Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

6th Oct 20

Dear Dr Choi,

First of all, please allow me to apologise for the delay in sending a decision on your manuscript titled "Ice dynamics will double the mass loss of the Greenland ice sheet over the next century". It has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you can make your methods fully transparent and discuss limitations and uncertainties, as requested by the reviewers. If so, the paper will be published under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper to address the concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within four weeks; please let us know if you need more time.

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper uses a state-of-the-art ice flow model to produce projections of the Greenland Ice Sheet, with a focus on capturing high resolution dynamics of marine terminating glaciers. The results demonstrate that ice dynamics will contribute 50+/-20% of the total mass loss by 2100, which is considerably higher than previous modelling efforts which have captured these dynamics in a more simplistic way. This is an important result because it highlights the need to better understand the changing dynamics of marine terminating glaciers, and that the processes affecting these glaciers need to be accounted for in ice sheet model projections on decades to century time scales. The study represents what must have been a substantial amount of effort to tune the calving law for each individual glacier basin (previously this has only been done for individual glaciers or small regions), to achieve a good match with observations for the calibration period (2007-2018). Therefore, I think it is a shame that the methods section is a little scant on details on how this calibration was achieved (see below). Otherwise the paper is well written and the figures are nicely presented. I have only a few minor comments:

L77-78: Do you have a sense of why the model struggles to estimate retreat in these cases? Is it a data quality issue or is the calving law missing some key processes in these glaciers in particular?

L113-114: "… the timing of the retreat is similar due to its control by bed topography" – the way this is worded makes it sound like this is not just a coincidence. Presumably it just chance that the particular geometries of these two glacier beds leads to the similar timing?

L141: "Similarly to SW, the overall SMB is positive until 2060 and then decreases until year 2080". This description does not match what I can see in Fig 3b for CW. SMB is positive throughout, and it decreases from ~2070 to 2100. It doesn't look similar to SW to me.

Fig 4b and c – what are the shaded areas? SD, range?

L245-252: Could you say a bit more about the stress threshold? Is it model/mesh/initialisation dependent? And if not, it might be nice to report the sigma_max values in the Dataset S1 tables. Can you say more about the implications of your assumption that it is constant throughout the simulations?

L253-255: The description of the calibration is a bit thin on details. Do you only take into account modelled versus observed retreat distance (i.e. information in Dataset S1)? How does the comparison with observed velocity and thinning rates, mentioned in this paragraph, factor into it? What is your threshold for a good match? For the glaciers that are marked as red or blue in the tables, is this the best possible match you could achieve after tuning sigma_max?

L271 and 274: some missing citation links

L285-288: A lot is covered in this sentence, but detail is missing. What is the purpose of the 1992-2017 period, when the calibration period starts in 2007? How does the transfer function work (ref 46 is in submission, so I can't even check)? How are the CTD and ocean model data combined? Ref 47 covers the period up to 2009, so was the ocean model simulation extended for this study and were more recent CTD measurements included?

Reviewer #2 (Remarks to the Author):

REVIEW

General comments

This manuscript estimates the contribution of the Greenland Ice Sheet to sea level rise the next century using ISSM. It builds on previous work by the same group, but using RCP forcing, more complex model physics and considering the entire ice sheet. The main findings are that the dynamic component of the mass loss plays a more important role than previously shown, and this is explained by the inclusion of ice dynamics in the model together with higher temporal and spatial model resolution than previous studies. Important for the success of the paper is also the thorough calibration and the use of a new bed topography.

The manuscript is well structured, starting with results from individual tidewater glaciers, moving on to regional results and finally ice sheet wide results. There are some grammatical flaws throughout the paper, specified below in the line by line comments. In general I'm missing some discussion on model uncertainty and limitations, and the implications of this, see specific comments below.

The work is scientifically and methodologically sound, and the findings are important and relevant for the field and the IPCC. I find the manuscript worthy of publication, after it has been revised to address the minor concerns detailed below.

Specific comments

You conclude that including ice dynamics in the model is important, and this is the reason why your dynamic component of mass loss is higher than previously found. I therefore find it important to discuss the parameter choices and uncertainty of these in the parameterizations (calving, ice shelf melt, and undercutting), as a change in these may have a critical impact on the results. I assume you have performed sensitivity experiments on a range of parameters and I miss a discussion on this. This could be presented in a figure similar to Figure 4b/c

I also miss some discussion on the limitations of the model. There is no transient thermal field in the simulations, how does this influence your results? Basal friction coefficient, viscosity and effective pressure are kept constant in time, is this realistic and what are the consequences of this? Surface runoff can influence ice dynamics through hydrofracture, shear softening, damage or through the subglacial hydrology system, these feedbacks are not included in your model setup, what are the consequences of this?

You state that it is crucial to use the new bed topography to capture the outlets in the calibration phase, and by using the old version it is simply not feasible. This is an important finding, and thus you mention this in the conclusions. However, it is not shown here, and not written what the old version is. Could a comparison using a former bathymetry/topography map be shown in supplementary, similar to Figure 1? In addition, you show that bed topography is an important control on the retreat of the tidewater glaciers. The new topography also includes some errors, how does this influence your main conclusions? This is briefly discussed in L133, but please elaborate here.

#You have chosen to mainly present results from the simulation with MIROC5 forcing. I miss a proper introduction of the various simulations you have run, explaining with what forcing etc, and then a statement on why you have chosen to present results from MIROC5. Other simulations could be presented in similar figures as Fig2, Fig 3, and Fig4b,c. In Dataset S2, is this MIROC5 forcing?

Line by line comments:

L6: " is about equally partitioned" consider rephrasing "about"
L16: " contribute most mass loss due to ice" consider rephrasing "most"
L18: ", or twice more than previously estimated." consider rephrasing "twice more"
L31-32: " is likely to continue playing a major role in glacier mass loss" consider rephrasing
L44-45:" because the glacier response to climate change is incompletely investigated." please explain why this leads to conservtaive and not overestimates?
L65: "within a unified formulation." what do you mean ?
L67: "Coupled Model Intercomparison Project Phase 5 (CMIP5)" missing "the" ?
L68: " by Ice Sheet Model Intercomparison Project for CMIP6 (ISMIP6)" missing "the" ?
L71: include reference
L74: "which develop an extensive floating ice shelf. " do they develop this during simulation? or do they have this present today?
L76: " the observations of most glaciers " what observations?
L78: "underestimate the retreat of 25 glaciers by up to 24 km" state somewhere that this is in the 2007-17 time period you're referring to.
L79: " was not captured for 67 glaciers because of " should this be present tense like the previous sentences?
L73-86: state more clearly that this is a section on the calibration results.
L83-83: "too little" "too much" , consider using the same terminology as in the figure caption of Figure 1 (overestimated retreat (red), underestimated retreat (blue), and poorly-known bathymetry (brown))..
L84: reference, what dataset is this "former bathymetry/topography map"
L85: " it is almost impossible to match a single glacier" poor formulation, please be more quantitative. This is a very interesting result, so maybe include a figure to show this in the supplementary. particularly since you mention this is in the conclusions.
L86: Here you can include "not shown here", or you can elaborate on this, as stated above.
L87: please make it more clear that you run many simulations with different forcing (present the various simulations here), and then state that you chose to focus and show results from MIROC
L88: state from what year they start. 2007?
L94: "Under MIROC5 RCP8.5 scenario". explain the MIROC5 abbreviation as this is the first time it is introduced. It can be clearer in general throughout the paper and supplementary (S2) that you mainly show results from MIROC5, but that you also performed other simulations with different forcings. If I understand correctly, this is the case in Fig 2 and all the figures in S2?
L102: "which possibly is a singularity" the word possibly feels weird
L130: hard for the reader to judge this excellent agreement from figure 3, as it is very small. Please try to be more quantitative, or try to show this agreement more clearly.
L134: you explain why SE displays a mismatch , but to me the NE sector also looks a bit off?

L150: “0 balance each other out.” consider rephrasing
L157: “Second, ” what is the first?
L159: I miss a comment on the ratio of land terminating vs marine terminating glaciers in each region and how this influences your results.
L168: “ in ice discharge combined with a decrease in SMB” refer to figure here
L227: skip “entire”, not necessary
L228: “cost for entire simulations. ” consider rephrasing
L235: how is N defined? I suppose that N is equal to the ice pressure above hydrostatic equilibrium? if so, state this. and kept constant in time?
L238: “coefficient remain to be constant during all simulations” consider rephrasing
L248: “between 2007 and 2018, ” do you mean 2017 as in L57?
L252: “The calibrated stress thresholds assume to be constant during all simulations” consider rephrasing
L255: This reference is different from the references using numbers, not sure this is journal requirements or what? same in L271,274 and 277
L273: calling it TF makes it look like two variables not one (T and F). consider something like T_F
L278 Eq6: here TF is bold, not consistent with Eq5
L305: this reference looks weird
L320: Complete this reference

Figures

FIG 1

This is a beautiful map. It would help Figure 2 if you included boxes (or dots?) to show the location of the outlets in this overview map. Consider changing the colors of the dots too, as these are not colorblind friendly. Very hard to distinguish the green, blue and brown dots, even for not colorblind people.

FIG 2

Please consider using a white box behind the black text in the maps, I find it hard to read. It could help with larger font size too. Is it possible to zoom in on the front position lines, the maps are so small? Maybe include a similar black line (observed mass balance) for consistency with Figure 3? If this exists of course. Please state clearly that these are results from the MIROC5 forcing? if this is correct..? These improvements would help readability of the figures in the supplement S2 too.

FIG 3

There should be consistency between Figure 2 and 3, with both color (as you have done) and line style (dashed or solid).

FIG 4

Figure 4a shows three different types of forcing (CMIP5 RCP8.5, CMIP5 RCP4.5 and CMIP6 ssp585), but only two of them are clearly distinguished (8.5 = solid line and 4.5= dashed line). It would be clearer if you somehow made a distinction between all three and showed this by for example having the legend split into three columns with headings. It took me a while to figure out all the different lines. Explain this clearly also in the caption. The difference between Fig4b and should also be explained in the panel itself, in addition to the caption.

Reviewer #3 (Remarks to the Author):

This manuscript presents modelling projections of the future mass change of the Greenland ice sheet using a fully dynamic ice sheet model. Given the current observed warming and related rapid dynamic changes at ocean terminating outlet glaciers the results are highly relevant and also of interest for the wider climate modelling and sea-level change community.
The novelty of this study and advance beyond previous modelling attempt clearly lies within the much improved treatment of the ocean terminating termini with regard to process representation (inclusion of calving process and freely moving calving fronts, longitudinal stress transfer, ocean and climate forcing) and spatial resolution of these outlet glaciers.
This advance in process representation and spatial resolution allows in my view for a first time to realistically include dynamic changes in ice sheet wide mass loss projections and to quantify and evaluate the role of ice dynamics in relation to surface mass balance. In comparison to earlier projections with strongly limited models with regard to outlet glacier dynamics, it demonstrates a sustained and substantially higher contribution from dynamic changes by the year 2100. In general, the main results and conclusions seem well supported by the undertaken modelling and uncertainties are discussed and investigated satisfactorily. The paper is in general well structured and written.
Besides some minor issues, I have a couple of more general issues/comments that should be considered before publication, but they do not really question the validity or relevance of the results. These issues mainly concern the explanation of the methodology, and to some degree the visualization and interpretation of the results.
Thus, overall, this is a highly relevant manuscript that provides a clear step forward in understanding and predicting the future behaviour of the Greenland ice sheet and hence I recommend publication after some rather minor revisions.

Calving approach and calibration:
One can of course question whether the used model for calving is the best approach, as all existing approaches have their advantages and deficiencies. Given the careful testing and comparison of their chosen approach to other calving models in their earlier papers, I would think it is a valid approach that importantly allows a fully dynamic treatment calving fronts and that has been compared comprehensively to observations (front positions and trends, thinning, velocities etc…) and allows further to include oceanic and atmospheric forcing. So, it is as got as it gets so far.
Perhaps the calibration process and how exactly the agreement between modelling and observations is evaluated/quantified could be explained a bit more in detail and better illustrated. For example a correlation plot in the supplements of observed against modelled front position maybe be useful to better demonstrate the fit (the table is ok but hard to evaluate as a whole). Further, it would be useful to explain in more detail how ocean forcing and calving are actually combined/integrated (I assume they are added and substracted from the front velocity to obtain front change, as explained in Choi et al. TC, 2018). It would be useful to add this more explicitly in the methods part.

A last point concerning the calibration: currently, the calibration has been undertaken during a phase a rapid dynamic mass loss (2007-2017) and the modelling actually successfully reproduces the dynamic retreat and mass loss trend. I wonder however, how well a 'stable' situation would be reproduced? Would the model also produce stable termini for a situation/climate as observed in the 1990s? has this been tested?

Visualization and presentation of results:
Although the figures of the cumulative mass changes (and different contributions) are as such ok, and correctly described and interpreted, I personally find it at times rather tricky to follow and would have preferred if directly SMB and discharge would have been shown as well (for some of the plots , e.g. Fig 2 or 3). Currently the changes in these quantities have to be deduced from the changes in slope of the cumulative line and for example for the discharge it is rather hard to see how it changes over time. Perhaps, some additional trend-lines that refer to the 'stable' pre-dynamic change period of the 1980s and early 1990s would help in fig. 2 and/or 3, so one could see the deviation of the 'balanced' cased better. This remark on presentation of the results is not essential but if addressed may may ease interpretation.

Atmospheric and ocean forcing products:
Some very brief explanation in the methods what the main modelling products for future atmospheric forcings mean, would be useful (e.g. main difference CMIP5 and CMIP6 (CMIP6 slightly warmer…), potentially also sketch out what RCP8.5 and 4.5 means in terms of average warming in temp, etc…), no details but basics, so a non-expert in climate modelling can follow (the many abbreviations are not known to everybody).
Further, in the explanation of the ocean forcing (methods p 18/19) it is just linked to GCM outputs, I assume, for the ocean forcing, the same GCM modelling outputs mentioned in the atmospheric forcing section (p. 18) in the methods. Could be clarified.

Role of SMB towards end of century
In my view, the two important findings from this paper that concern the overall ice sheet mass loss for the future seem to be:
1) discharge seems to be sustained at a high but constant level throughout the century (almost constant slope in fig 4) whereas
2) mass loss from negative SMB seems to progressively increase towards the end of the century and hence starting to dominate the mass loss signal.
The second point could perhaps be made more explicit and be included in the conclusions and at the end of the abstract.

Specific more minor comments:

The recent paper by King et al (2020) in Communications Earth & Environment may be useful to consider in this manuscript, as the importance of sustained high discharge is well demonstrated over the past two decades.

Line 25-26: this sentence gives the impression that enhanced calving is solely due to reduced ice mélange which I think is not what the authors mean. Later down they link calving correctly also to other processes (ocean forcing etc…). so maybe just rephrase things a bit more accurately.

Line 47: 'entirely' is in my view the wrong word here, I would rather say 'will be dominated by …'.
Lines 49-53: It maybe worth referring here also to the 'flowband' models that actually resolve the outlet glaciers well (process-wise and spatially) along the flowlines/troughs but that are limited to a few outlets and poorly resolve the full 3D geometry (eg. Ref 12 Nick et al 2013)

Line 75: '…85% of the observed DYNAMIC mass loss…'?

Line 84: 'earlier' instead of 'former'

Line 88…: it maybe useful to also show the actual discharge and SMB itself rather than just the cumulative values. Extracting changes from small slope changes in the cumulative lines is just a bit challenging sometimes.

Line 104/105: do you mean '…and the thermal oceanic forcing rather acts as a trigger'.

Line 114: I really struggle to actually see this synchronous retreat timing in fig 2 (light blue colored lines on blueish background is tricky to see!). Further, I can not quite follow the argument here. If they have different bed topography, it is not given that the retreat at the same time even if they experience the same forcing, as the dynamic threshold for retreat may be quite different.

Line 122/123: with regard to stabilization and other geometric effects (width), I think some additional reference to a 'tidewater outlet glacier' rather than an Antarctic ice stream would be useful: for example
Åkesson et al (2018) Impact of Fjord Geometry on Grounding Line Stability. Frontiers in Earth Science, 6, https://www.frontiersin.org/article/10.3389/feart.2018.00071
DOI=10.3389/feart.2018.00071
Or potentially Åkesson et al (2018) Atmosphere-driven ice sheet mass loss paced by topography: Insights from modelling the south-western Scandinavian Ice Sheet, Quaternary Science Reviews, Volume 195, 2018,

Lines 177-187: here a simple and brief explanation of what some of these climate scenarios represent (or what the differences between them are) would be useful (e.g. CMIP6 ssp585 comaprec to CMIP5…).

Line 210/211: just a note with regard to earlier modelling estimates, the much simpler flowband modelling of 4 large Greenland outlets and some upscaling by Nick et al 2013 delivered actually very similar Greeland-wide estimates of future mass loss (65-183mm by 2100) as this study (79-167mm by 2100). But I agree the model was limited (only 4 glaciers properly modellled), but at least it included some outlet glacier dynamics. In that sense Nick et al 2013 and this study are consistent.

Methods:

Line 240: '…to validate …we calibrate…'. Strictly speaking, if you calibrate you can not validating the model with the same data again… I am not saying you do this but I guess this should be rephrased.

Line 251: '… thresholds are assumed to be constant…'

Eqn (6) around lines 278-280: How is this ocean melt now combined/linked with the calving rate? I assumed just added the oceanic melt rate is just added to the calving rate as in Choi et al (2018). Maybe add the front evolution equation somewhere v(front)=v(flow)-(C+M)

Figures:
Fig. 1: maybe adjust the colour-scale of the dots, the blue and green are not easy to distinguish. Further, add the locations of the 5 outlet glaciers of Figure 2.

Fig. 2: for the modelled retreat rate the colour scale for the front position should be changed, light blue colored lines on blueish background is tricky to see!

Supplement
Figure in supplement to dataset S2: Details….The legends say Mass balance, SMB, Discharge which is not correct, this is CUMULATIVE SMB, Discharge etc,….

Maybe in the supplements, a graph showing the forcing curves (mean global air temp.) for the different climate projection scenarios would be useful.
Supplement

A map showing all locations of the modelled outlet glaciers maybe be useful and could be added in the supplements, so one can locate the individual outlet glacier results in the supplement.

Ice dynamics will remain a primary driver of Greenland ice sheet mass loss over the next century
– Response to reviewers –

Youngmin CHOI et al.

December 11, 2020

We thank the three reviewers for their positive and constructive comments that significantly improved the manuscript. We address their remarks below point by point.

1 Reviewer #1

This paper uses a state-of-the-art ice flow model to produce projections of the Greenland Ice Sheet, with a focus on capturing high resolution dynamics of marine terminating glaciers. The results demonstrate that ice dynamics will contribute 50+/-20% of the total mass loss by 2100, which is considerably higher than previous modelling efforts which have captured these dynamics in a more simplistic way. This is an important result because it highlights the need to better understand the changing dynamics of marine terminating glaciers, and that the processes affecting these glaciers need to be accounted for in ice sheet model projections on decades to century time scales. The study represents what must have been a substantial amount of effort to tune the calving law for each individual glacier basin (previously this has only been done for individual glaciers or small regions), to achieve a good match with observations for the calibration period (2007-2018). Therefore, I think it is a shame that the methods section is a little scant on details on how this calibration was achieved (see below). Otherwise the paper is well written and the figures are nicely presented. I have only a few minor comments:

Thank you for the positive comments.

L77-78: Do you have a sense of why the model struggles to estimate retreat in these cases? Is it a data quality issue or is the calving law missing some key processes in these glaciers in particular?

The calving law we use here may not capture all the different processes involved in calving. We added this limitation in Methods/Model calibration section: “The calving law used in this study may not capture all modes of calving, which likely explains the poor fit for glaciers highlighted in red.”

L113-114: “… the timing of the retreat is similar due to its control by bed topography” – the way this is worded makes it sound like this is not just a coincidence. Presumably it just chance that the particular geometries of these two glacier beds leads to the similar timing?

Yes. We clarified the sentence as the follows: “While the extent of the retreat for these two glaciers differs significantly, both retreats are controlled by bed topography.”

L141: “Similarly to SW, the overall SMB is positive until 2060 and then decreases until year 2080”. This description does not match what I can see in Fig 3b for CW. SMB is positive throughout, and it decreases from 2070 to 2100. It doesn’t look similar to SW to me.

Agreed. We deleted “Similarly to SW”.

Fig 4b and c – what are the shaded areas? SD, range?

The background shading is the ensemble spread for each component. We added it to the caption of the figure.

L245-252: Could you say a bit more about the stress threshold? Is it model/mesh/initialisation dependent? And if not, it might be nice to report the sigma$_{max}$ values in the Dataset S1 tables. Can you say more about the implications of your assumption that it is constant throughout the simulations?

This is a good point. In principle, it should not be model/mesh/initialization dependent, but the model is not a perfect representation of reality, the datasets we use to force the glacier and the bed topography may have errors that affect the calibration. The calibrated stress thresholds are assumed to be constant during all simulations because we assume that the ice tensile strength, which may also depend on other properties of ice (e.g., the existence of crevasses, presence of englacial water, ice temperature), would not change over the time scales considered here, and could not be well constrained by observations. We addressed this in the Method/Model calibration section as follows: “The calibrated stress thresholds are assumed to be constant through time during all simulations because we assume that the ice tensile strength, which may also depend on other properties of ice (e.g., the existence of crevasses, presence of englacial water, ice temperature), would not change over the time scales considered here, and could not be well constrained by observations. We run the simulations with one $\sigma_{\max}$ value for each glacier that gives the best possible matched distance."

L253-255: The description of the calibration is a bit thin on details. Do you only take into account modelled versus observed retreat distance (i.e. information in Dataset S1)? How does the comparison with observed velocity and thinning rates, mentioned in this paragraph, factor into it? What is your threshold for a good match? For the glaciers that are marked as red or blue in the tables, is this the best possible match you could achieve after tuning sigma$_{max}$?

Agreed. We added the details in Method/Model calibration section.

L271 and 274: some missing citation links

Done.

L285-288: A lot is covered in this sentence, but detail is missing. What is the purpose of the 1992-2017 period, when the calibration period starts in 2007? How does the transfer function work (ref 46 is in submission, so I can't even check)? How are the CTD and ocean model data combined? Ref 47 covers the period up to 2009, so was the ocean model simulation extended for this study and were more recent CTD measurements included?

Agreed. The period for ocean thermal forcing data is 1992-2017 but we use the data from 2007 in this study. The ocean model and CTD measurements are extended to 2017 to combine the CTD and ocean model data. We added the citation of paper that describes the details for ocean thermal forcings, that has been recently accepted (Draft available by Wood et al. upon request (mike.wood@jpl.nasa.gov)).

2 Reviewer #2

General comments

This manuscript estimates the contribution of the Greenland Ice Sheet to sea level rise the next century using ISSM. It builds on previous work by the same group, but using RCP forcing, more complex model physics and considering the entire ice sheet. The main findings are that the dynamic component of the mass loss plays a more important role than previously shown, and this is explained by the inclusion of ice dynamics in the model together with higher temporal and spatial model resolution than previous studies. Important for the success of the paper is also the thorough calibration and the use of a new bed topography.

The manuscript is well structured, starting with results from individual tidewater glaciers, moving on to regional results and finally ice sheet wide results. There are some grammatical flaws throughout the paper, specified below in the line by line comments. In general I'm missing some discussion on model uncertainty and limitations, and the implications of this, see specific comments below.

The work is scientifically and methodologically sound, and the findings are important and relevant for the field and the IPCC. I find the manuscript worthy of publication, after it has been revised to address the minor concerns detailed below.

Thank you. We address the reviewer's comments below.

You conclude that including ice dynamics in the model is important, and this is the reason why your dynamic component of mass loss is higher than previously found. I therefore find it important to discuss the parameter choices and uncertainty of these in the parameterizations (calving, ice shelf melt, and undercutting), as a change in these may have a critical impact on the results. I assume you have performed sensitivity experiments on a range of parameters and I miss a discussion on this. This could be presented in a figure similar to Figure 4b/c

This is an excellent point. The only parameter we calibrate in our model is stress threshold (σ_{max}) and we did not perform sensitivity experiments on a range of this parameter because changing it would increase the misfit to the data. However, we agree with the reviewer that this is an important point that should be mentioned clearly. We added this limitation to the Methods (Model calibration) section.

I also miss some discussion on the limitations of the model. There is no transient thermal field in the simulations, how does this influence your results? Basal friction coefficient, viscosity and effective pressure are kept constant in time, is this realistic and what are the consequences of this? Surface runoff can influence ice dynamics through hydrofracture, shear softening, damage or through the subglacial hydrology system, these feedbacks are not included in your model setup, what are the consequences of this?

These are excellent comments. We assumed that over the time scale considered here, changes in surface temperature would not affect the ice viscosity because diffusion is particularly slow. We did not consider hydrofrature or damage. These are additional levels of complexity that we did not explore because existing parameterizations remain poorly constrained. We added this limitation to the Methods section.

You state that it is crucial to use the new bed topography to capture the outlets in the calibration phase, and by using the old version it is simply not feasible. This is an important finding, and thus you mention this in the conclusions. However, it is not shown here, and not written what *the old version is. Could a comparison using a former bathymetry/topography map be shown in supplementary, similar to Figure 1? In addition, you show that bed topography is an important control on the retreat of the tidewater glaciers. The new topography also includes some errors, how does this influence your main conclusions? This is briefly discussed in L133, but please elaborate here.*

We reference *Morlighem et al.* [2017] which compares between new and old version (B2013 [*Bamber et al.*, 2013] and RTopo-2 [*Schaffer et al.*, 2016]) of bed topography map. The difference between two products is so large that some glaciers were land terminating glaciers in the old data instead of marine terminating. In addition, many fjords in BedMachine version 3 are significantly deeper than in previous maps suggesting between 30 and 100% more glaciers are potentially exposed warm Atlantic Water than suggested by B2013 or RTopo-2 [*Morlighem et al.*, 2017]. We added this comparison to the Results and Discussion section.

We also elaborated the influence of error in bed topography for SE in the Results and Discussion (Regional Changes) section and the Conclusions section.

You have chosen to mainly present results from the simulation with MIROC5 forcing. I miss a proper introduction of the various simulations you have run, explaining with what forcing etc, and then a statement on why you have chosen to present results from MIROC5. Other simulations could be presented in similar figures as Fig2, Fig 3, and Fig4b,c. In Dataset S2, is this MIROC5 forcing?

Thank you for the suggestion. We added the explanations for other GCM forcings (Results and Discussion) and figures for results from other GCM forcings in the Supplementary Information. We clarified the caption of Dataset S2 (now moved to Supplementary Data 1).

Line by line comments:

L6: " is about equally partitioned" consider rephrasing "about".

Done. We rephrased "about" to "nearly"

L16: " contribute most mass loss due to ice" consider rephrasing "most".

Done. "contribute more mass loss than other regions"

", or twice more than previously estimated." consider rephrasing "twice more"

Done. "twice as much as previously estimated"

“ is likely to continue playing a major role in glacier mass loss” consider rephrasing

Done. “will continue playing a major role ”

L44-45:” because the glacier response to climate change is incompletely investigated.” please explain why this leads to conservative and not overestimates?

Good point. We meant that “Models operating at a coarse resolution without calibrating the retreat of individual glaciers might produce overly conservative estimates of mass loss” because the glacier response to changes in ice-ocean interactions is not included in the model. We rephrased it.

L65: “within a unified formulation.” what do you mean ?

We removed the expression to clarify.

L67: “Coupled Model Intercomparison Project Phase 5 (CMIP5)” missing “the” ?

Done.

L68: “ by Ice Sheet Model Intercomparison Project for CMIP6 (ISMIP6)” missing “the” ? done

Done.

L71: include reference

Done.

L74: “which develop an extensive floating ice shelf. ” do they develop this during simulation? or do they have this present today? Rephrase to clarify.

They have it present today. We rephrased it as follows: “which have an extensive floating ice shelf.”

L76: “ the observations of most glaciers “ what observations?.

“Observed retreat distance”. We clarified it.

L78: “underestimate the retreat of 25 glaciers by up to 24 km” state somewhere that this is in the 2007-17 time period you’re referring to.

Done.

' L79: " was not captured for 67 glaciers because of " should this be present tense like the previous sentences?

Agreed. Done.

L73-86: state more clearly that this is a section on the calibration results.

Thank you for the suggestion. Done.

L83-83: "too little" "too much" , consider using the same terminology as in the figure caption of Figure 1 (overestimated retreat (red), underestimated retreat (blue), and poorly-known bathymetry (brown))..

Done.

L84: reference, what dataset is this "former bathymetry/topography map"

We reference *Morlighem et al.* [2017] which compares between new and old version (B2013 [*Bamber et al.*, 2013] and RTopo-2 [*Schaffer et al.*, 2016]) of bed topography map.

L85: " it is almost impossible to match a single glacier" poor formulation, please be more quantitative. This is a very interesting result, so maybe include a figure to show this in the supplementary. particularly since you mention this is in the conclusions. L86: Here you can include "not shown here", or you can elaborate on this, as stated above.

We elaborated on this point by adding a comparison between new and old version of bed topography map.

L87: please make it more clear that you run many simulations with different forcing (present the various simulations here), and then state that you chose to focus and show results from MIROC.

Thank you for the suggestion. We added it.

L88: state from what year they start. 2007?

Yes, it's 2007. We added it.

L94: "Under MIROC5 RCP8.5 scenario". explain the MIROC5 abbreviation as this is the first time it is introduced. It can be clearer in general throughout the paper and supplementary (S2) that you mainly show results from MIROC5, but that you also performed other simulations with different forcings. If I understand correctly, this is the case in Fig 2 and all the figures in S2?

The reviewer is right. We clarified this.

L102: "which possibly is a singularity" the word possibly feels weird

We edited the sentence as follows: "which has not been seen for the past 20 years *Khazendar et al.* [2019]."

L130: hard for the reader to judge this excellent agreement from figure 3, as it is very small. Please try to be more quantitative, or try to show this agreement more clearly.

Thank you for the suggestion. We added numbers for comparison between observed and modeled mass loss trends.

L134: you explain why SE displays a mismatch , but to me the NE sector also looks a bit off?

Agreed. We added NE for a mismatch and numbers for comparison.

L150: "0 balance each other out." consider rephrasing.

Done (deleted it).

L157: "Second, " what is the first?

We changed "Second" to "Additionally", Done.

L159: I miss a comment on the ratio of land terminating vs marine terminating glaciers in each region and how this influences your results.

We added it for SW versus NW and CW regions.

L168: " in ice discharge combined with a decrease in SMB" refer to figure here

Done.

L227: skip "entire", not necessary

Done.

L228: "cost for entire simulations. " consider rephrasing

We rephrased it as follows: "We divide the Greenland ice sheet into six domains (Fig. 1) to reduce computational cost compared to running simulations with one large domain."

L235: how is N defined? I suppose that N is equal to the ice pressure above hydrostatic equilibrium? if so, state this. and kept constant in time?i

The effective pressure (N) is defined as the difference between ice overburden pressure and basal water pressure and changes in time. We added this.

L238: "coefficient remain to be constant during all simulations" consider rephrasing

We rephrased as follows: "The inferred friction coefficient is kept constant in time during all simulations while the effective pressure changes in time".

L248: "between 2007 and 2018, " do you mean 2017 as in L57?

Yes. We edited it.

L252: "The calibrated stress thresholds assume to be constant during all simulations" consider rephrasing Done

We rephrased as follows: "The calibrated stress thresholds are assumed to be constant through time during all simulations."

L255: This reference is different from the references using numbers, not sure this is journal requirements or what? same in L271,274 and 277

Done.

L273: calling it TF makes it look like two variables not one (T and F). consider something like T_F . L278 Eq6: here TF is bold, not consistent with Eq5.

Thank you for the suggestion. We use $\tilde{T}$ instead of TF following *Morlighem et al.* [2019]

L305: this reference looks weird

Done.

L320: Complete this reference

Done.

Figures FIG 1 This is a beautiful map. It would help Figure 2 if you included boxes (or dots?) to show the location of the outlets in this overview map. Consider changing the colors of the dots too, as these are not colorblind friendly. Very hard to distinguish the green, blue and brown dots, even for not colorblind people.

Thank you for the suggestion. We added the locations of glaciers in Figure 2 and changed the colors of the dots.

FIG 2 Please consider using a white box behind the black text in the maps, I find it hard to read. It could help with larger font size too. Is it possible to zoom in on the front position lines, the maps are so small? Maybe include a similar black line (observed mass balance) for consistency with Figure 3? If this exists of course. Please state clearly that these are results from the MIROC5 forcing? if this is correct..? These improvements would help readability of the figures in the supplement S2 too.

We changed the Figure 2 and Supplement Figures for individual glaciers as suggested.

FIG 3 There should be consistency between Figure 2 and 3, with both color (as you have done) and line style (dashed or solid).

We changed the line style in Figure 2 to make it consistent with Fig 3.

FIG 4 Figure 4a shows three different types of forcing (CMIP5 RCP8.5, CMIP5 RCP4.5 and CMIP6 ssp585), but only two of them are clearly distinguished (8.5 = solid line and 4.5= dashed line). It would be clearer if you somehow made a distinction between all three and showed this by for example having the legend split into three columns with headings. It took me a while to figure out all the different lines. Explain this clearly also in the caption. The difference between Fig4b and should also be explained in the panel itself, in addition to the caption.

Done.

3 Reviewer #3

This manuscript presents modelling projections of the future mass change of the Greenland ice sheet using a fully dynamic ice sheet model. Given the current observed warming and related rapid dynamic changes at ocean terminating outlet glaciers the results are highly relevant and also of interest for the wider climate modelling and sea-level change community.
The novelty of this study and advance beyond previous modelling attempt clearly lies within the much improved treatment of the ocean terminating termini with regard to process representation (inclusion of calving process and freely moving calving fronts, longitudinal stress transfer, ocean and climate forcing) and spatial resolution of these outlet glaciers.
This advance in process representation and spatial resolution allows in my view for a first time to realistically include dynamic changes in ice sheet wide mass loss projections and to quantify and evaluate the role of ice dynamics in relation to surface mass balance. In comparison to earlier projections with strongly limited models with regard to outlet glacier dynamics, it demonstrates a sustained and substantially higher contribution from dynamic changes by the year 2100. In general, the main results and conclusions seem well supported by the undertaken modelling and uncertainties are discussed and investigated satisfactorily. The paper is in general well structured and written.
Besides some minor issues, I have a couple of more general issues/comments that should be considered before publication, but they do not really question the validity or relevance of the results. These issues mainly concern the explanation of the methodology, and to some degree the visualization and interpretation of the results.
Thus, overall, this is a highly relevant manuscript that provides a clear step forward in understanding and predicting the future behaviour of the Greenland ice sheet and hence I recommend publication after some rather minor revisions.

Thank you for positive comments.

Calving approach and calibration: One can of course question whether the used model for calving is the best approach, as all existing approaches have their advantages and deficiencies. Given the careful testing and comparison of their chosen approach to other calving models in their earlier papers, I would think it is a valid approach that importantly allows a fully dynamic treatment calving fronts and that has been compared comprehensively to observations (front positions and trends, thinning, velocities etc. . .) and allows further to include oceanic and atmospheric forcing. So, it is as got as it gets so far.
Perhaps the calibration process and how exactly the agreement between modelling and observations is evaluated/quantified could be explained a bit more in detail and better illustrated. For example a correlation plot in the supplements of observed against modelled front position maybe be useful to better demonstrate the fit (the table is ok but hard to evaluate as a whole).

Thank you for the suggestion. We added the correlation plot that shows the agreement between observations and model for all modeled glaciers in the Supplementary Information.

Further, it would be useful to explain in more detail how ocean forcing and calving are actually combined/integrated (I assume they are added and substracted from the front velocity to obtain front change, as explained in Choi et al. TC, 2018). It would be useful to add this more explicitly in the methods part.

Great point. We added the following in the Methods/Model calibration section. " To track the dynamic motion of the ice front, we rely on the level set method *Bondzio et al.* [2016], where the velocity of the calving front is defined as follows:

$$\mathbf{v}_{\text{front}} = \mathbf{v} - \left(c + \dot{M}\right)\mathbf{n} \tag{1}$$

where $\mathbf{v}$ is the ice velocity vector, c is the calving rate, $\dot{M}$ is the undercutting rate at the calving face and $\mathbf{n}$ is a unit normal vector that points outward from the ice domain. c and $\dot{M}$ are assumed to be independent. We use the undercutting parameterization *Rignot et al.* [2016] to estimate $\dot{M}$ and specifically optimize c to calibrate modeled ice front. "

A last point concerning the calibration: currently, the calibration has been undertaken during a phase a rapid dynamic mass loss (2007-2017) and the modelling actually successfully reproduces the dynamic retreat and mass loss trend. I wonder however, how well a 'stable' situation would be reproduced? Would the model also produce stable termini for a situation/climate as observed in the 1990s? has this been tested?

This is an excellent point. We did not calibrate σ_{max} for a stable period of ice front because, as stated in *Morlighem et al.* [2019], the stable glaciers are generally stable due to the bed topography under their terminus and have a wide range of σ_{max} for which the model is also stable. We added this point in the Methods/Model calibration section as follows: "We did not calibrate σ_{max} over a stable period of ice front position because, as stated in a previous study *Morlighem et al.* [2019], stable glaciers are generally stable due to the bed topography under their terminus and have a wide range of σ_{max} for which the model is also stable *Choi et al.* [2018]."

Visualization and presentation of results: Although the figures of the cumulative mass changes (and different contributions) are as such ok, and correctly described and interpreted, I personally find it at times rather tricky to follow and would have preferred if directly SMB and discharge would have been shown as well (for some of the plots , e.g. Fig 2 or 3). Currently the changes in these quantities have to be deduced from the changes in slope of the cumulative line and for example for *the discharge it is rather hard to see how it changes over time. Perhaps, some additional trend-lines that refer to the 'stable' pre-dynamic change period of the 1980s and early 1990s would help in fig. 2 and/or 3, so one could see the deviation of the 'balanced' cased better. This remark on presentation of the results is not essential but if addressed may may ease interpretation.*

We appreciate the reviewer's comments and had long discussions about what to do. We think that what matters in the end is the cumulative signal, not the year to year rate, along with the main text, which is in general smoother hence easier to interpret. Additionally, we do not add more lines for 'stable' period because we here focus on the calibration period (2007-2017) and the future simulations. We decided not to make changes for those figures.

Atmospheric and ocean forcing products: Some very brief explanation in the methods what the main modelling products for future atmospheric forcings mean, would be useful (e.g. main difference CMIP5 and CMIP6 (CMIP6 slightly warmer. . .), potentially also sketch out what RCP8.5 and 4.5 means in terms of average warming in temp, etc. . .), no details but basics, so a non-expert in climate modelling can follow (the many abbreviations are not known to everybody).

Thank you for the suggestion. We added the brief explanation for RCPs and SSPs in the Methods/Atmospheric forcings section.

Further, in the explanation of the ocean forcing (methods p 18/19) it is just linked to GCM outputs, I assume, for the ocean forcing, the same GCM modelling outputs mentioned in the atmospheric forcing section (p. 18) in the methods. Could be clarified.

The reviewer is right. We clarified this in the Methods/Oceanic forcings section.

Role of SMB towards end of century In my view, the two important findings from this paper that concern the overall ice sheet mass loss for the future seem to be: 1) discharge seems to be sustained at a high but constant level throughout the century (almost constant slope in fig 4) whereas 2) mass loss from negative SMB seems to progressively increase towards the end of the century and hence starting to dominate the mass loss signal. The second point could perhaps be made more explicit and be included in the conclusions and at the end of the abstract.

We added the second point in the conclusions and in the end of the abstract.

Specific more minor comments:

The recent paper by King et al (2020) in Communications Earth & Environment may be useful to consider in this manuscript, as the importance of sustained high discharge is well demonstrated over the past two decades.

Agreed. We added the citation.

Line 25-26: this sentence gives the impression that enhanced calving is solely due to reduced ice mélange which I think is not what the authors mean. Later down they link calving correctly also to other processes (ocean forcing etc. . .). so maybe just rephrase things a bit more accurately.

Agreed. We deleted the following to make the sentence clear: "due to melting of the sea ice melange that holds large blocks of ice together in glacial fjords."

Line 47: 'entirely' is in my view the wrong word here, I would rather say 'will be dominated by . . . '.

Done.

Lines 49-53: It maybe worth referring here also to the 'flowband' models that actually resolve the outlet glaciers well (process-wise and spatially) along the flowlines/troughs but that are limited to a few outlets and poorly resolve the full 3D geometry (eg. Ref 12 Nick et al 2013)

Thank you for the suggestion. We added this.

Line 75: '. . . 85% of the observed DYNAMIC mass loss. . . '?

It should be observed entire mass loss from Greenland, not just dynamic mass loss. We clarified it.

Line 84: 'earlier' instead of 'former'

Done.

Line 88. . . : it maybe useful to also show the actual discharge and SMB itself rather than just the cumulative values. Extracting changes from small slope changes in the cumulative lines is just a bit challenging sometimes.

We decided not to make changes for figures as explained above. We think that what matters in the end is the cumulative signal, not the year to year rate, along with the main text, which is in general smoother hence easier to interpret.

Line 104/105: do you mean '. . . and the thermal oceanic forcing rather acts as a trigger'.

We rephrased this sentence as follows: "The main factor responsible for these contrasting behaviors between glaciers is the bed geometry but the ocean thermal forcing also could affect the retreat of glaciers as shown in the recent deceleration of Jakobshavn Isbræ"

Line 114: I really struggle to actually see this synchronous retreat timing in fig 2 (light blue colored lines on blueish background is tricky to see!). Further, I can not quite follow the argument here. If they have different bed topography, it is not given that the retreat at the same time even if they experience the same forcing, as the dynamic threshold for retreat may be quite different.

Agreed. We clarified the sentence as follows: "While the extent of the retreat for these two glaciers differs significantly, both retreats are controlled by bed topography."

Line 122/123: with regard to stabilization and other geometric effects (width), I think some additional reference to a 'tidewater outlet glacier' rather than an Antarctic ice stream would be useful: for example Åkesson et al (2018) Impact of Fjord Geometry on Grounding Line Stability. Frontiers in Earth Science, 6, https://www.frontiersin.org/article/10.3389/feart.2018.00071 DOI=10.3389/feart.2018.00071 Or potentially Åkesson et al (2018) Atmosphere-driven ice sheet mass loss paced by topography: Insights from modelling the south-western Scandinavian Ice Sheet, Quaternary Science Reviews, Volume 195, 2018,

We added these references.

Lines 177-187: here a simple and brief explanation of what some of these climate scenarios represent (or what the differences between them are) would be useful (e.g. CMIP6 ssp585 comaprec to CMIP5…).

We added this to the Marine-terminating glaciers and Sea level contribution from Greenland section.

Line 210/211: just a note with regard to earlier modelling estimates, the much simpler flowband modelling of 4 large Greenland outlets and some upscaling by Nick et al 2013 delivered actually very similar Greeland-wide estimates of future mass loss (65-183mm by 2100) as this study (79-167mm by 2100). But I agree the model was limited (only 4 glaciers properly modellled), but at least it included some outlet glacier dynamics. In that sense Nick et al 2013 and this study are consistent.

We compare our models to studies that model the entire Greenland region. We added this point.

Methods: Line 240: '… to validate … we calibrate… '. Strictly speaking, if you calibrate you can not validating the model with the same data again… I am not saying you do this but I guess this should be rephrased.

Good point. We rephrased this sentence as follows: "To calibrate our model, we try to match observed ice front retreat from 2007 to 2017 following previous studies *Choi et al.* [2018]; *Morlighem et al.* [2019]."

Line 251: '… thresholds are assumed to be constant…'

Done.

Eqn (6) around lines 278-280: How is this ocean melt now combined/linked with the calving rate? I assumed just added the oceanic melt rate is just added to the calving rate as in Choi et al (2018). Maybe add the front evolution equation somewhere v(front)=v(flow)-(C+M)

We added this to the Model calibration section.

Figures: Fig. 1: maybe adjust the colour-scale of the dots, the blue and green are not easy to distinguish. Further, add the locations of the 5 outlet glaciers of Figure 2.

Done.

Fig. 2: for the modelled retreat rate the colour scale for the front position should be changed, light blue colored lines on blueish background is tricky to see!

Thank you for the suggestion but we decided to keep the color scale for consistency with figures from previous papers.

Supplement Figure in supplement to dataset S2: Details….The legends say Mass balance, SMB, Discharge which is not correct, this is CUMULATIVE SMB, Discharge etc,….

We fixed it.

Maybe in the supplements, a graph showing the forcing curves (mean global air temp.) for the different climate projection scenarios would be useful.

We did not use mean global air temp directly in our model, so we decided to reference several studies for the different climate scenarios instead of adding a plot.

Supplement

A map showing all locations of the modelled outlet glaciers maybe be useful and could be added in the supplements, so one can locate the individual outlet glacier results in the supplement.

We added the location of individual glaciers in figures of Supplementary Data 1

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