We would like to thank the reviewer for their insightful remarks. We note for the reviewer and the editor that there are many new additions and insights provided by this study, as compared to prior studies, of which the reviewer makes note of several. We found the reviewer's comments to help us identify places where we can further highlight the new insights provided by our novel experimental design, and that the revised paper (which is already mostly completed) will be satisfactory to address the reviewer's concerns. Below is our initial response, broken down by remark original review comments are shown in black, while our answers are shown in red.

*Novelty

I think the main new addition in this paper is the parameterization of ice melange effects. The authors do that by making calving stress threshold a linear function of ocean temperature. It is not clear how significant this addition is, as there is no comparison to simulations without this feature. Previous studies using the same model (Bondzio et al) were able to reproduce SK evolution just as well, so it seems that the proposed form of melange parameterization is not necessary for reproducing the past. This is a major issue for the claims and findings in this paper with regard to the importance of melange to SK dynamics - essentially there is no control run presented here to evaluate this importance against.

First off, we thank the reviewer for showing how our framing of this study was insufficiently clear so as to produce some misconceptions about our aims. To be clear: our aim throughout this study was to explore the drivers of SK's retreat during the period of 1985-2018. Additionally, we show that we can capture SK's evolution well without forcing the front with observed velocities as in Bondzio et al. [2017]. Our aim was not to accurately simulate mélange (which is not currently possible with current modeling systems, though the efforts are underway from the authors to remedy this), but to offer a scenario where disentangling SK's retreat can be accomplished with the considering of long-term changes in mélange buttressing of the terminus through modification of the calving threshold parameter. We place our results in context of Bondzio et al. [2018], thus making those prior simulations akin to a "control" that we compare against. Importantly, by including interannual variability in the calving threshold parameter driven by ocean forcing, we do not require 4x multipliers of frontal melt, as Bondzio et al. [2018] did to achieve reasonable match between observations and simulations. Our aim was to present a different perspective on how ocean-temperature-based processes could modulate calving in the context of SK. Given the central importance of SK to discussion and parameterizations of climate forcing of dynamic glacier retreats (i.e., calibrating ice cliff wastage parameterizations), our results provide an important advance showing the central importance of interannual changes in calving to explaining SK's observed retreat without resorting to unphysically high frontal melt rates. We strongly believe that by clarifying these aims, the reviewer's concerns about the importance of this work should be addressed. We have significantly re-written the introduction to highlight these aims more clearly.

*Goals and Scope

*The paper needs to better clarify the questions it is addressing and how the answers it comes up with support, or contradict what is already known about the SK retreat.

Our main question concerns the rapid retreat of SK and how melt and calving processes could have driven its evolution from 1985-2018. Beginning in 1998, the rapid retreat SK's calving front initiated a runaway mechanism wherein mélange could have played a large role in modulating calving rate. We produced a large-ensemble where we could compare relative contributions of melt and calving processes in the presence of weak and strongly sensitive mélanges. We mainly note that we do not need unphysically high melt rates to recreate SK's evolution but that beyond the period of retreat we notice a rapid runaway effect, suggesting that the calving dynamics which would accurately describe 1985-2011 evolution may not be suitable for later periods. We will clarify on these questions we are addressing in text.

There are multiple papers by Bondzio et al that concern SK, and I believe the authors here use the same model and try to build on this work. One of the papers of Bondzio focuses on disentangling drivers and mechanism of the retreat. How does your study compliment this work. Or other existing hypothesis of the retreat?

Bondzio et al. [2017] aims to explain SK's evolution from 1985-2015 using a 3-D thermomechanical model configuration. In that study the evolution of SK is forced using observed calving front positions and the authors find that "the viscosity drop, the trough's low basal drag, and the geometrical adjustment are the main drivers behind the observed inland acceleration of JI". Following up on that, Bondzio et al. [2018] to identify the ocean's role in SK's present and future mass loss. In that study the authors find that a seasonal calving cycle and ocean-temperature forced melting accurately capture's SK's behavior. Specifically, Bondzio et al. (2018), use a dynamic seasonal calving cycle which switches calving thresholds between 4MPa in the winter and a prescribed summer stress threshold which is varied from 60-180kPa. They find that models with summer stress threshold of 150kPa most accurately captures observed behavior, with the caveat of frontal and submarine melting having to be amplified to 4x and 2x respectively. Since Bondzio's studies came out, substantial importance has been placed on the role of calving in describing the very fast retreat at SK, and whether the ocean has a role in explaining changes in calving (e.g., Joughin et al. 2020). Therefore, there was a need to further study whether interannual variations in the propensity for calving (through sigma_max) explain (even partly) why calving has increased quite so much as SK. This is where our study comes in: we follow up on this work by addressing whether we can capture SK's behavior without employing such large melt amplifications and find that we can do so by allowing ocean-temperature calving dependence. We will contextualize our study more clearly in the aforementioned context and include a relevant section in methods.

The authors write "there is still debate about which physical processes are responsible for mediating the glacier response to ocean warming" but the reader never learns about what the debate is and how this study contributes to this debate.

We will highlight and clarify in the text the following hypotheses

(1) Processes occurring at tidewater glacier termini act to trigger a retreat and reduce buttressing stress. These processes are most commonly calving and mélange buttressing. Calving is further amplified by basal over deepening and retrograde sloping beds while mélange is primarily generated via calving and experiences a seasonal variability in response to fluctuating sea and air temperatures.
(2) The second hypothesis suggests that enhanced surface melting in response to warmer air and ocean temperatures initiated ice tongue collapse and subsequent inland ice velocity acceleration.

How about the papers of Nick et al. and similar, they seem to be relevant to this paper.

Nick et al. (2009) report that Greenland's outlet glaciers "adjust extremely rapidly to changing boundary conditions at the calving terminus." Additionally Bondzio et al. (2017) further affirms calving-front dominated evolution in the context of SK. This reinforces our findings in that we recognize that the calving front dynamics are a much greater control on retreat than submarine melt processes. We will include mentions of these studies in text in addition to a clearer contextualization of our study.

For an example how to clearly state hypothesis and place your study in existing context, see for example Bondzio et al. 2017: "The mechanisms behind Jakobshavn Isbræ's acceleration and mass loss: A 3-D thermomechanical model study"

We will re-introduce the hypothesis following the points made above.

*Methods

There is no information about the temperatures used to prescribe melting. A mention is made about ECCO, but no detail what so ever about how are these temperatures propagated/extrapolated into the fjord, whether topography, namely the sill, is taken into account during this extrapolation. Some comparison of existing vertical temperature profiles with whatever vertical temperature profiles the authors come up with would be good to show too.

We simulate the process of submarine melting by assuming that the water column is stratified such that water at maximum depth is also the warmest water. We use this assumption to justify the warmest waters overcoming the shallow sill. Following this, we take the depth-average of the water column's temperature between the surface and maximum of bedrock and sill height (-250 m). We fill the data gap between 1986-1992 by repeating the temperature time series between 1992-1996. We will incorporate this information in the submarine melt section of our methods.

Clarify when is frontal melt prescribed. Only when there is no floating tongue? Or also on the vertical calving front of the floating tongue? If the latter is the case, then how do you account for the effect of convective forcing due to meltwater from basal melting emerging at the front - do you have an estimate for that?

We do not estimate or account for the effect of convective forcing. Frontal melt is applied to the front of the glacier and only to submerged areas. Submarine melt is applied to all ice below sea-level on the ice-ocean boundary. We do not account for convective forcing due to meltwater, because the sensitivity to subglacial discharge is lower than to ocean temperature (as in Xu et al. 2013). We will further highlight this fact in the methods.

There is no information about which parameters are varied throughout the sensitivity studies. These parameters then appear, unexplained, in figures, but they are nowhere introduced.

While the previous version does provide an explanation of the various parameters, there was some inconsistency in symbols and explanation across the text. We will fix this by standardizing symbols across text and figures and summarizing the parameters perturbed in the ensemble at a single point in the text.

*Validation

There are some strange unphysically looking calving front positions that may indicate problems with the calving algorithm. Specifically, I am referring to the appearance of two structural arches, well before the glacier splits from one through into two (I would give you a location but there are no axes on the plot). Can you explain this?

Yes we agree that remnant ice is an artifact of our calving algorithm. However, the features that the reviewer has identified here are transient and we thank you for pointing it out. However, these are transient features which do not appear in all simulations. Even in simulations with the some overall retreat history, few have these features and others do not, and so we discount their overall impact on the main results of this study.

None of the simulations appears to be able to stabilize and readvance once it has begun to retreat, unlike what happened in observations around the 2017 cooling. This indicates that the model is more eager to retreat than in reality. This needs to be addressed.

The reviewer makes a good point here, we refer you to our discussion on lines 336-345 in particular we note that our simplistic calving tends to prioritize retreat when we select for the lowest RMSE model. This is due to the observed rapid retreat of the calving front in the 2000's which favors models with parameter combinations that allow for a rapid retreat during this period. However, we miss an important negative feedback on calving in the form of mélange which would inhibit potentially slow or fully arrest model runaway retreat as has been previously identified by Khazendar et al. (2019) and Joughin et al. (2020) as an important factor in describing the readvance in 2017. We will further highlight this shortcoming in the discussions section.

*Results

The results are not in general supported by figures. There needs to be a clear separation between results and discussion/interpretation and 'general knowledge' statements which assume that the reader is inclined to agree with the authors. All results should have figure references, which is currently almost never the case. This is a major issue in assessing the manuscript contribution.

We thank the reviewer in pointing this out and we will address the lack of in-text figure references by providing relevant references to claims we make in the discussion and results section.

There is no stress quantification that would support several claims and arguments by the authors.

We will include a contour plot of stress field for the best fit model with snapshots at relevant points in time: Initial, Disintegration of ice tongue, Initial calving acceleration and Final times.

The results seem to suggest that the main ice tongue disintegration occurred via basal melting and not by calving (Fig 3a). Is there observational evidence for that? I was under the impression that there were some major calving events that diminished the floating tongue, rather than in just melting off.

While submarine melt was the primary source of mass flux during the disintegration of the ice tongue, we do not want to imply that as the sole reason for ice tongue collapse in our model. Observational evidence shows that a series of calving events and consequential front retreat occurred during periods of looser ice mélange and heightened water temperatures in waters proximal to where the fjord enters Disko bay. Hypothesis concerning ice tongue collapse focus on 1)hydrofracturing induced by surface melt filling crevasses and 2)ice mélange weakening influencing SK's terminus stress state by reducing frontal buttressing and consequently facilitating calving activity. We will include these hypotheses for ice tongue collapse to further contextualize and reinforce our assumption in SK's evolution being largely driven by frontal dynamics.

*Hysteresis

The authors run experiments with different calving stress thresholds into the future(? unclear what temperature forcing they are using). And then claim the system experiences hysteresis, but they don't actually do experiments with reversal of the forcing conditions. While it seems perfectly plausible the system experiences hysteresis, it was not actually shown in this paper, and as such it remains a hypothesis.

We thank the reviewer in pointing this out and we will elaborate on our methodology. We conduct our hysteresis experiments by taking the best fit model and holding temperature forcing (cold ocean conditions producing low frontal melting) at its 2018 configuration. Since we assume that calving is the dominant control on SK's evolution by 2018, we vary the 2018 stress threshold from 1-10x for a total of 20 models. We term these "2100 runs" and were interested in whether we could see simulated SK readvance under these conditions. Thus, we do conduct a hysteresis experiment in that we reverse the calving parameter to much higher values, including those previously attained when calving rate was low, and even above those values. The point is that the evolution of the system during the period of low sigma_max causes it to enter a state that is not reverse when sigma_max is returned to these values or even above these values. This fits the definition of hysteresis (e.g., see the classic textbook on dynamical systems by Strogatz, p. 60). We will include this information in the results and further discuss their implications and shortcomings in the discussion section.

*Discussion/conclusion

There are some relatively strong statements at the end of the discussion and conclusion sections advocating for a fully dynamic and coupled model of melange. I don't see how any of this need is supported by this paper. What was done here is only a very simplistic parameterization of calving rates on ocean temperature, and as admitted, no feedbacks between melange strength and calving rate were incorporated. There is probably still room for more sophisticated parameterizations (e.g. following Schlemm et al.)

We thank the reviewer for pointing this out. We will add further discussion of mélange parameterizations such as that of Schlemm et al., which should help acknowledge some of the deficiencies that come from not representing mélange in ice sheet models. However, such simple parameterizations are ultimately meant as upper bounds since they do not represent the true rheology of mélange. New approaches using ideas from granular physics (i.e., Amundson et al., Currently Accepted at TC) to dynamically model mélange are necessary to accurately predict the buttressing force of mélange, and its interactions with the ocean and glaciers. We ultimately wish to use the results of this study as a motivator for why we must develop more robust melange models which would hopefully be integrated into regional ice sheet models. Especially since we note that terminus dynamics impose a significant control on overall glacier dynamics, it is imperative that we develop a proper representation of the seasonal and calving-associated mélange dynamics. We will flesh out the discussion of mélange parameterizations and models in more detail in the text in response to this suggestion.

IN LINE REVISIONS

INTRODUCTION

"I don't see what you mean by this and how that has anything to do with retreat"

Here we are referring to the ice tongue's ability to buffer melting near the grounding line by acting as a heat sink for warmer fjord waters. We will clarify this in text.

see Gladish et al. 2015: Oceanic Boundary Conditions for Jakobshavn Glacier. (Part I and II but mainly II) The oceanographic analysis there shows that the suggestion of Holland at el 2008 about warm waters actually making it to the Illulisat Fjord is actually quite simplistic and although widely accepted it has a lot of caveats.

We recognize that while warm water may not have directly reached SK's front, we keep consistent in our assumption of intrusion as to compare to Bondzio's study. But we will include references to highlight the possibility that warmer waters may not have intruded as deep as assume here and make explicit mention of this in-text.

what is the debate and which are the relevant papers on that?

We aim to identify whether SK's observed retreat can be attributed to an amplification of melt due to increased water temperatures or a reduction in buttressing due to a weakening of pro-glacial mélange leading to amplified calving [Thomas et al., 2004]. We refer the reviewer to our comments made earlier in the overview regarding our hypotheses and the relevant debate. We will clarify these hypotheses in the introduction and are thankful for the reviewer's input.

during which timeperiods did those occur? Is it based on direct temperature measurements in Disco Bay as are the data from the more recent time periods?

We will remove the sentence.

Do you model this period though? When does this warming occur? I don't see any warming in Figure 3c.

We will correct this sentence to: "why did the most recent period of warming beginning in 1985 cause such a dramatic and unprecedented retreat?"

But there are also counter examples where these aren't at all related, see Amundson et al., 2020, Le Conte Glacier

While counter examples do exist, we note that here our analysis is strictly contained to SK. However, we will make explicit references to these cases and hypothesize why it might be more important in the case of SK (melange and frontal melt) than Le Conte.

Maybe cite Xie et al for SK melange dynamics observations?

We will add a reference to Xie et al., 2019

what is direct melting, as opposed to indirect?

We will fix to "Disentangling the drivers behind SK's response to warming ocean conditions requires distinguishing between retreat driven through **ocean-induced melt** and calving."

why are icebergs an issue? which observations do you have in mind that would solve the problem?

Observations of environmental variables such as mélange density, detailed calving catalogues, geometry of calving front. Icebergs act as physical obstacles which may complicate data collection methods, especially near the glacier front. We will clarify why these observations are relevant and their associated difficulties more clearly in text.

calving rate? or threshold?

We will fix to: We perform a large ensemble of simulations of SK retreat through perturbation of three sensitivity parameters that impact its retreat: subshelf melt, melt at the calving front, and **calving threshold** modulated by mélange rigidity.

METHODS

The variability of which quantities are you actually comparing? If SMB and temperatures then that is not meaningful - perhaps 'small' SMB variation can produce an equal effect on submarine melt as 'big' ocean temperature variation.

Here we are pointing out that the relative variations in SMB are nominal relative to the relative variations in ocean temperature. We make this assumption as we are interested in disentangling ocean-driven mechanisms. We will clarify this in text.

which ones specifically?

Here we are referring to surface mass balance forcing. We will clarify this in text.

where? local? remote? This needs some elaboration.

Here we are referring to local surface mass balance forcing. We will clarify this in text.

more than what?

We will fix to: "…be more influential in it's role modulating ocean conditions **than it is in effecting SK's evolution via direct variations in SMB**."

can you give examples of these processes?

We will fix to: "The stress threshold parameter can be thought to conceptually represent many physical processes **such as fracture toughness, grain-scale deformation, and ice strength**, which have the ability to modify the propensity for calving events."

explain each symbol in text

We will add in-text explanations for each symbol.

this is sigma_min? or min(sigma_thr)?

This is the minimum stress threshold which corresponds to the highest temperature in the respective disko-bay temperature time-series. We will clarify this in text.

what is the max value?

The max value is the parameter we vary, in effect we are varying the calving threshold's ssensitivity to ocean temperature. We will clarify this in text.

But when there is floating ice tongue, part of the basal melt water that emerges at the calving front in a way acts as a subglacial discharge in that it produces a convectively forced plume rather than an unforced one. So that may need to be accounted for?

We note that while this is a potential short-fall of the frontal melt parameterization, beyond the removal of the ice-shelf, we would not need be concerned with convective plume forcing, at least within the context of our study. However, we will make a note of our omission of plume dynamics in our methods and are appreciate of the reviewer's point.

Do you mean basal, as opposed to frontal?

Submarine melt acts on floating and grounded ice exposed to ocean water (below sea level). We do not include basal melting processes in our simulations and will clarify this in the methods.

what do you use for gamma t? presumably that is a function of friction velocity - do you assume constant? I think this paper needs a table with symbols, their meaning, and values for constants (and references where they are taken from if that applies)

We assume the specific heat capacity of the mixed layer is constant and use the same value as in Bondzio's relevant work. Additionally, we will add a table with relevant parameters and their values.

how do you get this value? Is that just directly take from ECCO?

We use a constant melting point value of -1.85 C as an averaged value obtained from the equation of state with pressure and salinity values obtained from ECCO. We justify using a constant melting point value in that we expect its variation to be an order of magnitude lower than variations in ocean temperature. We will clarify this in text.

I think the methods section could use a summary of what are the three parameters that you are actually varying. Or state it somewhere else, but should be really clear and explicit.

We added a table with relevant parameters and their values. We also clarified the three parameters which we vary with a summary sentence at the end of the methods section.

what does this mean? what are you actually varying in the parameterizations? some factor that is not included in those equations? If that is the case, please include it

We vary the magnitude of melt obtained through empirical parameterizations through a simple multiplier parameter. For example, our parameter space for submarine melt would include 0x, 0.5x, 1x, 1.5x,… the base parameterization for submarine melt. We will clarify the distinction between a parameterization of a process and multipliers we use in our parameter space in text.

put axes on plot, or coordintates

We will add axes to the plot.

Can you mark when in observations and when in simulations the glacier looses the floating tongue? It is not clear from the calving front positions, as I assume those are either floating or grounded.

We will add in text reference to grounding line location when SK loses its ice tongue. Additionally, we will include a supplemental figure showing grounding line locations for our best fit model.

That is not quite true. You want to be able to capture quiescent times as accurately as retreat times.

We note that our aim is to capture quiescent as well as retreat periods, we are just highlighting that weighing observations based off observation density would not affect our analysis due to periods of rapid retreat coinciding with the highest observational density. We will expand our reasoning as stated here in text.

RESULTS

I am not sure what this whole segment is - is this results? If yes, then there need to be figure references associated with each statement, and supporting it. Is it introduction? - in that case the statements need to be referenced. Is it discussion of results? In which case it should be placed after the results that are supported by figures.

We thank the reviewer for pointing this out and we will Include specific figure reference to support our results.

I don't see any 'pause' in the simulated extent once the glacier starts retreating (yes it hangs on a bit at the initial position)

We will clarify in text that behavior of arrest at pinning points is behavior witnessed across ensemble members and not specifically only within our best fit model.

Except for the abilty to readvance

We discuss the issue in capturing re-advance later in the discussion.

why don't you just include it in the plot?

We will update the plot to include the entire ensemble

Can you explain which part of the figure and what region you are referring to?

We will clarify in the text that we are referring to the area of the parameter space within a high maximum stress threshold regime and a lower submarine melt multiplier.

what is that - how do you define it (the cold ocean part), and where did you conclude this from? which portion of which figure?

We will reference Figure 2a) in text, and re-clarify earlier and within this section that the maximum stress threshold parameter is equivalent to the stress threshold when the temperature is at its lowest within the time-series of ocean temperatures.

What is the red dot in panel b?

We will clarify in Figure 2 caption. The red dot indicates the model parameter combination which achieved the lowest RMSE.

Is M_s and M_f the same thing as m_sm and m_fr in text? Can you just it make it self consistent?

We apologize for the confusion and will clarify and standardize symbol-parameter combinations through the text.

explain legend what is alpha and other symbols used in the figure

We will fix the legend to indicate alpha as the multiplier to our parameters.

which parameter are you changing?

Here we are not changing a parameterization of melt, but we are referring to increasing the submarine melt multiplier. We will clarify this in text and will keep consistent in parameter/multiplier usage in text.

Are you actually testing different basal melt rate parameterizations? If not, how did you conclude this? If yes, which ones?

Here we are not testing different basal melt conditions instead we are making the point that a submarine melt multiplier <1 is required for models to achieve a lower RMSE. We will clarify this in text.

Again, point to figure

We will reference Figure 2 here and point the reader towards subpanels specifically to note relationships between parameters.

to compensate what?

We will change in text to: "…such that simulations with higher cold-ocean stress thresholds (i.e., less calving in coldwaters) also need higher submarine melt rates **to achieve reasonably low RMSE**"

You haven't define what Mf is and how it enters which equation

We will define what Mf is earlier and keep consistent through text.

I am a bit intrigued by this plot. Are you saying that calving did not at all contribute to the SK retreat? so the changes in the calving positions during that time are just a result of the frontal part of the ice tongue melted off? How does that compare with satellite observations?

We will include supplemental info plot to show initial stress state and as calving picks up as evidence for why calving fluxes become significant towards the beginning of 2010. We agree that calving resulted in the loss of the ice tongue and we highlight later the shortcomings of our simple parameterization of a temperature-based calving threshold. Specifically, since we are interested in the model with the lowest RMSE, we require a larger stress to accommodate later periods of retreat which may make the initial breakup of the ice tongue less dependent on large calving events as noted in observations.

Something like a plot of ice shelf basal area time series would be helpful here - to determine when the ice tongue is lost and why different terms gain importance at different times. And also timeseries of ice front area for the same reason.

We will include relevant figures of ice shelf basal and frontal area in the supplemental information.

simulation?

We will correct in text to "Best fitting model"

in observations or in simulation?

We will clarify in text that this paragraph is referring to observed ice tongue behavior not simulated behavior.

why? is there an evidence of seasonal cycle in temperatures inside the illulisat icefjord? I think you claim that subglacial discharge is not important elsewhere in the manuscript, but if it induces a significant seasonal cycle in melt rates, can you maybe clarify/remind the reader here?

We make this claim with respect to the observed behavior of SK's ice tongue, before its collapse. However, we thank the reviewer in pointing out our omission of calving as a component of balance during the period of 1985-2000. We will include further clarification highlighting that ocean melt AND calving were in balance with incoming upstream ice flow and surface accumulation pre-collapse.

that is a result from simulations? of that is interpretation of observations?

This is an interpretation of observations. We will clarify this in text.

I dont understand what you mean by this and how that buffers retreat

Here we are referring to the ice tongue's ability to buffer inland ice from warmer surface ocean temperatures but not necessarily melt at deeper depths. We will clarify this in text.

Where, local in the ice fjord? or local further on the shelf?

Local to the ice fjord. We will clarify this in text.

Add some measure of that into the time series?

We will include a supplemental figure indicating stress state at key points in best fit simulation.

ok, here it would be good to have some time series indicating the ice tongue base steepening

We will include a supplemental figure indication glacier profile at key points throughout our simulation.

which part? the one with 1 branch or the one with 2 branches?

We will change in text to "southern branch".

Same comment as before, this whole section needs figure referencing

We will include more in-text figure references to evidence claims.

how did you determine that

We will reference Figure 2 and note that the by 2010, SK has retreated into a much deeper and narrower bed topography.

which type of melt?

We will fix in text to submarine melt

Add point on figure indication loss of ice tongue

We will mark Figure 3 to indicate time point of ice tongue collapse.

Horizontal only, correct? You don't take into account vertical melting profile and its role in calving, right? Can you make that explicit somewhere

We will clarify earlier in the methods that we don't take into account a vertical melting profile and that melt acts horizonally.

cite them

We will include citations to studies highlighting potential hysteresis at SK.

We will fix the typo in referencing Kajanto et al. (2020)

what about frontal melting?

We will include in text, "driven by calving fluxes **and frontal melting** as floating ice area considerably decreases and the effects of submarine melting are diminished."

but that doesn't seem to be so consistent with observations, in which the ice front position is somewhat stabilized, definitely in the northern trunk

This is not consistent with observations and highlights the lack of a necessary negative feedback to address runaway calving within our model. We will clarify this inconsistency in text.

cite

We will include citations referencing high mélange output due to rapid calving

How do you conclude this, without actually doing tests with temperature reversal?

We will make a point in text to highlight that we are not reversing temperatures, instead we are setting ocean temperatures fixed to their 2018 state and focus our hysteresis analysis purely on increasing calving stress threshold. We refer the reviewer to our earlier response addressing hysteresis in the reviewer's overview.

DISCUSSION

which ones?

We will fix in text to "…that the Budd(1979) basal sliding law and coefficients captured SK's behavior fairly well. And include a reference to Budd et al., 1979

These were never introduced

We will introduce and define and keep consistent parameter symbols.

multiple factors larger than observed.

We will fix in text to "…fronts which closely match observed calving fronts without requiring melt forcing to be multiple factors larger than suggested by our empirical parameterization."

the only way for your model and for these particular parameterizations

We will fix in text to "The extended simulations described in section 3.3 indicate that the only way **for our best fit model** to arrest the future retreat of SK…"

where did you show that?

We recognize that we do not show this and we will remove the following text to "The extended simulations described in section 3.3 indicate that the only way to arrest the future retreat of SK is by increasing the calving threshold to values never attained during the historical period in the best-fitting simulation. ~~period and not achievable by changes in ocean temperature alone~~."

While this would be great, I don't think having a dynamic model of the melange, is not any more necessary than having a dynamic model of the ocean. You seem to be comfortable with a simplistic parameterization of the ice-ocean interactions, but these may be just as important (or more) to simulate, than the melange dynamics.

We thank the reviewer for making this point and agree that while our parameterizations are simplistic in nature, they still highlight that without some form of dynamic calving in response to ocean conditions we would need much higher melt rates than suggested in order to achieve a reasonable match to observations. This is compounded by the fact that had warmer waters not intruded into Disko Bay as far as assumed then local fjord conditions would represent cooler waters and require even higher melting rates than assumed by Bondzio (2018). We thus use these reasons to assess that a dynamic mélange component would provide the greatest constraint on the evolution of SK compared to other ice-ocean processes. We will further clarify this in text.

Fixed typo "and very computationally expens**ive** tools"

CONCLUSIONS

Do you actually use vertical temperature profile from ECCO in Disco Bay proxy, or do you just use a single temperature point? Do you take into account the sill discussed in Gladish et al? The first time we here about deep water in the manuscript is in conclusion!

We take into account the sill when building our temperature forcing time series. We limit temperature forcing to depths above the sill and take the average of the water column as a single point. We will include this in text.

Not sure what you are referirng to here and why it is important and what is the reason for that.

We will move this sentence to the end of paragraph 2 in conclusions to highlight the error arising from our simplistic ocean temperature based calving.

All this should have appeared in discussion at some point and been elaborated on there.

We will move these sentences to the hysteresis section of our discussion.

Can you mark that bimodality on figure 2 in some way?

We will include figure showing a histogram of RMSE along our flowline in the supplemental information.

do you mean metastable?

We thank the reviewer for the suggestion but believe quasi-stable and metastable are interchangeable in this context.

I don't think you have shown the likelihood of this in any way. It is just your hypothesis. There are a lot of simplistic parameterizations in your set up, so any of those is a potential candidate.

We will clarify in text that our ocean temperature based calving parameterization simplicity is the most likely culprit of runaway retreat.

This is not at all what was shown in this paper, as far as I can tell.

We will further clarify upon this conclusion by first reaffirming that our simplistic parameterization of ocean temperature-based calving is a first order approximation of mélanges potential effects on calving. We will also recontextualize the importance of frontal dynamics specifically on the evolution of SK.

This was not shown anywhere in the paper

We will include a supplemental figure of the stress state to back this claim.

This is shown here? Or that is hypothesis about what happened in reality?

We will note in text that this is our hypothesis about what happened in reality.