

In their review of the first version of this manuscript, reviewer 2 added their comments to the manuscript file. These comments have been added as an attachment in this Peer Review File - please download to view.

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

Decision letter and referee reports: first round

6th Apr 20

Dear Dr. King,

Your manuscript titled "Dynamic ice loss from the Greenland Ice Sheet driven by sustained glacier retreat" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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

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Please review our specific editorial comments and requests regarding your manuscript in the attached "CommsEarth Final revisions information checklist". Please outline your response to each request in the right hand column.

SUBMISSION INFORMATION:

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

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment
On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of the manuscript "Dynamic ice loss from the Greenland Ice Sheet driven by sustained glacier retreat" by Dr. King et al.

This is an interesting paper, which compare three decades of discharge variability and calving front position, and show that glacier retreat account nearly all of the discharge. The results are important to understand long-term ice dynamics. Overall, the paper is very well written and well organized. The figures are all clear, well-described in the text. I suggest publication of this paper after minor changes.

The abstract is clear and short but with no references. I guess no references are allowed by the journal? Otherwise, please add references.

Line 16: "glacier retreat can account for nearly all of the increase in glacier discharge"
Do you mean "discharge due to glacier retreat...?"

Line 34-35: "we perform the first GrIS wide analysis of how these two variables relate on individual, regional, and ice sheet-wide spatial". Before this line, I think it is important to acknowledge previous studies that analyze e.g. speed vs retreat. (Bevan et al. 2012, the cryosphere) or speed vs area change (Vijay et al, 2019 in GRL).

Line 50: is 1985 to 2018 "nearly four decades"? I would say "more than three decades"

Line 122 and 123, and Figure S3:

Concerning the decline in discharge around 2015 in panel C of fig S3, is it solely due to Jakobahavn Isbræ slowdown? No other glaciers in the regions show slowdown?
Perhaps you should add a panel to figure S3 that show Jakobahavn Isbræ only.

Line 122 and 123, and Figure S3:

Well, according to the text Jakobahavn Isbræ is located in the area of Fig. S3c (Central West). When I look at figure S1, it seems like Jakobahavn Isbræ is Fig. S3f (South West).

Line 139: Please add few lines about how uncertainties are estimated. Do you take into account the uncertainties of e.g. bed topography from BedMachine v3? uncertainties of surface elevation etc.?

Line 161-162: "...is close to the -244 Gt 162 yr⁻¹ estimated from GRACE for the same period (update of Wouters et al., 2013). "Adding the updated GRACE time series to figure 1B could be useful.

Line 211: Change in D per km of Fw. Fig S7.

Question: Do you have any explanation why NW behave different? It seems like weighted average front change of up to 2.5 km do not have any significant impact on D. D starts to increase around 2004.

Line 221-223_ "...likely due to several of the actively retreating glaciers doing so on increasingly retrograde slopes...".
Well, CE have many such glaciers and do not show this behavior.

Line 233: after "i.e. Upernavik Isstrøm" add a reference to
Larsen, S. H., S. A. Khan, A. P. Ahlstrøm, C. S. Hvidberg, M. J. Willis, and S. B. Andersen (2016), Increased mass loss and asynchronous behavior of marine-terminating outlet glaciers at Upernavik Isstrøm, NW Greenland, *J. Geophys. Res. Earth Surf.*, 121, 241–256, doi:10.1002/2015JF003507.

Line 248-149: "glaciers thinned by an average of 2% to 4% in all regions except the central west, where a 6% thinning was primarily due to the initial acceleration of Jakobshavn Isbræ in 1998-1999."

It is unclear to me what you mean by "thinning". Do you mean the frontal portion of a glacier, some 5-10 km? or the entire glacier basin drainage area?

Line 448: Add full reference to the IMBIE paper.

Best regards
Shfaqat Abbas Khan

Reviewer #2 (Remarks to the Author): 

Overall this is a really nice paper and I thoroughly enjoyed reading it. It's one of the best papers I've read in some time, especially for a short format journal. It tackles important questions, is well written and has a clear argument. The methods and datasets used are all appropriate and address the questions at hand. I really struggled to find anything to comment on, but have included a couple of minor points in the attached annotated pdf for the authors to consider. Thanks for a really strong and important piece of work: it was a pleasure to review.

Author Response to Reviewers

Dynamic ice loss from the Greenland Ice Sheet driven by sustained glacier retreat

Michalea D. King, Ian M. Howat, Salvatore G. Candela, Myoung J. Noh, Seonsgu Jeong, Brice P. Y. Noël, Michiel R. van den Broeke, Bert Wouters, Adelaide Negrete

We thank the referees for their careful review, thorough comments, and constructive tone. We have used their feedback to make improvements to two figure and integrate more thorough descriptions and context to the discussions and results. We have also adapted the text to reflect editorial requests regarding declarations of novelty, manuscript organization, and incorporation of main results into the introduction. These collective changes have improved the original manuscript.

We have responded to point-by-point comments from Reviewer 1, followed by Reviewer 2, below. Referee comments are listed first, immediately followed by our response in bold. The revised manuscript is included as a separate document.

Reviewer #1 (Remarks to the Author):

Review of the manuscript “Dynamic ice loss from the Greenland Ice Sheet driven by sustained glacier retreat” by Dr. King et al.

This is an interesting paper, which compare three decades of discharge variability and calving front position, and show that glacier retreat account nearly all of the discharge. The results are important to understand long-term ice dynamics. Overall, the paper is very well written and well organized. The figures are all clear, well-described in the text. I suggest publication of this paper after minor changes.

Abstract:

The abstract is clear and short but with no references. I guess no references are allowed by the journal? Otherwise, please add references.

The referee is correct in that references cannot be included in the abstract.

Line 16:

“glacier retreat can account for nearly all of the increase in glacier discharge”

Do you mean “discharge due to glacier retreat...?”

We have amended the text as suggested along with editorial suggestions, and now reads as: “Here we compare over three decades of variability in discharge and calving front position and find that increased glacier discharge was due almost entirely to glacier retreat, with a remarkably consistent speedup of 4-5% per km of retreat across the ice sheet.”

Line 34-35:

“we perform the first GrIS wide analysis of how these two variables relate on individual, regional, and ice sheet-wide spatial”. Before this line, I think it is important to acknowledge previous studies that analyze e.g. speed vs retreat. (Bevan et al. 2012, the cryosphere) or speed vs area change (Vijay et al, 2019 in GRL).

We agree that this addition would improve the introduction. We have added this line to the text as suggested: “Previous work has shown relationships between glacier speed and

retreat (Bevan et al., 2012) or glacier area (Vijay et al., 2019) for smaller subsets of Greenland glaciers.”

Line 50:

is 1985 to 2018 “nearly four decades”? I would say “more than three decades”

Changed as suggested.

Line 122 and 123, and Figure S3:

Concerning the decline in discharge around 2015 in panel C of fig S3, is it solely due to Jakobshavn Isbræ slowdown? No other glaciers in the regions show slowdown?

Perhaps you should add a panel to figure S3 that show Jakobshavn Isbræ only.

The majority of the slowdown (>75%) in the CW was due to Jakobshavn alone. Only two other glaciers showed notably deceleration during these years. We have added more context on this region to the text. Upon publication of this manuscript, individual glacier time series data will be publicly available to reproduction.

Line 122 and 123, and Figure S3:

Well, according to the text Jakobshavn Isbræ is located in the area of Fig. S3c (Central West).

When I look at figure S1, it seems like Jakobshavn Isbræ is Fig. S3f (South West).

Jakobshavn Isbræ does in fact fall within the Central West region. We agree with the reviewer that the delineation in Supplementary Figure 1 makes this unclear, as the region outline covers the referenced glacier. We have adapted this figure to not hide Jakobshavn’s marker.

Line 139:

Please add few lines about how uncertainties are estimated. Do you take into account the uncertainties of e.g. bed topography from BedMachine v3? uncertainties of surface elevation etc.?

Yes, we take into account systematic errors from BedMachine v3 and assume random errors in surface elevation data, estimated at +/- 5 m. We have added the following to the text in the second paragraph of the methods:

“We combine spatially variable errors in bed topography, treated as systematic errors derived from the BedMachine v3 product, with an estimated random error of ± 5 m from surface elevation data when computing total ice thickness errors.”

Line 161-162:

“...is close to the -244 Gt 162 yr⁻¹ estimated from GRACE for the same period (update of Wouters et al., 2013). “Adding the updated GRACE time series to figure 1B could be useful.

Our opinion is that feel adding the monthly GRACE time series to Figure 1b would create too much clutter and reduce the figure’s effectiveness. Therefore, to accommodate this suggestion, we have split Figure 1 into three panels, with cumulative mass balances moved to the third panel, c. We have then added GRACE estimates, smoothed with a 12-month moving average to be more cohesive with the other annual values plotted, to panel c as suggested by the referee.

Line 211: Change in D per km of Fw. Fig S7.

Question: Do you have any explanation why NW behave different? It seems like weighted average front change of up to 2.5 km do not have any significant impact on D.

D starts to increase around 2004.

We attribute some of this lagged sensitivity to the disintegration of floating ice tongues, which have a muted impact on upstream dynamics compared to the loss of grounded ice. This is specifically the case at Kong Oscar Glacier in NW Greenland, one of the largest glaciers by mean discharge in the region and therefore one where front changes are weighted more heavily, which lost its floating ice tongue prior to 2002. We have added more text and a reference to explain the NW behavior to the text (Results, part 4):

“We note that in the NW, the disintegration of floating ice tongues at large glaciers, as observed at Kong Oscar Glacier (Veitch et al., 2012), likely resulted in a delayed dynamic increase of discharge in response to retreat, as shown in Supplementary Figure 7.”

Line 221-223

”...likely due to several of the actively retreating glaciers doing so on increasingly retrograde slopes...”.

Well, CE have many such glaciers and do not show this behavior.

Thank you for the comment. While some CE glaciers may be retreating over retrograde slopes, there is less retreat, on average, in the CE compared to the other major regions (please see unweighted regional retreats in Supplementary Figure 7). Many glaciers in the CE region have relatively stable fronts, whereas recent retreat is more ubiquitous in the NW. We have added “...ubiquitous retreat in the region” to this sentence to further draw highlight the distinction in the NW.

Line 233: after “i.e. Upernavik Isstrøm” add a reference to

Larsen, S. H., S. A. Khan, A. P. Ahlstrøm, C. S. Hvidberg, M. J. Willis, and S. B. Andersen (2016), Increased mass loss and asynchronous behavior of marine-terminating outlet glaciers at Upernavik Isstrøm, NW Greenland, *J. Geophys. Res. Earth Surf.*, 121, 241–256, doi:10.1002/2015JF003507.

We have added the above reference as suggested.

Line 248-249:

“glaciers thinned by an average of 2% to 4% in all regions except the central west, where a 6% thinning was primarily due to the initial acceleration of Jakobshavn Isbræ in 1998-1999.”

It is unclear to me what you mean by “thinning”. Do you mean the frontal portion of a glacier, some 5-10 km? or the entire glacier basin drainage area?

We are describing thinning measured across the flux gate of the glacier (used to derive individual glacier discharge time series). We have added a “across their sampled flux gates” to this statement for clarity.

Line 448:

Add full reference to the IMBIE paper.

Full reference added.

Reviewer #2 (Remarks to the Author):

Overall this is a really nice paper and I thoroughly enjoyed reading it. It's one of the best papers I've read in some time, especially for a short format journal. It tackles important questions, is well written and has a clear argument. The methods and datasets used are all appropriate and address the questions at hand. I really struggled to find anything to comment on, but have included a couple of minor points in the attached annotated pdf for the authors to consider. Thanks for a really strong and important piece of work: it was a pleasure to review.

We thank the referee for their positive feedback and constructive tone throughout their review.

Individual referee comments pulled from annotated PDF:

Abstract, Page 1 Line 20

Really concise, precise abstract.

We thank the referee for the comment. We note that some minor changes have been made to the abstract following editorial suggestions.

Page 2, Line 28

Worth noting that the most recent estimate suggests it was roughly 50/50 SMB versus outlet glaciers for 1992-2018 (Shepherd et al., 2019).

We agree with the reviewer's suggestion and have edited the text to the following:

“Total GrIS mass loss over the 1992-2018 period was due to approximately equal contributions from both terms (Shepherd et al., 2020), but with greater contribution from increased melt runoff after 2000, when mass losses accelerated (van den Broeke et al., 2016; King et al., 2018; Mouginit et al., 2019).”

Page 2, Line 29

At annual, or finer, temporal resolution

Changes applied.

Page 3, Line 46

Were

“are” changed to “were” as suggested.

Page 3, Line 50

Might be useful to state the total timespan of the assessment in the brackets for clarity.

Or you could state the full period here [Page 3, Line 51] – assuming its 1985-2018 from the sentence above, but worth restating explicitly.

We added both references to the time period studied, as suggested.

Page 3, Line 61

Why? Are they not (well) covered by BedMachine?

In this region, bathymetry estimated using BedMachine mass conservation relies on radar surveys far from glacier fronts, and therefore less constrained. We have added the following text here for additional motivation of supplementing BedMachine v3:

We replace BedMachine v3 data with gravity inversion solutions from Millan et al. (2018) at select regions along the southeast margin, *which improved bathymetry constraints for ten fjord systems using multibeam echo sounding data combined with airborne gravity data from Operation IceBridge.*

Page 5, Line 104

Methods are very clearly described and sensible. The datasets used are appropriate and generally accepted within the community.

We thank the referee for their positive comment.

Page 5, Line 107

Not sure what you mean by local here. Local to which region?

Here, the authors meant “local” to describe local in time. We have added “temporal” before this phrase to improve clarity.

Page 5, Line 110

The

Added as suggested.

Page 5 Lines 108-109

I find this [“decreased before accelerating at a slower pace of 2 Gt/yr during 2008-2018”] – decreased before accelerating at a slower pace? I know you’re limited on words, but consider splitting.

We have split the text into two sentences, now reading as “After reaching a temporally local maximum in 2005, annual D then temporarily decreased for three years. Following the temporary decline, discharge accelerated again at a slower pace of 2 Gt yr⁻² during 2008-2018...”

Page 5, Line 112

Interesting that this is the winter component

Yes, we agree that this has interesting implications for how we understand evolving glacier seasonality.

Page 6, Lines 119-123

I find this sentence hard to follow, as there's so much going on. How about e.g.

The central west (CW; Fig. S3c) is dominated by Jakobshavn Isbræ, which accounted for an average of 30% of the regional discharge. Within the CW, discharge steadily increased after the initial retreat and acceleration Jakobshavn in 1999 (Joughin et al., 2004) to a peak in 2014, before declining 10% by 2018 due to Jakobshavn’s slowdown (Khazendar et al., 2019).

We agree that the suggested text is easier to follow and have edited the manuscript accordingly.

Page 6, Line 124

Not sure I like the term 'climbing'. I'd prefer increases or accelerates, as it's specific.

We have replaced the word ‘climbing’ with ‘increases’.

Page 6, Line 128

I'd note in brackets the date you mean by initial values (e.g. 1981)

We have added [1980's mean] to the text to clarify "initial".

Page 7, Figure 1 caption

Really minor point, but why's the caption in grey?

We have changed the captions to be in black following the journal's formatting requirements.

Page 7, Line 145

Why compare to this paper specifically? Does it do a similar thing? Would be worth briefly noting the context for this comparison.

We compare these two studies because they are the only two that show ice sheet wide discharge estimates at sub-annual resolution over a similar time period. We have added some text on this for context.

Page 7, Line 152

Useful comparison, that lends support to the conclusions of the study.

We thank the referee for the comment.

Page 8, Line 175

Presume this is from your data, so please reference your figures. Also worth noting how this compares to the partitioning in Shepherd et al., 2019.

We have added a reference to Figure 1a.

While it is difficult to compare directly with estimates from Shepherd et al., 2019, which began in 1992, we have added the following text for further context:

"Assuming the mean 1985-2000 D of 426 ± 6 Gt yr⁻¹, the mass gain from SMB more than offset the loss due to D , resulting in ice sheet growth, nearly 40% of the years between 1985 and 1999 (Figure 1a), which is similar to estimated mass gain estimated in one half of the years during the 1992-1999 period in Shepherd et al., 2019."

Page 9, Line 182 [Section 3]

Wow this really highlights the bad state of things

Page 9, Figure 2

Might be clearer to have a legend with the colours, rather than label each year on the graph. Or maybe both. I find the labels a bit hard to read.

We agree with the reviewer's suggestion. For clarity, we have added a colorbar to the plot (Figure 2) and have only labelled individual points at 5-year intervals on the plot itself.