potential future evolution OF the calving front'
Done.
L219 'will detach' instead of 'detaching'
Done.
L234 'was last observed' instead of 'disappeared'
Very good suggestion!
L236 'area' instead of width
We replaced length with area.
L238 What do you mean by a 'normal' calving front?
The WIS ice bridge had also two calving fronts, one supported by a thick ice melange, while the other had only open ocean or winter sea ice and thus no support.
L241 remove 'from south' before the reference to the figure.
Done.
L242 'A' bulging zone. Also refer to the relevant figure here.

Done.

Fig1 caption. In the description of panel b, refer to IR1 & 2 when you talk about the pinning points.

Fig1b shows all pinning points, not only IR1 & 2. Therefore, we would suggest to leave the caption of thins panel as is.

Replace ‘marked in blue’ by ‘shaded in blue’

Done.

Fig3 caption. Is ‘The location of the profile is presented in Fig 1d’ needed here?

We do think that it is important to mention where the profile is actually located and suggest to keep the reference. In the revised version we moved this sentence to the end of the description of panel (b), as the panels © and (d) are not showing data from that profile.

Fig5 caption. Can you indicate the location of the radar profiles?

Yes, we added a reference to Fig.1d now.

Fig6 caption. ‘scale bar’ instead of ‘scale bare’

Done

L267 ‘larger’ instead of ‘more dramatic’

Done

L276 ‘predominantly’ instead of ‘in the majority’

Done

L282 is 0.8% correct here?

Thanks for catching this, the number is not correct. We have now corrected the numbers.

L284 ‘significant increase’ or ‘166% increase’ instead of ‘massive’

Done

L288 ‘First we discuss the WIS, which has …’

Done

L292 ‘…15% of area…’: do you mean the area of the ice bridge?

No, it was the ice shelf area, the ice bridge collapsed entirely. We included ‘ice shelf’ now.

L289-294 The comparison based on broad-brush similarities in the geographical setting for both ice tongues seems rather speculative, given the detailed stress arguments that are invoked in the remainder of the manuscript. Is there any strong evidence from the literature that ice tongue area and other geographical descriptors such as lateral confinement, are good metrics to determine the vulnerability to collapse?

We are convinced that it is not speculative. The ice bridge on Wilkins Ice Shelf was astonishingly similar to what we find on 79NG. Yes, you are right, that there are different levels of detail in the arguments in the different sections/topics, but we would be reluctant to leave this comparison out. Actually, we instrumented the ice bridge with an autonomous GPS.

L295 ‘retreated past A lateral embayment’.

Done

Also: it is not clear to me what is meant by a lateral embayment. Can you describe the geographical setting in more detail?

In Csatho et al. (2017) it is called bay, in Johnson et al. (2004) ice lobe. We added now ‘of the fjord’.

L307-310 Again, this sounds rather speculative. Do you have any evidence that the stress field changed significantly between calving events, i.e. once the ice front retreated beyond the pinning points?

We do not understand what you are referring to. Do you refer to 'Once a change in the calving front situation occurs the elastic stress redistribute instantaneously and can trigger further follow up events as well as lead to a modified viscous response over month–years, see Rankl et al. (2017) for WIS.'? The publication of Rankl et al (2017) presents stress in Fig. 4. So, yes, we have evidence that the stress field changed significantly at WIS.

L300 '…far easier to retreat past this area': Do you have a reference to the published literature for this statement?
We hope we understood the request correctly: you are asking for a reference for the statement about the Jakobshavn Isbræ. We included a reference to Csatho et al., 2008.

L320-325 The discussion about air and ocean temperatures is rather detached from the remainder of the discussion and reads like an afterthought. Can this be integrated better, e.g. by saying that you consider the different external drivers that might have contributed to the changes observed at the ice front, and then systematically discuss the different possibilities?
This is a great idea. We start the paragraph now with 'Next we consider different external drivers that might have contributed to the changes observed at the calving front, such as air and ocean temperatures.'

L329 '…crack E is none of these but 'COULD BE' a tidal fatigue crack…'
Done
L330 'speculate' instead of 'interpret'
Done
L334 'long' instead of 'large'
Done
L339 The use of terminology 'transition from stability to instability' is misleading in the context of dynamical system theory, since no stability analysis was performed in this paper. Please consider replacing this statement with 'is potentially at the onset of a major retreat phase' or similar.
It is correct that it is misleading in the context of dynamical system theory, but we never draw a link to dynamical system theory. The great works of Chris Doake often discussed the stability of ice shelves, also without any dynamical system theory context. However, we are happy to replace the text as suggested.

General comment about the discussion section: I'd like to echo Reviewer 2 and say I'm missing a more in-depth discussion about the model results. Besides the changes in GL flux, it would be interesting to explore the underlying reasons for the modelled behaviour. For example: if I understand correctly, your model is initialized with velocities prior to the formation/propagation of some of the rifts. Since you simulate a 5% increase in GL flux as a result of calving following the formation of the rifts, I wonder if that is consistent with observations?
You are right in saying that we initialise with velocities prior to the rift formation, but the first experiment calv2iceberg detaches also the ice bridge and this has not yet happened in reality, thus no observational comparison is possible at this stage. Indeed, once that happens the change in velocity will be the way to benchmark our results.

Can you also provide some further intuition about the changes in the buttressing parameters, e.g. what is the reason for the large areas of unbuttressed ice shelf in the calv2fjord experiment? Would one not expect shear contact with the lateral mountain slopes to provide some amount of buttressing?
The snapshots in time that we are looking at here, include (1) the initial state, a state in which the ice shelf is pinned along the calving front and (2) a state without pinning of the calving front. While in case 1 the velocity gradient in flow direction is negative - the velocity is getting lower towards the ice front, in case 2 the velocity towards the calving front is having little gradient. The change in lateral shear stress has less

influence, than the change in longitudinal stresses. The change in stress in along flow direction is what reduces buttressing.

In L102 you state that “Based on this variety of datasets we aim to investigate whether recent changes of the 79NG configuration are indicating a regime change, what exactly is the cause of the changes and how this will impact the stability of 79NG.” yet I find little direct evidence in the text for the ‘cause of the changes’. Are you referring to the ungrounding of the pinning point? If so, I think stress maps at different times before/after the ungrounding might help you identify how the glaciological conditions have changed, and how this has impacted on the preferred direction of fracture propagation.

We fully agree that this would be what one wants to see, but it is unfortunately not achievable. We have looked into every available velocity dataset, have tried to compute velocity fields from all missions we could get data from, but the coverage is poor and the scatter in the velocity fields from Landsat so large, that one has to smooth the fields that much, that one cannot use it for our purpose. During writing the original manuscript we have spent really a lot of energy on that and finally concluded that the one we present is the only dataset we can use for the stress field.

We think that in future, phase field modeling can be used to investigate this further, but currently phase field modeling for long time periods is still quite tricky. Sondershaus et al. (2023, https://doi.org/10.1002/pamm.202200256 ) is an example for such an approach, but lacks large deformations. However, that is the route to go to compare stress fields for different boundary conditions like grounded/ungrounded and crack formation. We will be working on this in future and hope to submit a manuscript on this in the next years.