

Peer Review File

Manuscript Title: Antarctic calving loss rivals ice shelf thinning.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

Many studies across the last ~20 years have focused on ice shelf thinning as a mechanism for ice sheet destabilization: thinning of ice shelves reduces buttressing forces on grounded ice, which in turn results in increased flux of grounded ice to the ocean. Fewer studies, however, have focused on the role of ice shelf extent change (calving) in moderating grounded ice flux. This study does just that by first showing Antarctic ice shelf area change across the last 24 years and then modeling the impact that further area change could have on ice flux. The central conclusion of the work is that the ice sheet is equally as sensitive to variations in ice shelf extent as it is to ice shelf thickness. These results are interesting because they serve as a sign-post for directing future research by showing that a previously ignored process is equally as important as a previously well-considered process. This is novel work with timely conclusions that is well worthy of publication in Nature with only minor revisions.

The work presented here is strongly original: no previous study to my knowledge has attempted to quantify Antarctic-wide coastline change. This study quantifies annual coastline change from 1997-2021 using a variety of remotely sensed imagery that has been very carefully corrected: this in an of itself is a valuable contribution, and will be a widely-used dataset once it is made publicly available. Beyond the dataset itself, this work is novel because it highlights a relatively ignored — yet impactful — process. The community interested in ice sheet stability has paid huge attention to the role of ice shelf thinning in destabilizing marine ice sheets. Very little focus has been paid to the impact of changing ice shelf area on ice sheet stability. This work quantifies the importance of ice shelf area loss to ice sheet discharge and, in doing so, will redirect future efforts of the ice sheet community. The quality of both the experimental approach and treatment of uncertainty is excellent and the writing style is quite clear. The figures included in the main text are visually appealing. The references are appropriate and complete.

The supplement demonstrates the degree of care that the authors took in working with the continent-wide data used in this study. For example, many studies simply take the MEaSUREs V2 or BedMachine data products and do not manipulate/clean them further: I am impressed with the care that the authors took to fill holes and unreasonable regions within all of the datasets used here. The authors are to be commended for their commitment to being comprehensive in mapping a physically reasonable coastline.

I have two minor concerns with the work at present, though both should be readily addressed with some simple clarifications, discussion of assumptions, and a supplementary figure.

My primary concern is the usage of fixed grounding line positions and thicknesses in the modeling efforts presented here. This is a significant assumption, and warrants additional discussion about the limits that it places on the final conclusions. It seems like this would result in significantly steeper grounding zone slopes than would otherwise be observed (e.g. if the grounding line is fixed in thickness and position but the shelf is dramatically thinned, the grounding line surface slope increases). These steep slopes promote rapid discharge, but if the grounding line were allowed to shift and thin, discharge could be lower. I think this effect would be the most pronounced in the case of thinning ice shelves and have less of an impact on calving of the shelf —

this then places some bias into the comparison of the two cases (though I would think it biases in the direction of making calving an even more important effect). I recognize that an evolving grounding line position adds a significant level of complexity to the modeling efforts - I'm not suggesting that should be done. What about adding a one-catchment sensitivity test using a moving grounding zone to test the hypothesis that ice shelf thinning and ice shelf calving have a similar order-of-magnitude impact on grounding zone-discharge feedbacks?

My secondary concern with the work as it stands focuses on the assumptions surrounding the projected timing of the next major calving events. The authors make the assumption that the ice shelf cannot grow beyond its maximum observed extent, but there is no physical mechanism discussed as part of this. The trajectory of the Ronne ice shelf I think best highlights my concern here: if the dataset only extended from 2005-present, we might assume that the ice shelf cannot grow beyond its 2020 calving location. The maximum possible extent of an ice shelf would seem to be the product of many environmental factors that are changing through time — can we assume that the maximum possible extent in 1964 is identical to the maximum possible extent today? This assumption warrants some discussion.

Minor line-by-line comments

L62 This sentence could use additional detail for how this prediction was made and the uncertainty surrounding it — either here or in the supplement.

L81 Is citation 39 not yet published?

L96 Would this response be different if the removal of buttressing was ramped across several weeks to months instead of being instantaneous? This is perhaps a large rabbit hole, but I would be interested to read a few sentences in the supplement defending the choice of instant removal.

L103 Does this mean the model is run to steady-state for the new configuration?

L125-127 Super interesting result — I wouldn't have predicted this.

L supplement 177 This is an excellent pun.

Figure 2: I find this figure confusing. It seems to suggest widespread synchronous advance and calving events? EG a widespread advance event in 2007, a widespread retreat event in 2009.. etc. I don't think these events occurred, so it makes me think that I must be interpreting this figure wrong. Clarify in some way.

Figure 3: This figure would benefit from some kind of differentiation between observations and inferences. For example, are the "zero" extents from pre-1995 inferred or based on observations of some kind? Or is the timing of calving known from some other kind of observation and the assumption is that it calved back to that location?

Extended Data Figure 3: A) are the contours meters of ice thickness? I'm not sure what I am supposed to be seeing in this figure, particularly in A). It looks like there is very little pattern to where there are positive and negative acceleration. Visual inspection of any trends would be easier if the dots were larger.

--Kiya Riverman

Referee #2 (Remarks to the Author):

I am reviewing the manuscript of Greene et al., 'Antarctic calving loss rivals ice shelf thinning', and was asked to assess the ISSM modelling part. The modelling part explores the isolated response of the grounding line flux to loss of buttressing by calving or ice shelf thinning over the observational period until 2021. The simulated grounding line flux responds linearly to the forcing whereby the glaciers in the Amundsen Sea Embayment show the strongest response. In addition, experiments are conducted where the buttressing (either calving or thinning) is incrementally reduced until a

complete collapse of the ice shelf occurs. These future scenarios suggest that the total ice shelf loss of Ronne, Filcher, and Amery would most significantly impact the grounding line flux.

Although I found the results very well described and presented, I have several concerns about the modelling part:

1. I am not convinced that the (ice shelf) thinning experiments produce reliable grounding line flux results. Just changing the ice thickness of the ice shelf seems odd as strong geometric gradients are likely introduced that distort the results. Gudmundsson et al. (2019) explained in detail how to ensure that the driving stress is unaffected by changing only the floating ice thickness; additionally, they performed some sensitivity experiments. Are you following this methodological approach? However, I think the ice surface over the grounded ice should also be changed (see my comment to Line 314/315).
2. In the whole manuscript, I missed a clear sentence that calving front retreat and ice shelf thinning are closely related to each other. They are not rival processes as the title suggests (BTW, the word 'rival' does not appear in the text). Both processes interact, i.e., calving front retreat can initiate velocity increase and therefore ice shelf thinning. The other way around, ice shelf thinning might trigger higher calving frequencies. It is unclear whether the authors think that the modelled grounding line flux anomalies from both experiment types should be added to get the full response (Line 19-21)? I guess no. So, it should be made clearer that the modelled grounding line flux anomalies from both isolated experiments should not be added to get the full grounding line flux anomaly (i.e., sea-level equivalent). Your model experiments could be extended by running a full-response scenario, i.e., combining the calving and thinning experiments.
3. A retreating calving front or ice shelf thinning reduces buttressing and increases the grounding line flux is not new.
 - a. The full collapse of the AIS ice shelves buttressing effect (upper-end member) on the grounding line flux was shown by the ABUMIP experiments (Sun et al., 2020). Your full-collapse calving experiment is comparable to ABUK and your thinning experiments to ABUM. So, ABUMIP explored these processes also in isolation but with several transient ice flow models (not instantaneous response approach). ABUMIP also showed that Filchner and Ronne are the largest sea-level driver under this scenario.
 - b. Gudmundsson et al. (2019) performed a similar study, but used a different observational period and conducted only the thinning experiments.
So, what are the new key aspects compared to ABUMIP and Gudmundsson et al. (2019)?

Minor comments

Abstract

Line 15, 17 and 18 For a journal like Nature, I would use percentage values instead of the net values.

Line 17 In order to be consistent with the title, I would replace "ice front retreat" with "calving"

Main

Line 26: Please slightly rewrite. Melt is expected to increase at the margins. But in the interior SMB will increase and in general the total SMB will increase (e.g., Fig 3b in Rodehacke et al., 2020)

Chapter "Antarctica's evolving coastline" not reviewed.

Line 94 Suggest to drop NASA. Is this important? Please add reference to ISSM.

Line 100 The number of 20 million elements tells me nothing. What is the horizontal resolution in critical areas (e.g., grounding line)? Are you setup a 3D mesh? Why is such a high-resolution needed?

Line 100 Please provide a date of initialization.

Line 101 What is the date of the observed dataset? It is rather close to the 90s or the 20th century? Please add a reference.

Line 101/102 suggest to drop "... followed by a short relaxation simulation". I think, this technical aspect is not important for the main text. It also does not fit to your statement above (Line 97/98) that time-evolving feedbacks are not considered.
What is the motivation for the relax run?

Line 102/103 You should say, that you only alter the ice thickness of floating ice.

Line 103 "... solve for stress balance". I found that a bit too technical. Maybe just: "... solve for ice velocities to estimate the change in grounding line flux due to the prescribed. perturbation".

Line 104 replace 'model' with e.g. simulation results.

Line 105 In the 'observational' chapter before you are referring observations starting at 1997. Why are your simulations start in 1992?

Line 106 Linear to what?

Line 107 Maybe I missed that, but why is the linear behaviour expected?

Line 111 add "... response of the grounding line flux ..."

Line 112 replace "coastal evolution" with "calving front evolution"

Line 112 replace "calving front geometry" with "calving front extent".

Line 121 What is present-day? Is present-day equal to initialization?

Line 197/198 Please provide the ISSM version (or revision number). Also, I think, a code snippet how you maintain the 10.000m/a velocity threshold (SI Line 338) is required; to my knowledge this isn't ISSM's standard.

Supplementary Material

The Supplementary Material consists of 37 pages without chapter numbering. The reference in the main text to "Supplementary Material" required a lot of searches for the reader to what material you are referring to.

Line 280 Why 1992? Your observational dataset starts in 1997.

Line 281 How are you dealing with initial ice thicknesses that are beyond the BedMachine ice mask extent?

Line 282 100m AND 75km in the interior? What do you mean with finer? Finer than 100m in the interior? I guess you have 75km in the interior and 100m in critical areas, right? By the way, have you considered to construct a mesh for each calving experiment where the annual calving fronts

determine the model domain? So, you could avoid the high resolution of 100m at the calving front. Also, the thinning experiments don't need the high resolution of 100m at the calving front. In my opinion you could save a lot of elements, i.e., computing time, by improving the mesh. Or just state, you employ the same mesh for all simulations to avoid numerical issues.

Line 288 Are the layers equally distributed?

Line 292 In my opinion, depth averaging the temperatures results into too stiff ice. Note, that most of the deformation takes place at the ice base where we have much warmer ice than over most of the ice column. The effort to reach a good thermal state by relying on the method by Schlegel et al (2018) is lost by depth averaging the temperatures. Not sure, whether this will influence the results or not ... Maybe a few tests with constant temperatures, i.e., rheologies would help. Moreover, you dismiss the thermal information by inverting for the rheology in the floating ice (Line 297). In my opinion, the strategy to get the thermal information is useless and could be replaced by a simpler approach. Generally, I like the approach to start with a good thermal state but here it is levelled out by the subsequent steps.

Line 296 I think you should state which kind of friction law you use (linear or non-linear) and motivate your choice. The sensitivity in modelled grounding line flux is likely dependent on this choice (see the ABUMIP experiments, Sun et al. (2020)).

Line 298 Concerning the reference to Fig S1, I was now expecting a figure about your 'match' instead of just showing me a figure of observed velocities.

Line 299 So, you allow the model to relax. Also, the grounding line? I think it's important to mention that this gives you a different grounding line than BedMachine (as mentioned in Line 345). I don't understand "... relax to all model forcing". And what does "... relax ... to the domain which extends beyond that of the present-day ice sheet" mean?

Line 313/314 drop "levelset". This is ISSM language. Just ice mask or ice-covered regions.

Line 314/315 I am very concerned by this approach. Just altering the geometry of the floating ice (keeping the floating-grounded ice mask based on the end of the relaxation run fixed) would probably cause steep gradients of the geometry (both ice base and ice surface) at the grounding line. These steep geometry gradients could cause a strong velocity in-/decrease, i.e., change grounding line flux. I am wondering, why the authors are not using observed ice thicknesses to construct a new ice sheet without a discontinuous gradient in geometry at the grounding line. Of course, grounding line flux anomalies are more difficult to calculate (since the shape and length of the grounding line changes), however, you might define drainage catchments etc. to calculate anomalies ... In the present state, I don't believe you are getting reliable grounding line fluxes in the "thinning" experiments.

Line 324 add "... of individual ice shelves"

Line 324 Would call the chapter "Model comparison" instead of "validation".

Line 328 Gudmundsson et al. (2019) is not listed in the SI references. However, the ref is provided in the main text. Not sure, whether it should be also appearing in the SI refs. It seems that your comparison to Gudmundsson et al. (2019) is quite ok, but can you please use the same colorbar to make comparison easier?

Line 324-328. Is your model now validated or not? Based on which criterion? Is Gudmundsson et al 2019 using the same model, data etc? Please help me to understand your validation approach.

Line 339 Why instability? Values of 10000m/a are observed at glaciers, e.g., Jakobshavn.

Line 340 Maybe it occurs where you generate steep geometry gradients by keeping the grounding

line fixed it the thinning experiments (see my comment to Line 314/315). Are the nodes located at the grounding line?

Line 343 Well, I agree, that the geometry change at grounded ice is slow. However, your statement "the ice has not been given time to draw down" is incorrect. According to the mass transport equation or the free surface equation, the geometry over grounded ice will change over one time step. Over one year I would expect some changes at grounded ice at least to maintain a consistent geometry at the grounding line (see major point 1). Also, in line 149 you say that ice shelf thinning occurs slowly. This is contradictory to the statement here.

Line 347/348 Not sure why you reference Gardener et al (2018). I mean, grounding line flux is clearly defined and available as an output-variable in ISSM. Is the calculation of the grounding line flux differently as the ISSM variable GroundinglineMassFlux? If so, why?

Line 352 I suggest to drop these "Notes". I found it very speculative and not well supported by the figures and the text. It also took me a while to figure out about which step/jump you are talking.

References

Line 363 Not sure about citing an AGU abstract ...

Line 375 Drop 'Larour ...'.

Line 386 'J. Glaciol' not consistent with Line 367 where the full Journal name is given (there are more of these inconsistencies)

Referee #3 (Remarks to the Author):

Overview

The study from Greene and others produces an impressive roughly annual record of coastline change across the whole of Antarctica between 1997 and 2021 using a novel assimilation of a variety of datasets. From this record they calculate as a whole Antarctic ice shelves have lost area since 1997. They also calculate the observed iceberg calving rivals basal melting in the overall mass balance of ice shelves. They go further quantifying the importance of ice shelf calving on Antarctic ice flux using numerical modelling.

I think this is a very interesting study. The significance of this study can be described in two accounts. Firstly it provides an updated pan-ice sheet coastline change estimate over the past 25 years and an estimate on the partitioning of calving vrs basal melt in ice shelf mass loss over the same time period. Secondly, and I think where the main novelty of the study lies, is the quantitative assessment in the importance of iceberg calving on the recent mass loss of the Antarctic ice sheet. The background here is that we don't really consider calving in most of the models that simulate the future evolution of the Antarctic ice sheet in response to global climate change, but this study shows that actually over the past 25 years it has been relatively important. So I think this study has the potential to meet the novelty criteria to be published in a journal of this stature. However, I do have a few concerns and comments that I would like to see addressed that I have detailed below.

Main Comments

Ice shelf area change: The problem here is that all data is reported in area change. This means that the large ice shelves (e.g. Ross/Filchner-Ronner/Mertz etc) where there is no evidence of any

long-term change dominate the signal here with their natural calving cycles. So the actual figure of area change, while I agree is important to report, does not tell the full story. I think the authors need to go further in clarifying this in the manuscript. I am wondering if the authors could categorize glaciers that are definitely retreating outside any likely natural calving cycles and provide a separate area change and mass loss figure for these glaciers throughout the manuscript and highlight their locations on an appropriate figure (maybe Figure 1). There is an element of subjectivity about this, but as far as I can see in the Supplementary plots it looks like it the regions that previous studies have highlighted are retreating (Antarctic Peninsula, Thwaites/Pine Island and maybe Totten), pretty much all others are undergoing something similar to natural calving cycle? At the moment the manuscript could be interpreted like there is widespread ice shelf retreat across Antarctica, I don't think this is the case, is it not concentrated in a few set regions?

I also think the assumption that glaciers will calve at their last known location is problematic for some of these very large ice shelves with multi-decadal calving cycles. Really we need much longer records of change to be able to quantify this. I think there needs to be a strong caveat that this may not be the case. For example the Brunt, Cook, and Denman ice shelves all have known variable calving locations. In the most extreme case the Cook Ice Shelf, I think it is now the equivalent of 15 years advanced of its last known calving location.

Modelling: I am fan of the quantitative work done to estimate the impact of observed rates of ice shelf thinning and iceberg calving on ice flux across Antarctica because they are based on observations and over a relatively short period of time. However, I am less keen on the future hypothetical experiments. This is because in reality ice shelf thinning and calving work in tandem. It is perhaps ok to separate these processes over short timescale, but ultimately changes in ice shelf thickness are somewhat of a prerequisite to any notable changes in ice shelf area. Essentially major losses of ice shelf area simply are not going to happen without prior changes in ice shelf thickness. So on these extreme experiments I don't really see the benefit in separating these processes. Its all theoretical and I am not sure on the real world benefit.

Furthermore, I know it's not the authors intention, but I also think it is important to state in there is no evidence of the likes of the Ronne and Ross Ice shelves collapsing at any point in the near future. Non-specialists reading some of these sections could be left with that impression.

Minor comments

Line 48: There is a big difference between the overall area change reported in this study ($36,701 \pm 1465$) compared to the study of Baumhoer et al (ref 27; $\sim -22,500 \text{ km}^2$), granted the studies cover slightly different time periods. Unless this can be reconciled by the slightly different time periods – this should be commented on.

Line 52-53: Is there any examples of some ice shelves advancing? E.g. Cook. Also I presume for some sections of the ice sheet (e.g. East Antarctica) most ice shelves are in the natural advance/retreat category?

Line 61-62: I don't think we really know this for certain. Our problem here is that the calving cycles of these ice shelves operate on multi-decadal timescales that are much longer than the observational records. Last known location is of course a good start, but in the very few locations where ice front histories have been extended using old aerial photographs/maps this assumption can be quite far off e.g. Brunt, Cook, Denman.

Line 74-75: The Ronne Ice Shelf, while technically a loss over the observational time period, likely would not be a loss if the observational time period was much longer. There is no evidence this ice shelf is changing considerably, this needs to be clarified in the manuscript.

Line 117: Not sure I would call the changes at the Thwaites tongue 'steady' in my view, the complete loss of a 100 km floating tongue in less than 20 years is anything but.

Line 126-127: strong caveat is needed here that both of these processes work in tandem in reality. Substantial ice shelf loss through calving is unlikely to happen without significant thinning.

Line 133-137: Need to clarify that for some of these glaciers mentioned there is currently absolutely no/v.little evidence of some of the ice shelves mentioned weakening or any prospect of them collapsing in the short – medium term.

Line 154: 'crumbling edges' – this implies that the coastline of the whole ice sheet is crumbling, this is not the case. Only in some regions.

Line 163-166: I think here a figure of ice shelf area loss is needed that excludes ice shelves that are simply undergoing a natural cycle

Figure 3: Just something for the authors to consider. The manuscript is set up in a way that highlights ice shelf retreat. Could the ice shelves that are actually retreating and contributing to this overall ice shelf retreat signal be highlighted here, rather than those that are simply undergoing their natural calving cycle, which may or may not calve from the same location in the next event?

Extended Figure 2: What do the colours of the circles represent? The location? I cant really tell from the inset?

Supplementary Information

Line 177: I am satisfied that the coastlines generated are roughly correct for the purpose of this pan-ice sheet analysis and I reiterate I think it is a very impressive composite dataset. It is impossible to be spot on with coastline mapping because in some regions it is subjective as the authors state in the case of Thwaites. But I think this caveat should be extended to any ice shelf with a similar disintegrating type front to Thwaites. I think it could also be worthwhile to state that anyone potentially interested ice shelf extent records for the individual ice shelves presented (which I am sure there will be!) should really double check the records on the imagery as well. There are a few spikes on the composite ice shelf area records that do not look geophysical in nature.

Author Rebuttals to Initial Comments:

To Senior Editor Michael White and the reviewers of this manuscript,

We thank the reviewers for their thoughtful, constructive feedback. To briefly summarize their reports, both reviewers who were asked to review the full study found our results of worthy significance for publication in *Nature* (Reviewer 2 was asked by the editor to focus on the modeling component), and all comments on our writing and overall presentation of the results were positive. All three reviewers, however, pointed out key elements that needed to be strengthened or clarified before publication. Specifically, Reviewers 1 and 3 took issue with our interpretation of calving cycles of Antarctica's largest ice shelves; and all three reviewers had at least some concern about the limitations of the ice sheet model. We respond specifically to each reviewer comment in the pages that follow, but here we briefly summarize our response to the two main concerns of the reviewers:

Timing of future calving events: Based on reviewer feedback, we have come to realize that the original presentation of our results may have encouraged an interpretation that was different than we intended. Reviewer 1 states, *the authors make the assumption that the ice shelf cannot grow beyond [its] maximum observed extent*, and Reviewer 3 states, *the assumption that glaciers will calve at their last known location is problematic*. We wish to clarify that we do not make any such assumptions in this manuscript, nor do our results depend on the timing or location of any future calving events.

We do place our findings in the context of the largest ice shelves' roughly periodic cycles of calving, which have been previously reported and are not a matter of controversy to our knowledge, but we have been very careful not to predict any exact timing or conditions under which future calving will occur. Rather, for the largest five ice shelves, we present their present-day extents, their observed growth rates, and the extents at which they experienced their last major calving event.

We have presented our observations this way because we believe the context is important, but we understand that we have a particular responsibility not to mislead readers about our predictive capabilities. **In this revision, we've included several new statements to be more clear about what we do and do not know about future Antarctic calving events.**

Modeling studies: Chief concerns related to the modeling component of our study are broadly that our model

1. is not sufficiently novel,
2. may inadvertently include effects of steep gradients at the grounding line, or
3. is not sufficiently complex to capture all relevant ice sheet processes.

In this revision, we have tried to emphasize the fact that our model is *not* novel in any way. Rather, it was designed to mimic several other instantaneous-response-based process studies, and we replicate the results of a previous study by Gudmundsson et al. (2019) to show similarities between the models. In the main text and in the methods section, we have tried to make this point more clear.

The concern about steep gradients at the grounding line is an important point that multiple reviewers caught and was an inadvertent omission on our part when describing the flux calculation. We have now clarified that we do not include nodes immediately adjacent to any step change in thickness, and therefore avoid calculating flux through any steep gradients.

Although we could not find any specific statements that were incorrect or terribly misleading in our initial submission, we sense a general skepticism among the reviewers about how well our modeled

grounding line flux might represent reality. This is because, as the reviewers point out, we neglect a host of processes and feedback mechanisms by isolating calving and thinning as separate independent variables. This is an important point, which we have dealt with by directly acknowledging the processes and feedback mechanisms we neglect in the text, and by reframing our results to focus more squarely on how calving and thinning each impact the *buttressing strength* of ice shelves. The distinction is subtle, but more closely describes the instantaneous stress response that we model, and carries less potential for confusion about the processes we do not model.

Our new focus on buttressing is particularly timely, as just last week a preprint by Bamber et al. (not cited in our manuscript, as it is not yet peer reviewed) identified ice shelf buttressing as the primary source of uncertainty in projections of Antarctica's future contribution to sea level rise (<https://doi.org/10.1002/essoar.10510825.1>). We believe our manuscript gets one step closer to understanding the processes that have had a measurable impact on buttressing over the past quarter century.

An additional note: In this revision, we have moved most of the Supplementary Information text into a new Methods section at the bottom of the main text document. To help keep track of which text has been altered in this revision, we have done our best to quote new text directly in the responses to individual comments below.

Legend for responses:

*Reviewer comment with **key points in bold***

Author response with **key points in bold**

*Manuscript text with **updated text in bold***

Referee #1 (Remarks to the Author):

*Many studies across the last ~20 years have focused on ice shelf thinning as a mechanism for ice sheet destabilization: thinning of ice shelves reduces buttressing forces on grounded ice, which in turn results in increased flux of grounded ice to the ocean. Fewer studies, however, have focused on the role of ice shelf extent change (calving) in moderating grounded ice flux. This study does just that by first showing Antarctic ice shelf area change across the last 24 years and then modeling the impact that further area change could have on ice flux. The central conclusion of the work is that the ice sheet is equally as sensitive to variations in ice shelf extent as it is to ice shelf thickness. These results are interesting because they serve as a sign-post for directing future research by showing that a previously ignored process is equally as important as a previously well-considered process. This is novel work with timely conclusions that is **well worthy of publication in Nature with only minor revisions**.*

The work presented here is strongly original: no previous study to my knowledge has attempted to quantify Antarctic-wide coastline change. This study quantifies annual coastline change from 1997-2021 using a variety of remotely sensed imagery that has been very carefully corrected: this in an of itself is a valuable contribution, and will be a widely-used dataset once it is made publicly available. Beyond the dataset itself, this work is novel because it highlights a relatively ignored — yet impactful — process. The community interested in ice sheet stability has paid huge attention to the role of ice shelf thinning in destabilizing marine ice sheets. Very little focus has been paid to the impact of changing ice shelf area on ice sheet stability. This work quantifies the importance of ice shelf area loss to ice sheet discharge and, in doing so, will redirect future efforts of the ice sheet community. The quality of both the experimental approach and treatment of uncertainty is excellent and the writing style is quite clear. The figures included in the main text are visually appealing. The references are appropriate and complete.

The supplement demonstrates the degree of care that the authors took in working with the continent-wide data used in this study. For example, many studies simply take the MEaSUREs V2 or BedMachine data products and do not manipulate/clean them further: I am impressed with the care that the authors took to fill holes and unreasonable regions within all of the datasets used here. The authors are to be commended for their commitment to being comprehensive in mapping a physically reasonable coastline.

***I have two minor concerns** with the work at present, though both should be readily addressed with some simple clarifications, discussion of assumptions, and a supplementary figure.*

We thank Dr. Riverman for the many kind words. In this revision, we've done our best to address all concerns, as described below.

***My primary concern is the usage of fixed grounding line positions and thicknesses** in the modeling efforts presented here. This is a significant assumption, and warrants additional discussion about the limits that it places on the final conclusions. It seems like this would result in significantly steeper grounding zone slopes than would otherwise be observed (e.g. if the grounding line is fixed in thickness and position but the shelf is dramatically thinned, the grounding line surface slope increases). These steep slopes promote rapid discharge, but if the grounding line were allowed to shift and thin, discharge could be lower. I think this effect would be the most pronounced in the case of thinning ice shelves and have less of an impact on calving of the shelf — this then places some bias*

into the comparison of the two cases (though I would think it biases in the direction of making calving an even more important effect). I recognize that an evolving grounding line position adds a significant level of complexity to the modeling efforts - I'm not suggesting that should be done. What about adding a one-catchment sensitivity test using a moving grounding zone to test the hypothesis that ice shelf thinning and ice shelf calving have a similar order-of-magnitude impact on grounding zone-discharge feedbacks?

We appreciate the point being made here. If we may put it into our own words, we have isolated calving and thinning each as independent variables in the model, but in reality, these two processes are never fully independent from each other, nor from other processes such as grounding line migration or inland changes in ice thickness.

In response to this important comment and similar feedback from the other reviewers, we have made a gentle, but very meaningful change to the way we present our findings. Specifically, **we now frame our results primarily in terms of the impact that thinning and calving have on buttressing, because the instantaneous-response provides a direct measure of the stress field within the ice.** We have reworded our results and interpretation throughout the paper, from this new sentence that we've added to the model description:

From any change in the modeled velocity field, we calculate change in grounding line flux as a measure of the instantaneous change in buttressing that occurs when the ice shelf geometry is altered.

to this final sentence of the abstract, with the new language appearing in boldface text:

*We model the impacts of Antarctica's recent coastline evolution **in the absence of additional feedbacks, finding that calving and thinning have produced equivalent reductions in ice shelf buttressing since 2007.***

We've altered the language throughout the manuscript to reflect the new focus on buttressing strength, and we make clear in the discussion that our model is simple and does not include any additional feedback mechanisms:

*Our approach only accounts for instantaneous changes in stress balance, and does not explore time-evolving ice sheet responses or any transient feedbacks that are known to cause further impacts on ice flow⁴⁵. For example, ice shelf thinning often leads to grounding line retreat⁴⁶, which itself can increase grounding line flux⁴⁷; calving can reshape an ocean environment and influence melt rates²¹; and increased melt can lead to a weakened ice shelf that is more susceptible to calving or collapse⁸⁻⁹. Accordingly, the modeled sensitivities we present offer a somewhat narrow, but insightful⁴¹⁻⁴³ view of how **the buttressing strength of ice shelves may be compromised due to thinning or calving, when each process is considered in isolation.***

In regard to the consideration about slopes along the grounding line changing as we alter ice shelf geometry, yes, this is an effect we considered and avoided when conducting our analysis, but failed to mention anywhere in the initial manuscript. We have now clarified in the Methods section that *we discard the grounded nodes directly adjacent to the ice shelf*, which is similar to way Gudmundsson et al. (2019) avoided calculating flux through a steep surface gradient.

*My secondary concern with the work as it stands focuses on the assumptions surrounding the projected timing of the next major calving events. **The authors make the assumption that the ice shelf cannot grow beyond its maximum observed extent, but there is no physical mechanism discussed as part of this.** The trajectory of the Ronne ice shelf I think best highlights my concern here: if the dataset only extended from 2005-present, we might assume that the ice shelf cannot grow beyond its 2020 calving location. The maximum possible extent of an ice shelf would seem to be the product of many environmental factors that are changing through time — can we assume that the maximum possible extent in 1964 is identical to the maximum possible extent today? This assumption warrants some discussion.*

We agree with the reviewer that such an assertion would be problematic and that we need to make it more transparent that no such assertion is made. We do not assume that ice shelves *cannot* grow beyond their maximum observed extents. We only present our observations in the context of decades of recorded calving history that took place before pan-Antarctic coastal surveys were possible, and we make the case that a continuation of the well-documented 30 to 60 year calving cycles of the major ice shelves would produce further reductions in total ice shelf area before the ice sheet is able to recover to its 1997 extents.

We do not preclude the possibility that calving could occur later than expected, especially by such a crude metric as total ice shelf area. We are not the first to report the 30 to 60 year calving frequencies of the major ice shelves, and the approximate timescales of the calving cycles for these particular ice shelves are not a matter of controversy as far as we know. We simply use what's known about these big ice shelves to present a better understanding of Antarctica's present place in history.

We have taken great care not to predict the exact timing of any particular calving event, but we do feel it's important to give context to our observations and help readers understand whether or not Antarctica is on a road to recovery. Accordingly, without making any assumptions or specific predictions, we include this statement in our concluding remarks:

Unless the Filchner, Ronne, Amery, and Ross ice shelves all grow beyond extents that have ever been seen before, a continuation of their natural cycle could further reduce Antarctica's ice shelf area ~60,000 km² if their recent major calving events are repeated in the near future.

Perhaps our strongest statement on future behavior comes earlier in the manuscript, which previously stated:

*An examination of the timing of major calving events from the Ross, Ronne, Filchner, and Amery ice shelves suggests **that they are all on track for major calving events in the next decade, which will cause further reductions in area well before the ice sheet has had a chance to recover to its 1997 extent.***

We have now tempered our language slightly, and the sentence now reads:

*An examination of the timing of major calving events from the Ross, Ronne, Filchner, and Amery ice shelves suggests **they could all experience major calving events by the end of the 2030s, which would cause further reductions in area well before the ice sheet has had a chance to recover to its 1997 extent.***

For an added measure of safety against any possible misinterpretation, we have now noted in the caption of Figure 3 that:

...dotted lines extend observed growth rates ... forward in time to indicate how long it may take for each ice shelf to reach the extent corresponding to its last major calving event.

Minor line-by-line comments

L62 *This sentence could use additional detail for how this prediction was made and the uncertainty surrounding it — either here or in the supplement.*

We have added this description of our method of estimation:

Using a least-squares fit, we find that Antarctica has gained area at a rate of $350 \text{ km}^2 \text{ yr}^{-1}$ since 2002, and at this rate it would take about 100 years to recover to the total area observed in 1997.

We then point out that Antarctica's five largest ice shelves could all experience major calving events in the next decade, but we add this word of caution:

We caution that the exact timing of future calving events cannot be predicted from past behavior; yet, a full recovery of Antarctic ice shelf area appears unlikely within any plausible range of timing for the next major calving events.

L81 *Is citation 39 not yet published?*

The manuscript is under consideration for publication in *Nature Geoscience* (NGS-2022-03-00494). If our article is accepted and the Paolo manuscript is still in review at press time, we will request that Dr. Paolo upload his manuscript to a preprint server to meet *Nature's* guidelines.

L96 *Would this response be different if the removal of buttressing was ramped across several weeks to months instead of being instantaneous? This is perhaps a large rabbit hole, but I would be interested to read a few sentences in the supplement defending the choice of instant removal.*

The question regards this line in which we introduce the model:

We employ a simple instantaneous-response approach that calculates the change in the ice sheet's stress balance in response to an instantaneous change in ice shelf area or ice shelf thickness.

We immediately follow the above statement with a clarification about the most important limitation of the instantaneous-response model:

Our approach does not consider the effects of time-evolving feedbacks, such as grounding line migration or changes in grounded ice geometry, but closely mimics previous studies of grounding line flux sensitivity to thinning and calving^{2-3,40-43}.

To assist readers in interpretation, we offer this guidance when discussing our results:

Our approach only accounts for instantaneous changes in stress balance, and does not explore time-evolving ice sheet responses or any transient feedbacks that are known to cause further impacts on ice flow⁴⁵. For example, ice shelf thinning often leads to grounding line retreat⁴⁶, which itself can increase

*grounding line flux⁴⁷; calving can reshape an ocean environment and influence melt rates²¹; and increased melt can lead to a weakened ice shelf that is more susceptible to calving or collapse⁸⁻⁹. Accordingly, the modeled sensitivities we present offer a somewhat narrow, but insightful⁴¹ view of how **the buttressing strength of ice shelves may be compromised due to thinning or calving, when each process is considered in isolation.***

We also point out that the instantaneous-response model is particularly well suited to calving studies, because:

...calving and ice shelf collapse can occur suddenly, with little warning, and can produce immediate increases in grounding line flux and sea level rise.

For further guidance on the ways in which an ice sheet may respond to various types of changes, we point readers to several papers that describe the setup and results of more experiments than we can discuss in this paper.

L103 *Does this mean the model is run to steady-state for the new configuration?*

We have clarified that we solve for the *instantaneous* response, which is another way of saying that the model is not run to steady state for each new configuration. The sentence now reads:

*For each model experiment simulation, we alter the ice mask or the floating ice thickness, and then solve a stress-balance equation to calculate the **instantaneous** velocity response to the ascribed perturbation (see Methods).*

L125-127 *Super interesting result — I wouldn't have predicted this.*

The concavity of the calving-response curves in Fig. 4d is particularly strong, reaffirming that calving can have increasingly large impacts on buttressing as an ice front retreats toward the grounding line.

We also found this noteworthy, but perhaps it should not be surprising. The shelf ice closest to the grounding line does the most to mechanically restrict the flow of ice across the grounding line, and that makes sense. Fürst et al. did a nice job of mapping the effect, but still we find it insightful to see how calving events can become more impactful as they grow closer to the grounding line.

Supplement L177 *This is an excellent pun.*

We appreciate the reviewer's attention to detail.

Figure 2: *I find this figure confusing. It seems to suggest widespread synchronous advance and calving events? EG a widespread advance event in 2007, a widespread retreat event in 2009.. etc. I don't think these events occurred, so it makes me think that I must be interpreting this figure wrong. Clarify in some way.*

After some consideration, we think we've identified the source of the confusion. This stacked-area plot displays the area anomalies of each ice shelf as visible area in the figure, so when any ice shelf changes in area, the patch of color corresponding to other ice shelves may shift up or down to keep the chart neatly stacked. The effect is most clearly seen in 2009, when the calving of Mertz Glacier Tongue produces a downward shift in the area displays of all ice shelves below it in the figure. The visible area of each ice shelf below Mertz does not change when Mertz calves, even though they each get shifted down a bit.

To clear up any confusion, we've added this text to the figure caption:

Note that ice shelf area anomalies correspond to their visible areas rather than distance from the zero axis, so the downward shifts seen in many ice shelves toward the lower end of the figure in 2010 and 2017 correspond to major calving events at Mertz and Larsen C, respectively.



Figure 3: This figure would benefit from some kind of differentiation between observations and inferences. For example, are the “zero” extents from pre-1995 inferred or based on observations of some kind? Or is the timing of calving known from some other kind of observation and the assumption is that it calved back to that location?

We have added this description to the caption to help readers differentiate between observations and inferences:

Solid lines connect observations presented in this study, while dotted lines extend observed growth rates back in time to known calving events and forward in time to indicate how long it may take for each ice shelf to reach the extent corresponding to its last major calving event.

The caption then goes on to describe in further detail,

At Amery Ice Shelf, we measure a growth rate of $177 \text{ km}^2 \text{ yr}^{-1}$ from 1997 to 2019 and extrapolate it back to 1963, when a large $10,000 \text{ km}^2$ calving event occurred³². Amery's history suggests the 2019 calving of iceberg D28³³ occurred as the ice shelf neared its 1963 extents, and extrapolation into the future suggests another calving event may occur at Amery by about 2030. Similarly, a constant growth rate of $220 \text{ km}^2 \text{ yr}^{-1}$ since the Filchner Ice Shelf calved $11,500 \text{ km}^2$ in 1986³⁴ suggests Filchner may be due for another major event in or before the mid 2030s.

Extended Data Figure 3: A) are the contours meters of ice thickness? I'm not sure what I am supposed to be seeing in this figure, particularly in A). It looks like there is very little pattern to where there are positive and negative acceleration. Visual inspection of any trends would be easier if the dots were larger.

We have removed the figure from the manuscript in this revision.

Referee #2 (Remarks to the Author):

I am reviewing the manuscript of Greene et al., 'Antarctic calving loss rivals ice shelf thinning', and was asked to assess the ISSM modelling part. The modelling part explores the isolated response of the grounding line flux to loss of buttressing by calving or ice shelf thinning over the observational period until 2021. The simulated grounding line flux responds linearly to the forcing whereby the glaciers in the Amundsen Sea Embayment show the strongest response. In addition, experiments are conducted where the buttressing (either calving or thinning) is incrementally reduced until a complete collapse of the ice shelf occurs. These future scenarios suggest that the total ice shelf loss of Ronne, Filcher, and Amery would most significantly impact the grounding line flux.

Although I found the results very well described and presented, I have several concerns about the modelling part:

*1. I am not convinced that the (ice shelf) thinning experiments produce reliable grounding line flux results. Just changing the ice thickness of the ice shelf seems odd as strong geometric gradients are likely introduced that distort the results. **Gudmundsson et al. (2019) explained in detail how to ensure that the driving stress is unaffected by changing only the floating ice thickness;** additionally, they performed some sensitivity experiments. **Are you following this methodological approach?** However, I think the ice surface over the grounded ice should also be changed (see my comment to Line 314/315).*

Yes, we are following a nearly identical approach to Gudmundsson et al. (2019), which we mention very briefly in the main text:

Our approach...closely mimics previous studies of grounding line flux sensitivity to thinning and calving^{2-3,40-43}.

Going into more detail in the Methods section, we note that we recreate the results of Gudmundsson et al. (2019) almost exactly, which we show in Extended Data Figure 7, whose caption states:

Extended Data Figure 7: Comparison to Gudmundsson et al., 2019. *To help validate the ice sheet model, we replicate the results from Fig. S12 of Gudmundsson et al. (2019). This figure shows the instantaneous velocity response resulting from step change in thickness from 2010 to 2017 observations.*

Although we stated the above in the initial version of the manuscript, the reviewer brings up a good point that we failed to directly address whether or not such geometric gradients might influence our results. The reviewer's intuition here is correct (and Reviewer 1 brought up the same point) that the stress balance measured directly in between unaltered and thinned nodes could be influenced by strong slopes between those nodes. To avoid this issue, we use a one-node-away approach similar to that which was implemented by Gudmundsson et al. (2019) to deal with the same issue. In this revised manuscript we've added a note to the Methods section stating that *we discard nodes directly adjacent to ice shelves* before calculating grounding line flux.

2. In the whole manuscript, I missed a clear sentence that calving front retreat and ice shelf thinning are closely related to each other. They are not rival processes as the title suggests (BTW, the word 'rival' does not appear in the text). Both processes interact, i.e., calving front retreat can initiate

velocity increase and therefore ice shelf thinning. The other way around, ice shelf thinning might trigger higher calving frequencies. It is unclear whether the authors think that the modelled grounding line flux anomalies from both experiment types should be added to get the full response (Line 19-21)? I guess no. So, it should be made clearer that the modelled grounding line flux anomalies from both isolated experiments should not be added to get the full grounding line flux anomaly (i.e., sea-level equivalent). Your model experiments could be extended by running a full-response scenario, i.e., combining the calving and thinning experiments.

We have added the following section to clarify that ice shelf thinning and calving are not opposing forces, but are related to each other, and often act in tandem:

In nature, ice shelf thinning and calving are not entirely separable, as one can lead to the other, and they are often driven by the same regional influences; yet, we separate the two processes here to identify their relative importance and to better understand such phenomena as thinning in the absence of major calving events, or hydrofracture-driven ice shelf collapse, which does not strictly require prior thinning⁴⁵.

The suggestion of conducting a combined calving and thinning model run is a good one! In fact, it was our initial intention. However, we soon realized that all available thickness observations were generated with a-priori assumptions of a fixed ice shelf mask. In our model run of the effects of observed thinning, we held the ice shelf mask constant at the minimum extents observed over the altimetry period in an effort to restrict the experiment to a single, well-understood independent variable. Reconciling the dH/dt data with the varying coastlines would require a bit of extrapolation of the dH/dt data to meet observed coastlines, which isn't impossible, but would involve several decision points and would introduce more variables into the experiment. This could certainly be done, but we felt it would muddy the waters about exactly which independent variables we were testing, and we anticipated a lengthy discussion of all of the agreements and disagreements between all of the model runs. We like the idea of a combined model run, but for the added complexity and potential for distraction from the main results (our calving front observations), we feel it's best to test only one independent variable at a time.

We do agree that readers might want some guidance for how to interpret our results of the effects of thinning and calving in isolation. To help them, we have also added this clarification to help readers understand the effect of isolating calving and thinning in the model:

Using a stress-balance approach, we test thinning and calving independently, but their combined effects on buttressing may be approximated by a linear combination of the independent experiments when perturbations are small.

Regarding the title of our paper, we employ this definition of *rival* from Merriam-Webster:

- · · to possess qualities or aptitudes that approach or equal (those of another) · · ·

We believe this is a very good word to describe the relationship we find between thinning and calving. The mass loss we report due to calving nearly equals the mass lost due to thinning over the same period, and in recent years the modeled impact of calving on grounding line flux has approached similar magnitudes to that of ice shelf thinning.

3. A retreating calving front or ice shelf thinning reduces buttressing and increases the grounding line flux is not new.

Agreed. Decades of observations and modeling studies have repeatedly shown the importance of ice shelves, and **we do not imply any novelty in our findings that ice shelf loss to thinning or calving can each induce an increase in grounding line flux**. We cite a pan-Antarctic model by Reese et al. that shows grounding line flux sensitivity to thinning, and a comparable study by Furst et al. that models the impact of calving on grounding line flux. We also cite recent modeling work by De Rydt et al. (2021) and Joughin et al. (2021) which each modeled the instantaneous response of Pine Island Glacier to recent observations of thinning and calving. **The only novelty of our modeling study is that we have applied these same methods using pan-Antarctic observations of calving front evolution.**

3a. The full collapse of the AIS ice shelves buttressing effect (upper-end member) on the grounding line flux was shown by the ABUMIP experiments (Sun et al., 2020). Your full-collapse calving experiment is comparable to ABUK and your thinning experiments to ABUM. So, ABUMIP explored these processes also in isolation but with several transient ice flow models (not instantaneous response approach). ABUMIP also showed that Filchner and Ronne are the largest sea-level driver under this scenario.

In this revision, we have added a few more citations to the ABUMIP paper by Sun et al. to acknowledge the similarities to our findings (e.g., the prominent role of the Ronne Ice Shelf in present-day buttressing) as well as the differences between our instantaneous model and transient runs that include extra feedbacks. We mention the ABUMIP paper here, for example, to point readers to Sun's results using a suite of ice sheet models that include transient feedbacks:

Our approach only accounts for instantaneous changes in stress balance, and does not explore time-evolving ice sheet responses or any transient feedbacks that are known to cause further impacts on ice flow^{41,45}.

We have added this sentence to acknowledge Sun's previous results of the end-member cases of total ice shelf collapse:

It should be noted that there is no evidence that the Ross, Ronne, Filchner, or Moscow University ice shelves are under any imminent threat of collapse, but we mention them here only to reaffirm their previously reported role⁴¹ in holding back a rapid influx of glacial ice into the ocean.

And we point to Sun et al. as an example of what can be learned by examining the effects of such hypothetical scenarios as total ice shelf collapse:

Accordingly, the modeled sensitivities we present offer a somewhat narrow, but insightful⁴¹⁻⁴³ view of how the buttressing strength of ice shelves may be compromised due to thinning or calving, when each process is considered in isolation.

3b. Gudmundsson et al. (2019) performed a similar study, but used a different observational period and conducted only the thinning experiments. So, what are the new key aspects compared to ABUMIP and Gudmundsson et al. (2019)?

Correct, Gudmundsson et al. (2019) performed a very similar study, which we cite in reference to a statement that:

Our approach...closely mimics previous studies of grounding line flux sensitivity to thinning and calving^{2-3,40-43}.

We also replicate the results of Gudmundsson et al. (2019) using ice shelf thinning observations for the same exact period, and include an Extended Data figure to show that our model produces very similar results. The only thing new in our modeling work compared to Gudmundsson et al. (2019) is the inclusion of a variable ice front position.

We also point readers to the ABUMIP paper by Sun et al. as an example of much more complex modeling work that includes feedbacks associated with a transient run.

Minor comments

Abstract

Line 15, 17 and 18 For a journal like Nature, I would use percentage values instead of the net values.

We have added a percent to the measure of area in the abstract:

...finding a net loss of 36,701±1465 km² (1.9%) of ice shelf area...

but we keep the mass values in Gt, because these numbers are very important to ocean/climate modelers who have not accounted for this massive influx of freshwater in their models, while the percentage of ice shelf mass isn't terribly meaningful.

Line 17 In order to be consistent with the title, I would replace "ice front retreat" with "calving"

We understand the rationale here, and would generally agree, but in this particular instance we want readers to understand we're discussing the mass loss associated with a change in ice front position, which we don't wish to be confused with a steady-state measure of calving rates.

Main

Line 26: Please slightly rewrite. Melt is expected to increase at the margins. But in the interior SMB will increase and in general the total SMB will increase (e.g., Fig 3b in Rodehacke et al., 2020)

Thanks for the clarification. We have made the change as follows:

*As the climate warms, Antarctic surface melt is expected to increase **along the periphery** and potentially lead to rheological weakening⁴, hydrofracture⁵, and the partial or wholesale collapse of ice shelves around the continent⁶⁻⁸.*

Chapter "Antarctica's evolving coastline" not reviewed.

Noted. [The reviewer is indicating that they focused on other sections of the manuscript, as requested by the Editor.]

Line 94 Suggest to drop NASA. Is this important? Please add reference to ISSM.

We have replaced **NASA's Ice-sheet and Sea-level System Model (ISSM)** with **the Ice-sheet and Sea-level System Model (ISSM)**, and we have referenced it to *Larour et al., 2012*.

Line 100 *The number of 20 million elements tells me nothing. What is the horizontal resolution in critical areas (e.g., grounding line)? Are you setup a 3D mesh? Why is such a high-resolution needed?*

For this study, the most critical areas are the floating ice elements that change between states of ice and no-ice (due processes of calving and regrowth). By employing very high spatial resolution in these areas, we are able to capture small changes in the ice mask from year to year. We have updated the wording now to state this and note that in these areas the model mesh has a horizontal resolution of 100 m or finer. We also make note here that the mesh is 2-dimensional and reference the Methods section (where we justify the use of the SSA equations for our experiments), as follows:

To best capture the effects of small, annual changes to the calving front, we employ a 20 million element 2-dimensional model mesh with a dense, 100 m spatial resolution in areas that advanced or retreated during the observational period, and coarser resolution farther from the coast (see Methods).

Line 100 *Please provide a date of initialization.*

The model is initialized to a present-day state, but on a mesh that is a domain extended to the maximum ice extent from 1997-2021. Here, we have added wording to clarify to the reader that the model initialization datasets (geometry, mask, and velocities) are described in the Methods: Extended thickness, velocity, and mask grids section of the manuscript, as follows:

Model initialization follows that described by Schlegel et al. (2018) with adaptations outlined in by Seroussi et al. (2020) for JPL_ISSM, including a step for data assimilation⁷² of the present-day geometry conditions and ice surface velocities (Gardner et al., 2018) derived by this study on an extended mask (see Methods: Extended thickness, velocity, and mask grids). The assimilation step serves to infer the unknown basal friction coefficient for all grounded ice and the ice viscosity for all floating ice.

Line 101 *What is the date of the observed dataset? It is rather close to the 90s or the 20th century? Please add a reference.*

We now mention in the main text that the nominal date of the observed velocities and thickness corresponds to the early 2010s. It's hard to succinctly be more specific than "the early 2010s", as we've combined multiple datasets that were collected over decades (described in detail in the Methods section), but most of the velocity data comes from the first two years of Landsat 8 (2013-2015) and thickness was built primarily on REMA for floating ice, whose nominal date is mostly in the 2013-2015 range. Thickness changes over grounded ice over the past few decades of radar data collection are mostly negligible, as they're within the range of uncertainty of radar bed picking. In addition to the detailed Methods section, this now appears in the main text:

The model is initialized with velocity and thickness observations collected primarily in the early 2010s, which we extend to the maximum observed extents of the ice sheet where necessary (see Methods).

Line 101/102 *suggest to drop "... followed by a short relaxation simulation". I think, this technical aspect is not important for the main text. It also does not fit to your statement above (Line 97/98) that*

time-evolving feedbacks are not considered.
What is the motivation for the relax run?

Thank you for this suggestion. We have dropped this statement in the main text, and the methods for relaxation are described only in the Methods: Ice Sheet Model section. In this section, we now explain the motivation for our relaxation run as follows:

.. we run the model forward for 1 year, holding the grounding line location constant, but allowing the ice geometry to relax in response to the initial model configuration, forcing, and boundary conditions which are not temporally consistent datasets. This procedure is particularly critical step for modeling the areas of the domain which are extended beyond that of the present-day ice sheet, and are, in most cases, extrapolated outward from current observations (see Methods: Extended thickness, velocity, and mask grids). This procedure ensures that the model geometry is consistent with modeled ice velocities prior to conducting any perturbation sensitivity experiments.

Line 102/103 *You should say, that you only alter the ice thickness of floating ice.*

We have now clarified in the text that we only alter the **floating ice thickness**.

Line 103 *'.. solve for stress balance". I found that a bit too technical. Maybe just: '.. solve for ice velocities to estimate the change in grounding line flux due to the prescribed. perturbation".*

We see that we used the term *stress balance* in such a way that it would difficult for non-experts to infer its meaning. It's an important concept for the instantaneous response calculation, so we'd like to keep the term in the main text, but we hope that this rewrite is more clear:

For each model experiment simulation, we alter the ice mask or the floating ice thickness, and then solve a stress-balance equation to calculate the instantaneous velocity response to the ascribed perturbation (see Methods).

Line 104 *replace 'model' with e.g. simulation results.*

Done.

Line 105 *In the 'observational' chapter before you are referring observations starting at 1997. Why are your simulations start in 1992?*

We understand the potential point of confusion. We have now clarified in the Methods section that:

Historical calving experiments are conducted for the years 1997 to 2021 which correspond to our calving-front observations, and historical ice shelf thinning experiments are conducted for the years 1992 through 2017, when pan-Antarctic ice shelf thickness observations are available.

Line 106 *Linear to what?*

We have clarified that the rate of thinning has been approximately linear over time. The section now reads:

*Around Antarctica, the overall rate of thinning has been approximately linear **over time** throughout the satellite record²³...*

Line 107 *Maybe I missed that, but why is the linear behaviour expected?*

We believe that this question is answered in the next sentences, which begins:

In agreement with previous modeling studies⁴⁰ and observations⁴⁴, our model shows a roughly linear velocity response to recent ice shelf thickness changes...

Line 111 *add "... response of the grounding line flux ..."*

The reviewer's intention here was to insert *grounding line flux* for clarity, but in light of our new focus on buttressing, we've reworded the sentence as follows:

*In addition to testing the effects of ice shelf thinning, we model the **effects of changes in buttressing** that result from observed changes in calving front geometry.*

Line 112 *replace "coastal evolution" with "calving front evolution"*

Done.

Line 112 *replace "calving front geometry" with "calving front extent".*

Done.

Line 121 *What is present-day? Is present-day equal to initialization?*

We understand the potential point of confusion. We have clarified the text, which now reads:

*Thinning was applied to each node as a percentage of the present-day thickness **that was used for model initialization**...*

Line 197/198 *Please provide the ISSM version (or revision number). Also, I think, a code snippet how you maintain the 10.000m/a velocity threshold (SI Line 338) is required; to my knowledge this isn't ISSM's standard.*

We have added a note in the main text that we use **ISSM version 4.19**.

We have also added this clarification to the description in the Methods section:

We note that the 10,000 m yr⁻¹ velocity threshold was applied in the analysis after the model was run, and it is not part of ISSM.

We describe the velocity thresholding further in this section:

... velocity of any node that suddenly exceeds 10,000 m yr⁻¹ was set to its last velocity in the experiment before it exceeded the velocity threshold. We note that the issue of instability is rare, however, and our need to govern velocities never affected more than 407 of 10,529,015 nodes in any particular run.

If any questions remain about the velocity thresholding, our code is available for inspection on GitHub. (Currently private, but will be made public upon acceptance of this manuscript.)

Supplementary Material

The Supplementary Material consists of 37 pages without chapter numbering. The reference in the main text to “Supplementary Material” required a lot of searches for the reader to what material you are referring to.

Our apologies. There was some confusion on our part regarding how to properly follow the *Nature* formatting guidelines for Methods sections that exceed the ~3000 word limit (our initial Methods section contained about 5000 words). The editor has since clarified that we should move the description of the Methods into its own supplemental section, and so we have made that change in this revision.

Line 280 *Why 1992? Your observational dataset starts in 1997.*

Thanks for catching this typo. It's fixed now.

Line 281 *How are you dealing with initial ice thicknesses that are beyond the BedMachine ice mask extent?*

Our method for dealing with ice thickness beyond the BedMachine ice mask extent is described in detail in the Methods section under the heading **Extended thickness, velocity and mask grids**.

Line 282 *100m AND 75km in the interior? What do you mean with finer? Finer than 100m in the interior? I guess you have 75km in the interior and 100m in critical areas, right? By the way, have you considered to construct a mesh for each calving experiment where the annual calving fronts determine the model domain? So, you could avoid the high resolution of 100m at the calving front. Also, the thinning experiments don't need the high resolution of 100m at the calving front. In my opinion you could save a lot of elements, i.e., computing time, by improving the mesh. Or just state, you employ the same mesh for all simulations to avoid numerical issues.*

Yes, this interpretation is correct. Critical areas have a horizontal resolution of 100 m, while the interior has a spatial resolution of 75 km. We have rephrased this wording to be clearer as follows:

The model resolution ranges from 100 m in areas with the largest gradients in surface elevation and ice velocity, to 75 km in the Antarctic high interior.

We also have added a statement to clarify that in order to ensure numerical consistency, we use the same mesh for all experiments:

To maintain numerical consistency between all experiments, we use the same high-resolution model mesh initialized as follows.

Line 288 *Are the layers equally distributed?*

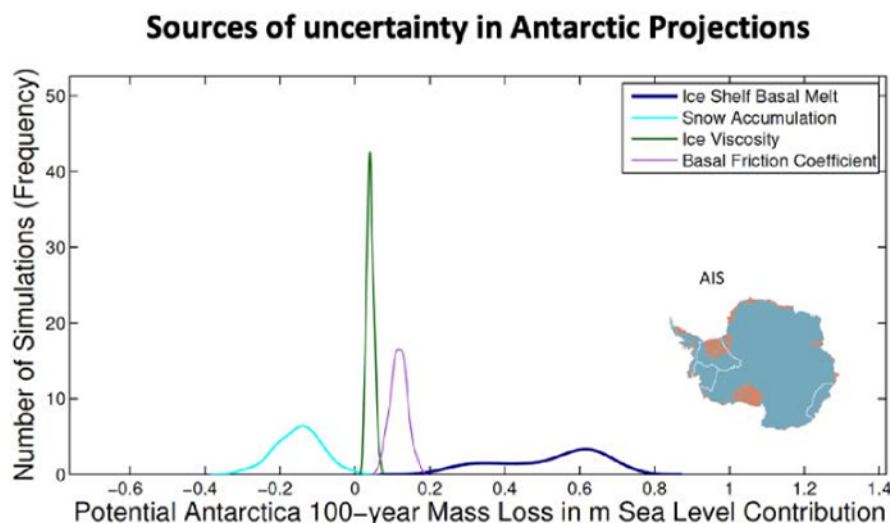
The 15 layers are not evenly distributed. There are more layers near the base, and they are distributed such that they increase by 10% with height. We now state this in the text.

For ice viscosity over grounded ice, we take advantage of ice viscosity calculated on a lower-resolution mesh by Schlegel et al, (2018), using a mesh initialized with a geometry from BedMachine v2, extruded to 15 layers, distributed such that they increase by 10% with increased height.

Line 292 *In my opinion, depth averaging the temperatures results into too stiff ice. Note, that most of the deformation takes place at the ice base where we have much warmer ice than over most of the ice column. The effort to reach a good thermal state by relying on the method by Schlegel et al (2018) is lost by depth averaging the temperatures. Not sure, whether this will influence the results or not ... Maybe a few tests with constant temperatures, i.e., rheologies would help. Moreover, you dismiss the thermal information by inverting for the rheology in the floating ice (Line 297). In my opinion, the strategy to get the thermal information is useless and could be replaced by a simpler approach. Generally, I like the approach to start with a good thermal state but here it is levelled out by the subsequent steps.*

The reviewer is correct. Here, we follow typical ISSM setup procedures, where we invert for ice viscosity on floating ice (Khazendar et al., 2011, doi:10.1029/2011GL046775.), to account for the ability of ice to effectively flow due to qualities not directly related to temperature (e.g. fracture). In the case of floating ice (i.e. in the absence of basal friction), we consider ice rheology to be the ice parameter with the largest uncertainty. As a result, we find that for floating ice it is more accurate to invert for ice viscosity than to directly calculate it from estimated ice temperatures. Since we are running a 2D model, we consider this an appropriate treatment for the determination of ice rheology on floating ice (Schlegel et al., 2018). On grounded ice, in contrast, there is basal friction which is likely the largest unknown in the flow equation. Therefore, in the grounded regime we invert for basal sliding, which then requires a physical model estimate ice viscosity. To save resources, we take advantage of the solution from Schlegel et al., 2018, as extrapolation of our high-resolution mesh into 3D space would be computationally demanding. While not ideal, we consider this approach to be highly practical.

With regards to model sensitivity to viscosity values, we note that Schlegel et al., 2018 investigated how ISSM Antarctica transient runs respond to uncertainties in various input and boundary conditions. They found that 100-year projections were highly insensitive to unknowns in ice viscosity (and basal friction) in comparison to unknowns in ice shelf melt rates (i.e. ice thickness). For example (after Schlegel et al., 2018):



Schlegel et al., 2018 also explored model sensitivity to full ice shelf collapse, revealing that model response to this loss of ice-shelf buttressing is larger than that resulting from ice viscosity. With respect to these transient results, we do not expect our instantaneous perturbation studies to be largely sensitive to the possible ranges of ice viscosity given the range of basal melt rates and perturbations to the floating ice mask explored here. This expectation is supported by the results of Larour et al., 2012, (doi:10.1029/2011JF002146) who concluded that modeled instantaneous grounding line flux of Pine Island Glacier is more sensitive to errors in ice thickness than to unknowns in ice viscosity or in the basal friction coefficient.

Line 296 ! think you should state which kind of friction law you use (linear or non-linear) and motivate your choice. The sensitivity in modelled grounding line flux is likely dependent on this choice (see the ABUM!P experiments, Sun et al. (2020)).

We have added a statement noting that our basal friction treatment, employed by default in ISSM, follows a Budd-type linear viscous friction law after Larour et al, 2012. As a result, this choice likely represents a conservative treatment of flux response across the grounding line compared to non-linear, as seen in Sun et al., 2020 and previously reported by Bulthuis et al., 2019 (doi: 10.5194/tc-13-1349-2019), but results in a more sensitive grounding line flux as compared to a Schoof/Tsai or Weertman law in response to ice shelf thinning (Brondex et al., 2019, doi: 10.5194/tc-13-177-2019.).

Line 298 Concerning the reference to Fig S1, ! was now expecting a figure about your 'match' instead of just showing me a figure of observed velocities.

Now that you point it out, we see how the reference to Fig S1 was confusing. We have removed the reference to the figure.

Line 299 So, you allow the model to relax. Also, the grounding line? ! think it's important to mention that this gives you a different grounding line than BedMachine (as mentioned in Line 345). ! don't understand '... relax to all model forcing'. And what does '... relax ... to the domain which extends beyond that of the present-day ice sheet' mean?

As mentioned in the above response with regards to model relaxation, we do not allow the grounding line to relax. In this case, the floating ice mask is held to that of the BedMachine product. Only the ice thickness and velocities are allowed to equilibrate to the imposed initial conditions over the 1-year relaxation period. This procedure prevents numerical inconsistencies - that result from the use of diverse datasets in model initialization – from affecting our sensitivity experiments. This is especially critical because the model domain is initialized to a domain extent beyond that of present-day, as described in the section Methods: Extended thickness, velocity, and mask grids. It also ensures repeatability in our experiments, and increases confidence that the differences between the control and perturbed solutions are due solely to our imposed changes to model geometry. We agree that it was not clear that the model domain and geometry is initialized to be consistent with extended thickness, velocity, and masks described in manuscript. We hope that the rewording and reference to the extended Methods section, as noted in our above response, clarifies the motivation behind our relaxation procedure.

Line 313/314 drop “levelset”. This is !SSM language. Just ice mask or ice-covered regions.

The word levelset has been removed from the sentence as suggested.

Line 314/315 ! am very concerned by this approach. Just altering the geometry of the floating ice (keeping the floating-grounded ice mask based on the end of the relaxation run fixed) would probably **cause steep gradients of the geometry** (both ice base and ice surface) at the grounding line. These steep geometry gradients could cause a strong velocity in-/decrease, i.e., change grounding line flux. ! am wondering, why the authors are not using observed ice thicknesses to construct a new ice sheet without a discontinuous gradient in geometry at the grounding line. Of course, grounding line flux anomalies are more difficult to calculate (since the shape and length of the grounding line changes), however, you might define drainage catchments etc. to calculate anomalies ... !n the present state, ! don't believe you are getting reliable grounding line fluxes in the “thinning” experiments.

This point is well taken, and we have done our best to address this concern above, in response to Main point #1.

Line 324 add “... of individual ice shelves”

Done.

Line 324 Would call the chapter “Model comparison” instead of “validation”.

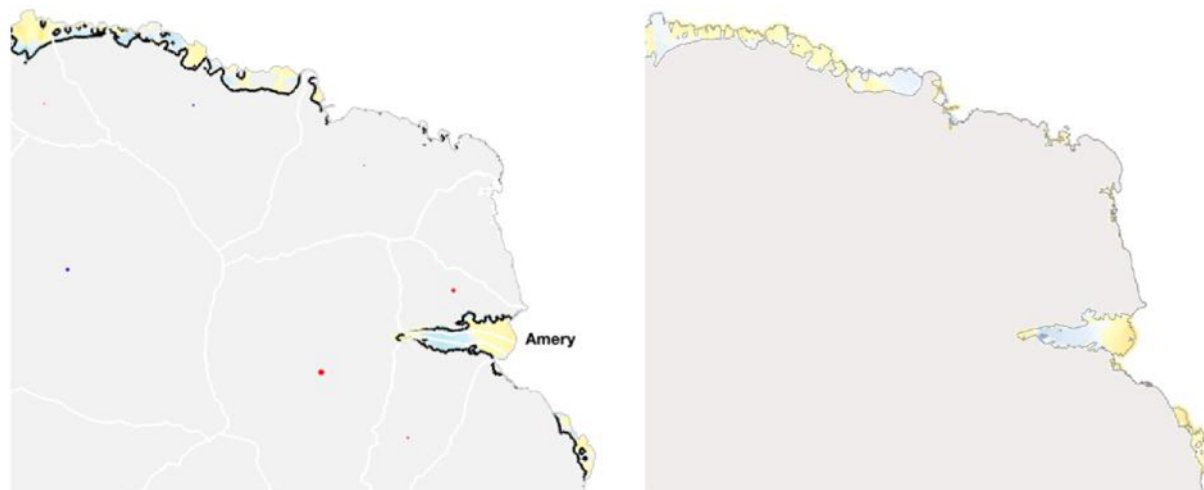
Fixed.

Line 328 Gudmundsson et al. (2019) is not listed in the S! references. However, the ref is provided in the main text. Not sure, whether it should be also appearing in the S! refs.

According to the *Nature* guidelines, references that appear in the in the main text should not repeated in the SI reference list.

Line 328 !t seems that your comparison to Gudmundsson et al. (2019) is quite ok, but can you please use the same colorbar to make comparison easier?

We think alike. Our very first approach was to match Gudmundsson’s figure exactly, which we provide here for comparison, with Gudmundsson on the left and a recreation using our model run on the right. Both are zoomed in here for clarity:



The trouble with Gudmundsson’s colormap, however, is that it’s not a true diverging colormap. It appears to be a simple mashing together of Cynthia Brewer’s “Blues” colormap on the low end, and her “YlOrRd” colormap on the high end. The result is a discontinuous jump at the zero mark, which can instantly switch the perception of very small signals to appear either decidedly positive or decidedly negative. We see here that our results match Gudmundsson’s almost perfectly for Amery, but toward the top end of the figure, where signals are very small, we get the false impression of tremendous disagreement.

Although we like the idea of replicating Gudmundsson’s figure exactly, down to the colormap, we think it’s best for publication to stick to scientific-grade, perceptually uniform colormaps. After all, someday, someone might want to try to replicate *our* results.

Line 324-328. *Is your model now validated or not? Based on which criterion? Is Gudmundsson et al 2019 using the same model, data etc? Please help me to understand your validation approach.*

These are fair questions about a section that was previously titled **Model validation**. We have now adopted the more apt suggested title **Model comparison**, and in doing so, we may have deflated some of the present concerns about whether we have fully validated the model in this section. Rather, we simply describe a comparison of our model to the previous study by Gudmundsson et al.

Nonetheless, we see that the overarching question of model validation may feel unresolved, so we have added the following clarifications at the start of the section:

An ice sheet model can never be fully validated until nature sweeps through a range of test scenarios and complete observations come available to measure the ice sheet response. Until then, we can only use the observations that are available to tune and evaluate the performance of our model, and we can compare against other models that have been accepted by the community. In this work, we tune the model to match velocity observations, we compare modeled grounding line flux to observed grounding line flux,

and we perform a model run to compare directly with published results from a previous modeling study that uses a very similar setup to ours.

Line 339 *Why instability? Values of 10000m/a are observed at glaciers, e.g., Jakobshavn.*

We see that our description of the issue was somewhat vague. We have adjusted the wording to clarify the cause of the issue, which only occurs in ice shelf collapse scenarios, when an ice rumple in the middle of an ice shelf suddenly becomes a disconnected island in the middle of the ocean:

*We found that in a few nodes **corresponding to small, grounded ice rumples or ice rises, total loss of the surrounding ice shelf** produced unrealistically high velocity responses. To address the issue, the velocity of any node that suddenly exceeds 10,000 m yr⁻¹ was set to its last velocity in the experiment before it exceeded the velocity threshold. We note that the issue of instability is rare, however, and our need to govern velocities never affected more than 407 of 10,529,015 nodes in any particular run.*

We hear the point that the velocity threshold could potentially be too strict, because 10,000 m/yr is physically plausible as seen at Jakobshavn, but this is well above any velocity of grounded ice ever observed in Antarctica, and the problematic nodes are easily identifiable as they tend to suddenly jump well beyond this threshold. We didn't find any significant effects of varying the threshold, and at 10,000 m/yr it never affects more than 0.004% of nodes in any particular run.

Line 340 *Maybe it occurs where you generate steep geometry gradients by keeping the grounding line fixed in the thinning experiments (see my comment to Line 314/315). Are the nodes located at the grounding line?*

We have clarified in response to the comment above that the issue only occurs in a few nodes, when tiny ice rises that were once bound on all sides by a continent-sized ice sheet suddenly become detached little ice sheets of their own.

Line 343 *Well, I agree, that the geometry change at grounded ice is slow. However, your statement "the ice has not been given time to draw down" is incorrect. According to the mass transport equation or the free surface equation, the geometry over grounded ice will change over one time step. Over one year I would expect some changes at grounded ice at least to maintain a consistent geometry at the grounding line (see major point 1). Also, in line 149 you say that ice shelf thinning occurs slowly. This is contradictory to the statement here.*

Here, we describe our analysis of the *instantaneous* response experiments, and there are no timesteps over which change is measured in an instantaneous-response analysis. The one-year relaxation period is conducted prior to any of our sensitivity experiments for purposes of initialization. For the sensitivity perturbation experiments, the model velocity responses to perturbations are only evaluated against each other, for consistency. For clarity, the text has been updated to reflect this logic as follows:

In the instantaneous response, grounded ice thickness change is neglected, because by nature, we only solve for a stress balance without consideration to mass transport; therefore the ice does not transiently draw down.

Line 347/348 *Not sure why you reference Gardener et al (2018). I mean, grounding line flux is clearly defined and available as an output-variable in ISSM. Is the calculation of the grounding line flux differently as the ISSM variable GroundinglineMassFlux? If so, why?*

ISSM does indeed have the ability to offer grounding line flux as an output. As we describe in this section, we use Gardner's method of calculating flux rather than the built-in ISSM module, so to prevent any potential confusion, we have added this justification to the description:

By calculating grounding line flux this way, we are able to easily obtain flux values within each dilated ice shelf mask that we use for analysis of the observed thinning and calving data.

Line 352 *I suggest to drop these "Notes". I found it very speculative and not well supported by the figures and the text. It also took me a while to figure out about which step/jump you are talking.*

We have removed the speculative "Notes" about interpretation of the model data.

References

Line 363 *Not sure about citing an AGU abstract ...*

I think this is okay, according to the *Nature* formatting guide (<https://www.nature.com/nature/authors/formatting-guide>), which states:

Published conference abstracts...may be included in reference lists, but text, grant details and acknowledgements may not.

Line 375 *Drop 'Larour ...'.*

Fixed. [The issue was a duplicate reference entry.]

Line 386 *'J. Glaciol' not consistent with Line 367 where the full Journal name is given (there are more of these inconsistencies)*

Nature's formatting guide does not indicate a preference for full journal names or their commonly accepted abbreviations. We assume that if the paper is accepted, their typesetting team will either format the references to their liking, or provide instructions for how they'd like us to format them at that time.

Referee #3 (Remarks to the Author):

Overview

The study from Greene and others produces an impressive roughly annual record of coastline change across the whole of Antarctica between 1997 and 2021 using a novel assimilation of a variety of datasets. From this record they calculate as a whole Antarctic ice shelves have lost area since 1997. They also calculate the observed iceberg calving rivals basal melting in the overall mass balance of ice shelves. They go further quantifying the importance of ice shelf calving on Antarctic ice flux using numerical modelling.

*I think this is a very interesting study. The significance of this study can be described in two accounts. Firstly it **provides an updated pan-ice sheet coastline change estimate over the past 25 years** and an estimate on the partitioning of calving vrs basal melt in ice shelf mass loss over the same time period. Secondly, and I think **where the main novelty of the study lies, is the quantitative assessment in the importance of iceberg calving** on the recent mass loss of the Antarctic ice sheet. The background here is that we don't really consider calving in most of the models that simulate the future evolution of the Antarctic ice sheet in response to global climate change, but this study shows that actually over the past 25 years it has been relatively important. So **I think this study has the potential to meet the novelty criteria to be published in a journal of this stature. However, I do have a few concerns and comments that I would like to see addressed that I have detailed below.***

Thank you for the kind word and the thoughtful review. We've done our best to respond to your comments below, at times placing your words in bold text, simply to highlight your main points or make clear which points we're focusing on in our response.

Main Comments

*Ice shelf area change: The problem here is that all data is reported in area change. This means that the large ice shelves (e.g. Ross/Filchner-Ronner/Mertz etc) where there is no evidence of any longterm change dominate the signal here with their natural calving cycles. So the actual figure of area change, while I agree is important to report, does not tell the full story. I think the authors need to go further in clarifying this in the manuscript. **I am wondering if the authors could categorize glaciers that are definitely retreating outside any likely natural calving cycles and provide a separate area change and mass loss figure for these glaciers** throughout the manuscript and highlight their locations on an appropriate figure (maybe Figure 1). There is an element of subjectivity about this, but as far as I can see in the Supplementary plots it looks like it the regions that previous studies have highlighted are retreating (Antarctic Peninsula, Thwaites/Pine Island and maybe Totten), pretty much all others are undergoing something similar to natural calving cycle? At the moment the manuscript could be interpreted like there is widespread ice shelf retreat across Antarctica, I don't think this is the case, is it not concentrated in a few set regions?*

It's a wonderful idea to display calving types on a map to highlight regional relationships, and it's what we tried first, but we just couldn't find a way to turn it into a compelling story. The trouble is that ice shelf extent change can be aliased by natural cyclicity, and there's simply not enough data yet to start separating what's natural from what's unnatural.

We see ice shelf retreat across all sections of Antarctica, but the calving-front-retreat signals do not cluster based on region as neatly as ice shelf thinning signals do. The regional distribution of both signals is on display in Extended Data Figure 2, where marker colors are scaled by the central longitude of each ice shelf. What's notable here is that the pink markers of the Amundsen and Bellingshausen Sea embayment ice shelves (Getz, Dotson, George VI, Venable, Crosson, Abbot) all show coherent thinning, but not much calving-front retreat.

To reiterate, the regional coherence of ice shelf thinning has already been reported extensively, but we don't find much regional coherence among calving front advance/retreat signals. As the regional distribution of calving front retreat is not terribly compelling and could easily distract from our main findings, we do not discuss regional distribution directly in our manuscript.

Regarding the clustering of ice shelves into natural and unnatural calving cycles, we wholeheartedly agree that we'd like to understand which ice shelves have tipped out of balance, but we just don't have enough data to comment on whether the years-long or decades-long calving cycles are occurring more frequently than could be deemed "natural." Accordingly, we've been very careful *not* to describe anything as unnatural, and we've been very careful not to let our analysis hinge on any subjective thresholds that would be required for any type of binning.

I also think the assumption that glaciers will calve at their last known location is problematic for some of these very large ice shelves with multi-decadal calving cycles. Really we need much longer records of change to be able to quantify this. I think there needs to be a strong caveat that this may not be the case. For example the Brunt, Cook, and Denman ice shelves all have known variable calving locations. In the most extreme case the Cook Ice Shelf, I think it is now the equivalent of 15 years advanced of its last known calving location.

We appreciate the comment, and when paired with similar feedback from Reviewer 1, it's clear that in this revision we needed to address the confusion. We don't find any statements in the manuscript that could be invalidated by any realistic scenario of future calving, but in this revision, we have included additional language to clarify that exact predictions of future calving cannot be made based on the data we present here. In-depth responses to this point and the new text we've added appears in the detailed comments below and in response to similar comments from Reviewer 1.

We understand the point that Cook, Brunt, and Denman do not calve at consistent locations, and accordingly, we do not specifically analyze or describe the calving cycles of Cook, Brunt, or Denman in this manuscript. We realize they have been mentioned here only as examples of ice shelves whose behavior is not consistently cyclic, but in this manuscript, we only discuss calving cycles of the ice shelves presented in Figure 3, whose approximate calving frequencies have been understood and written about for decades, they are not controversial as far as we know, and our findings do not contradict any previous reports about these ice shelves.

Modelling: I am fan of the quantitative work done to estimate the impact of observed rates of ice shelf thinning and iceberg calving on ice flux across Antarctica because they are based on observations and over a relatively short period of time. However, I am less keen on the future hypothetical experiments. This is because in reality ice shelf thinning and calving work in tandem. It is perhaps ok to separate these processes over short timescale, but ultimately changes in ice shelf thickness are somewhat of a prerequisite to any notable changes in ice shelf area. Essentially major losses of ice shelf area simply are not going to happen without prior changes in ice shelf thickness. So

on these extreme experiments I don't really see the benefit in separating these processes. Its all theoretical and I am not sure on the real world benefit.

We have added a note to the manuscript that thinning and calving often occur in tandem, but we also acknowledge that neither process strictly requires the other:

In nature, ice shelf thinning and calving are not entirely separable, as one can lead to the other, and they are often driven by the same regional influences; yet, we separate the two processes here to identify their relative importance and to better understand such phenomena as thinning in the absence of major calving events, or hydrofracture-driven ice shelf collapse, which does not strictly require prior thinning⁴⁵.

One such case of major calving without any rapid thinning leading up to it recently occurred at Nansen Ice Shelf, where a surface river dumped such a quantity of water into a crack that it caused a significant portion of the ice shelf area to calve (Dow et al. 2018). With such cases in mind, we believe it is entirely possible that we may see more such major calving events in our lifetimes. We may even see total collapse events, such as has been seen at Larsen B and Wilkins. These are the types of calving events we model in our hypothetical calving experiments, and we see no harm in attempting to model the full range of possibilities.

Furthermore, I know it's not the authors intention, but I also think it is important to state in there is no evidence of the likes of the Ronne and Ross Ice shelves collapsing at any point in the near future. Non-specialists reading some of these sections could be left with that impression.

Good point. In response to a related comment that appears in the Minor Comments section below, we have added the following clause:

It should be noted that there is no evidence that the Ross, Ronne, Filchner, or Moscow University ice shelves are under any imminent threat of collapse, but we mention them here only to confirm their previously reported role⁴¹ in holding back a rapid influx of glacial ice into the ocean.

Minor comments

Line 48: *There is a big difference between the overall area change reported in this study (36,701±1465) compared to the study of Baumhoer et al (ref 27; ~22,500 km²), granted the studies cover slightly different time periods. Unless this can be reconciled by the slightly different time periods – this should be commented on.*

From 1997 to 2008, Baumhoer et al. found a loss of 29,618 km² compared to our value of 29,998 km² for the same period. For those dates we each use the same data (RAMP in 1997 and MOA in 2008) and come up with very similar values.

When comparing to Baumhoer's 2018 mapping, we find a disagreement of about 5000 km² (when we interpolate our values to mid 2018 to match the timing of Baumhoer's mapping) but we stress that this disagreement is only present when comparing to Baumhoer's 2018 mapping. We go into detail below about what might cause disagreements, but here's a summary:

1. Seasonal aliasing. Baumhoer's 2018 mapping corresponds to August, when very little calving occurs and the ice sheet has been growing for months, whereas our mapping occurs in a

consistent two week window in March. The seasonal growth and retreat of Antarctica has not been documented, but it could easily come close to 5000 km² over the entire continent.

2. Manual adjustments that are mentioned but not fully described in Baumhoer's paper.

The Baumhoer 2018 coastline is publicly available, and from our visual inspection it appears to be a very well put together data product. We find no problems with it whatsoever, but it is difficult to track down exactly where discrepancies have crept in, in part because as they state in their manuscript: *To minimize errors and mismatches, all three coastlines were manually corrected and adjusted.* The exact logic employed by Baumhoer et al. to manually adjust their data is unclear, and their adjusted 1997 and 2009 coastlines have not been made public.

Without access to Baumhoer et al.'s adjusted data or any clear description of their methods, it does not feel appropriate for us to comment on any discrepancies in a public manuscript, as our comments would be based purely on speculation. However, in this private forum we posit that Baumhoer's automated methods could potentially have delineated the coastline slightly oceanward of the MOA-derived dataset it was compared to, and that any tiny bias sums to a significant difference over the entire continent. It is also possible that Baumhoer's data may include noise that is simply aliased. Our methods attempt to minimize such noise by combining data from multiple sensors, taken over multiple years (accounting for ice flow), with the idea that many noisy signals averaged together tend to produce a clearer signal in which the noise is significantly reduced. We believe our dataset provides the best available estimate of coastal change, but again, without access to Baumhoer's manually adjusted data, we can only speculate on why their 2009 to 2018 difference does not match our estimates over the same period.

Line 52-53: *Is there any examples of some ice shelves advancing? E.g. Cook. Also I presume for some sections of the ice sheet (e.g. East Antarctica) most ice shelves are in the natural advance/retreat category?*

Good catch. We have added Filchner and Cook as examples of ice shelves that steadily advanced over our period of observation.

Line 61-62: *I don't think we really know this for certain. Our problem here is that the calving cycles of these ice shelves operate on mutli-decadal timescales that are much longer than the observational records. Last known location is of course a good start, but in the very few locations where ice front histories have been extended using old aerial photographs/maps this assumption can be quite far off e.g. Brunt, Cook, Denman.*

Figure 3 shows the remarkable accuracy with which the crude measure of ice shelf area "predicted" the calving events of Iceberg D28 from Amery and icebergs B15-18 from Ross West. It also shows the failure of this simple metric to predict the calving of Iceberg A76, which calved "early" from the Ronne Ice Shelf. The figure presents all of these facts openly and honestly, and although we do not predict the exact date of any particular calving event, we do invite the reader to ask themselves, "how far beyond their farthest-observed extents are any of these ice shelves likely to grow?" (If each ice shelf has a 50% chance of growing beyond its maximum observed extent, then by $P=1-0.5^N$, there is a 97% chance that at least one of the five ice shelves presented here will calve by the time it reaches its maximum observed extent.)

We think it's important to present our interpretation of the data as directly as possible, but in response to this comment and similar feedback from Reviewer 1, we have slightly softened the language of the sentence in question. It now reads:

An examination of the timing of major calving events from the Ross, Ronne, Filchner, and Amery ice shelves suggests they could all experience major calving events by the end of the 2030s, which would cause further reductions in area well before the ice sheet has had a chance to recover to its 1997 extent. (Fig. 2 and Fig. 3).

Line 74-75: The Ronne Ice Shelf, while technically a loss over the observational time period, likely would not be a loss if the observational time period was much longer. There is no evidence this ice shelf is changing considerably, this needs to be clarified in the manuscript.

In this paragraph we are simply listing the largest numbers in the tally sheet of our observations, without commentary. Elsewhere in the main text we state very directly:

To be clear, the calving behavior of the Ross and Ronne ice shelves is naturally cyclic on multidecadal timescales, and there is no indication that either ice shelf exhibited any abnormal behavior during our observations.

Line 117: Not sure I would call the changes at the Thwaites tongue 'steady' in my view, the complete loss of a 100 km floating tongue in less than 20 years is anything but.

Good point. We have changed "smaller, but steady" to "smaller, successive". The sentence now reads:

*For the past decade and a half, however, the model response suggests that smaller, **successive** ice shelf area reductions elsewhere around the continent may have cut into more critically important ice, particularly at Pine Island and Thwaites glaciers.*

Line 126-127: strong caveat is needed here that both of these processes work in tandem in reality. Substantial ice shelf loss through calving is unlikely to happen without significant thinning.

We have added the following caveat immediately after the statement in question:

In nature, ice shelf thinning and calving are not entirely separable, as one can lead to the other, and they are often driven by the same regional influences; yet, we separate the two processes here to identify their relative importance and to better understand such phenomena as thinning in the absence of major calving events, or hydrofracture-driven ice shelf collapse, which does not strictly require prior thinning⁴⁵.

Line 133-137: Need to clarify that for some of these glaciers mentioned there is currently absolutely no/v.little evidence of some of the ice shelves mentioned weakening or any prospect of them collapsing in the short – medium term.

Very good point, and much appreciated—We do not wish to present any sort of alarmist view. We have added this new sentence to the end of the paragraph:

It should be noted that there is no evidence that the Ross, Ronne, Filchner, or Moscow University ice shelves are under any imminent threat of collapse, but we mention them here only to reaffirm their

previously reported role⁴¹ in holding back a rapid influx of glacial ice into the ocean.

Line 154: ‘crumbling edges’ – this implies that the coastline of the whole ice sheet is crumbling, this is not the case. Only in some regions.

In this final thought of the paper, we feel it’s okay to step back and put our observations into slightly more poetic terms. Calving occurs everywhere around Antarctica, and even in steady state, it is a process by which the ice sheet crumbles at its edges.

Line 163-166: *I think here a figure of ice shelf area loss is needed that excludes ice shelves that are simply undergoing a natural cycle*

Reiterating a point from above, we don’t think there’s sufficient data yet to separate natural from unnatural calving frequencies in the observations. However, if readers wish to imagine a time series of ice shelf area change without the big outliers like Ronne, Ross, or Filchner, **Figure 2 is already designed to allow such inspection directly.** In response to this comment, we have added this helpful note in the caption to inform readers how to do it:

...note that the relative influence of the largest outliers such as the Ronne, Ross West, and Filchner ice shelves can be effectively removed by ignoring them when visually inspecting this figure.

Figure 3: *Just something for the authors to consider. The manuscript is set up in a way that highlights ice shelf retreat. Could the ice shelves that are actually retreating and contributing to this overall ice shelf retreat signal be highlighted here, rather than those that are simply undergoing their natural calving cycle, which may or may not calve from the same location in the next event?*

We appreciate this note, and it’s something we’ve thought a lot about. The trouble with zooming in too close on any of the smaller ice shelves is that the signal can easily be lost in the noise, and the magnitudes are never anything near what we see at the big ice shelves. It’s only in the pan-Antarctic accumulation of all the small, noisy signals that the big picture becomes clear.

Nonetheless, the effect we want to see here is quite visibly on display in Fig. 2. Simply take your hand and cover up the big ice shelves at the bottom of Fig. 2. Cover up the Ronne, Ross West, Larsen B, Ross East, and Wilkins, and still the curve trends downward with the accumulation of all the smaller area-loss signals above it. The same can be done to visually remove the big outliers (Filchner, Brunt, Amery) on the area-gain side of the equation. No matter how you slice it, the losses always outweigh the gains over the past quarter century.

Extended Figure 2: *What do the colours of the circles represent? The location? I cant really tell from the inset?*

We have added a sentence to the caption that now reads:

Marker color corresponds to the central longitude of each ice shelf.

Supplementary Information

Line 177: *I am satisfied that the coastlines generated are roughly correct for the purpose of this pan-*

ice sheet analysis and I reiterate I think it is a very impressive composite dataset. It is impossible to be spot on with coastline mapping because in some regions it is subjective as the authors state in the case of Thwaites. But I think this caveat should be extended to any ice shelf with a similar disintegrating type front to Thwaites. I think it could also be worthwhile to state that anyone potentially interested in ice shelf extent records for the individual ice shelves presented (which I am sure there will be!) should really double check the records on the imagery as well. There are a few spikes on the composite ice shelf area records that do not look geophysical in nature.

We point to Thwaites as the most significant example of potential errors in the dataset, because it is by far the place we see the most disagreement among other datasets, and it's where we had the most difficulty manually delineating the coastline too. But the reviewer makes a good point here, that we should not imply that Thwaites is the only location where errors could creep into the data. We have added this clause after describing the difficulty of mapping Thwaites:

At Thwaites Glacier and elsewhere, we encourage close inspection of the agreement between our composite coastline and the constituent data in time series in the Supplementary Information. We also recommend comparing our coastline product to available satellite imagery where it is possible.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

This is going to be an embarrassingly short review: in short, the authors have sufficiently addressed all of my previous concerns and I support publication without further delay or edits.

Concern 1 (fixed grounding line positions and thicknesses): the re-framing as 'impact to buttressing' helps me here, as does the addition to the methods section concerning grounding line slope. I have no further concerns here.

Concern 2 (ice shelf growth beyond previous-known extent): the clarifications added are helpful - in particular the additions to the Figure 3 caption. The tempering down of 'predictiveness' is effective. No further concerns.

I support the use of 'rival' in the title, as meaning 'equivalent to'

The additional language in the Figure 2 caption is helpful. I had a 'oh duuuuh' moment in the shower after submitting my original review that what I was seeing was a stacked area plot - not overlapping areas from the zero-axis. The clarification in the new figure caption will save future readers from requiring such shower epiphanies.

Tiny things:

EDFig 2 I assume, like F4, that dot size is shelf area, but worth mentioning here too.

S 458 my understanding is that REMA is a mosaic of imagery from 2012-2016 with somewhat heavier weighting towards 2015/16.. so more like a 4 year 'snapshot'.

S 479 all floating *ice*?

S 527 employed *by* default

This manuscript provides a timely addition on the role of calving in the future evolution of the Antarctic Ice Sheet and should be published without any additional delays.

-KR

Referee #2 (Remarks to the Author):

This is a revised version of the manuscript "Antarctic calving loss rivals ice shelf thinning". The author's rebuttal clearly and sufficiently addresses the comments by the reviewers. I am satisfied with this version. I have a few comments which are easily to solve.

Line 177 maybe: „ influence on ... grounded glacier flow.”

Line 402 "Calveats" -> "Caveats"

Methods – Ice Sheet Model:

I would appreciate if you present a figure showing some ISSM model results. I am thinking of 2 subfigures:

1) Change in grounded ice surface velocity ($v_{\text{experiment}} - v_{\text{init}}$) from one (or more) of your experiments. Just to demonstrate how the model respond.

2) As you invert for viscosity of floating ice (Line 505) you might be able to plot the buttressing parameter as ins Borstad et al. (2013) or Fürst et al. (2016). That would help to understand why the ice shelves respond differently to the forcings (e.g., your statement in Line 140 that the large calving events of Ronne and Ross had little impact on buttressing). The scripts are available in

ISSM (mechanicalpropertis.m and calcbckstress.m).

References:

Borstad, C. P., Rignot, E., Mouginot, J., and Schodlok, M. P.: Creep deformation and buttressing capacity of damaged ice shelves: theory and application to Larsen C ice shelf, *The Cryosphere*, 7, 1931–1947, <https://doi.org/10.5194/tc-7-1931-2013>, 2013

Fürst, J., Durand, G., Gillet-Chaulet, F. et al. The safety band of Antarctic ice shelves. *Nature Clim Change* 6, 479–482 (2016). <https://doi.org/10.1038/nclimate2912>

Referee #3 (Remarks to the Author):

I commend the authors for their very detailed response to all reviewer comments. In my original review I was very impressed by the manuscript and stated that it, in my opinion, it had the potential to be published in *Nature* subject to a few clarifications.

My main concern was that the reader could have assumed that some of the large ice shelves could not re-advance past their last known calving position. I am pleased that the authors have clarified that this was unintended and I am satisfied by the several caveats the authors have added throughout the manuscript. It is a shame that the authors did not feel that they could distinguish between retreating glaciers and those undergoing a natural calving cycle, but I am sure the dataset they have provided will greatly help themselves or someone else do this at some point in the future.

Overall, I am now satisfied that this manuscript meets both the stringent novelty and scientific criteria for publication in *Nature*. Congratulations on a great study.

Author Rebuttals to First Revision:

Comments from Reviewer 1:

This is going to be an embarrassingly short review: in short, the authors have sufficiently addressed all of my previous concerns and I support publication without further delay or edits.

Concern 1 (fixed grounding line positions and thicknesses): the re-framing as 'impact to buttressing' helps me here, as does the addition to the methods section concerning grounding line slope. I have no further concerns here.

Concern 2 (ice shelf growth beyond previous-known extent): the clarifications added are helpful - in particular the additions to the Figure 3 caption. The tempering down of 'predictiveness' is effective. No further concerns.

I support the use of 'rival' in the title, as meaning 'equivalent to'

The additional language in the Figure 2 caption is helpful. I had a 'oh duuuuh' moment in the shower after submitting my original review that what I was seeing was a stacked area plot - not overlapping areas from the zero-axis. The clarification in the new figure caption will save future readers from requiring such shower epiphanies.

Excellent. We're glad we've adequately addressed the main concerns.

Tiny things:

EDFig 2 I assume, like F4, that dot size is shelf area, but worth mentioning here too.

Done.

S 458 my understanding is that REMA is a mosaic of imagery from 2012-2016 with somewhat heavier weighting towards 2015/16.. so more like a 4 year 'snapshot'.

Good point. We've changed the wording to:

...thickness data for ice shelves is largely from the REMA mosaic, which was compiled from data collected over a much shorter timespan.

S 479 all floating *ice*?

Fixed.

S 527 employed *by* default

Fixed.

This manuscript provides a timely addition on the role of calving in the future evolution of the Antarctic Ice Sheet and should be published without any additional delays.

Thanks!

Comments from Reviewer 2:

This is a revised version of the manuscript "Antarctic calving loss rivals ice shelf thinning". The author's rebuttal clearly and sufficiently addresses the comments by the reviewers. I am satisfied with this version. I have a few comments which are easily to solve.

Thank you for taking the time to re-review the paper!

Line 177 maybe: „ influence on ... grounded glacier flow.”

Fixed.

Line 402 “Calveats” -> “Caveats”

We understand that this pun is wholly unnecessary, and one might even argue it risks undermining the seriousness of our results. We don’t disagree with these points, but this case feels rather innocuous, it’s buried deep in the Methods section, the quotation marks around it indicate that it’s self-aware, and it’s a rare bit of fun in scientific literature. Furthermore, in a previous round of reviews, Reviewer 1 explicitly mentioned it as an asset to the paper.

Admittedly, we’re surprised our little pun made it this far in the review process, yet given the differences in opinions among the reviewers about it, we leave this most important decision up to the editor. Regardless of whether the editor prefers to keep it as *Additional “calveats”* or change it to *Additional caveats*, we’ll be happy to see it published either way.

Methods – Ice Sheet Model:

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Fürst, J., Durand, G., Gillet-Chaulet, F. et al. The safety band of Antarctic ice shelves. Nature Clim Change 6, 479–482 (2016). <https://doi.org/10.1038/nclimate2912>

The suggested figures are not bad ideas, but in the interest of convergence we think it’s best to keep the manuscript as similar as possible to the version the other reviewers have seen and approved of.

Comments from Reviewer 3:

I commend the authors for their very detailed response to all reviewer comments. In my original review I was very impressed by the manuscript and stated that it, in my opinion, it had the potential to be published in Nature subject to a few clarifications.

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Overall, I am now satisfied that this manuscript meets both the stringent novelty and scientific criteria for publication in Nature. Congratulations on a great study.

Thank you for the kind words!