nature portfolio

Peer Review File

Holocene thinning in central Greenland controlled by the Northeast Greenland Ice Stream

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Main comments

Overall, this is a well-designed study that tackles an interesting feature of Greenland Ice Sheet history. The findings advance our understanding of the ice stream's response to climate change. The methodology mostly appears sound, and the sensitivity experiments and validation process provide confidence in the results. However, there are some aspects of the study and manuscript that should be improved, which I outline below.

1. Proposed mechanism for the drop in ice surface elevation

First, the hypothesis that is being tested regarding the mechanism of the drop in ice surface elevation at NGRIP needs to be made explicit. Second, the glaciological basis should be more clearly outlined. There are several characteristics of ice sheet change, such as grounding line migration, ice surface elevation change at NGRIP, basal shear stress, ice surface velocity change, but what is the relative order of events? Taking the abstract sentence 'Fast ice-stream flow caused by reduced basal shear stresses following the NEGIS formation' as an example, does fast ice-stream flow not dictate the formation of NEGIS?

2. Sensitivity to ocean forcing

The modelling experiments are heavily focused on atmospheric forcing, principally on air temperature, and there is limited consideration and discussion of ocean forcing. This may be justified in studies such as Briner et al. (2020) where Holocene retreat was mostly across a terrestrial margin, but NEGIS would have had a marine ice margin during its Holocene retreat that would be sensitive to oceanic forcing. An ice stream may respond differently dependent on whether retreat is primarily atmosphere or ocean driven, and while the simulations fit reconstructions fairly well, they could appear correct for the wrong reasons.

An ocean temperature anomaly derived from the TraCE-21ka climate model is applied as a forcing, and the magnitude of this forcing is varied in a sensitivity experiment. However, 1) How realistic is this forcing? Syring et al. (2020, Paleoceanography and Paleoclimatology) state that warm ocean inflow occurred near the mouth of NEGIS at 10-7.5 kyr ago, which corresponds to the timing of reconstructed retreat and ice surface elevation lowering at NGRIP. This study and potential driver should be discussed. 2) Does the model adequately allow for a sensitivity to ocean forcing? The model resolution is 8 km, yet the width of NEGIS trough (where Holocene retreat occurred) is 20 km, meaning just 2.5 model cells are used to capture the retreat of the grounding line through this important, overdeepened trough. Does sensitivity to ocean forcing increase with a finer model resolution?

3. Clarity of manuscript

There are multiple parts of the manuscript where the meaning of the wording is ambiguous, preventing a clear understanding of the motivation, method and interpretation. Several parts of this text are identified below.

Further comments
(unfortunately there are no line numbers to pin-point locations in the text)

Abstract:
- “both reconstructions and models” – Reconstructions from ice cores? Ice sheet models, climate models, or both?
- “The hypothesis…” is unclear – What hypothesis? That NEGIS has been in a similar configuration to today since 8000 years ago?
- “thinning gap” – What thinning gap? The wording needs to more clearly link to the previous sentences.
- “early retreat in northeast Greenland” – here and elsewhere, ‘retreat’ needs more specific wording – retreat of the grounding line in northeast Greenland?

Main
Part 1:
- “Punctual information” – What does this mean?
- “surface elevation drop” – This wording may be misunderstood by a reader, and should be more descriptive as the first time it is mentioned. "Abrupt lowering of the ice surface elevation by X over Y years"?
- “data-model mismatch” – It would be useful to first describe the timing and magnitude of this data-model mismatch.
- “meltdown” – I suggest more common wording (e.g. retreat, demise).
- “Northeast Greenland Ice Stream (NEGIS)” – It would be useful to include a map with the ice stream identified, perhaps with the locations of various reconstructions that are used in other figures.
- “this aspect” – What aspect?
- “inversion techniques are not valid…” – Explain why they are not valid, and how that is relevant to this study/your approach.
- “Here we explore…” – This sentence could include “(see Methods for details)”, or similar, at the end.
- “inland penetration of the NEGIS flow upon margin recession” – Unclear.

Part 2:
- “the large ice cap” – What ice cap? Are you referring to the Greenland Ice Sheet in the northeast region of the continental shelf?
- “agreement with reconstructions…” – What aspect is in agreement with what reconstructions? The cited 2011 review paper shows that there are few constraints (at the time of publication) in this region.
- “high Arctic temperatures” – needs to be more specific (e.g. air temperature, annual air temperature).
- “trigger and drive” – What is meant by "trigger" vs "drive"?
- “ice sheet continental border” – What does this mean? The present-day ice sheet margin/coastline?
- “the NEGIS margin” – Needs to be more specific. The grounding line or the ice (shelf) margin?
- “Such retreat history is congruent with…” – refer to the relevant figures where the simulations are compared to the data.
- “ice stream penetrates the interior” – Unclear and confusing wording. Describe the glaciological characteristics.
- “tail of the stream” - What does this mean? The lower reaches of the ice stream? The terminus?
- “In our simulations,…” - Why does the ice surface elevation increase prior to 9 kyr ago when air temp increases and retreat occurs? Why is there an offset between the timing of reconstructed and modelled ice surface elevation change?

Part 3:

- “potential relationship between…” - What is the argument here? That basal sliding state explains the NGRIP elevation change?
- “This implies that a faster-sliding and…” - What is the chicken, what is the egg? The wording suggests that basal sliding affects the NGRIP elevation, but this relationship needs a clearer glaciological explanation. What if these characteristics are both effects of retreat, and basal sliding changes are an effect of dynamic thinning?

Part 4:
- This is clear at the NEGIS margin,…” - Is the air temperature shown in Fig.4b just of the NEGIS margin?

Part 5:
- “Our late-Holocene results are consistent with…” - What aspect is consistent?
- “NEGIS continental intrusion” - Unclear.

Figure 1:
- It should be made clear at the start of the caption that the modelled data in blue is from this study.
- Wording of “NEGIS ocean border” and “present margin was ice-free” is not clear.
- “Annual temperature” - specify that this is air temperature.

Figure 2:
- Try for a colorblind friendly sequential color scheme, rather than divergent scheme.
- The NEGIS basin should be identified in this figure, if not elsewhere.
- “best run” - refer to Figure 3.

Figure 3:
- “coloured following their Holocene maximum thinning” - The total amount of thinning across the Holocene at NGRIP?

Figure 4:
- How was the dashed red curve determined? I assume that this curve is supposed to be the same as that in Fig. 2a in Larsen et al. 2018, but it does not seem to be exactly the same (e.g. differences in the variations in the slope of the curve, and retreat in last ~500 years).

Methods:
- “the upper mantle is uniformly set to relax in 3000 yr” - Why? Is this over 3000 years from the perturbation, by 3 kyr ago, or over the last 3 kyr?
- “RMSE of simulated to reconstructed grounding line movement…” - Is this done using the approximated grounding line position time series from Larsen et al., or against the specific data points for different locations?

Data availability:
- “All Yelmo simulations presented here are available upon request” - The authors should provide the code used to run the experiments, and at least the best fit simulation.

Reviewer #2 (Remarks to the Author):

The paper investigates a possible explanation for the significant ice sheet elevation drop (~200m) reconstructed during the last deglaciation and through much of the Holocene, from analyses of the

NGRIP ice core. It undertakes an ensemble numerical modelling exercise where they assess the dynamics of the Northeast Greenland Ice Stream (NEGIS), and the role it can play in the drawdown of elevation from the ice divide. The model is forced with high temporal resolution (not defined) climate data, has reduced basal friction and high geothermal heat flux and provides a good representation of the velocity pattern of NEGIS. As the ice sheet margin migrates landward during the deglaciation so the onset zone of the NEGIS migrates farther back towards the ice divide and thus explains about 50% of the ice-core derived elevation change at NGRIP. Overall the findings of the paper support the general consensus of knowledge at the present time.

1 – It is unclear to me that this inference can be so straightforwardly made. During the deglaciation NEGIS evolved from a shelf-based terminal zone some 300 km in length into a more topographically constrained terminal zone (esp 79°N and Zacharie Isstrøm). If in the future the margin steps back into the "basin" area then it is perhaps less clear how the dynamics of the ice stream will evolve. It seems that the boundary conditions may be very different. In addition, Fig 2 e, f, g show the ice margin inboard of the present day and the drawdown of ice at NGRIP continues until ~4 ka (and longer in the ice core reconstruction). There is no modelled recovery in ice elevation presumably associated with the ice margin advance as seen in the ice core reconstruction. So how well the model is capturing the detail of the drawdown is less clear than being able to explain at least half of the total drawdown. Additionally thinking of a forward look is where of course where the numerical modelling will be invaluable. However, without such modelling I think this inference is ill-founded.

2 – are these numbers correct. It appears from Fig 2e that the ice margin at 8 ka is further inboard than the current grounding line location and retreat continues until at least 6 ka. The "further recession" seems to be rather more than ~100 ±20 m, which I would have thought to be rather below the model spatial resolution? Indeed, there is no information provided on the spatial resolution at which the model has been run?

3 – it is unclear why this description is used? The surface velocities depicted in Fig 2 A-D appears to show two well-defined ice streams linked to the broad NEGIS catchment draining thought Westwind and Norske troughs? This fits well with mapping of the sea floor which shows evidence of glacial lineations (e.g. Arndt et al, 2017 http://dx.doi.org/10.1016/j.quascirev.2017.01.018; Olsen et al, 2020 - doi.org/10.5194/tc-14-4475-2020) which in such settings is often liked to ice stream. The fast ice flow seems well structured and topographically controlled by the troughs crossing the continental shelf?

4 - The model fails to capture the duration and rate of NGRIP drawdown (see also point 1) and some comment on this is warranted. In light of this, the reduction in modelled surface velocities is likely to be a function of the "steady" NEGIS. The longer and lower rate drawdown reconstructed from NGRIP does not at first sight square with the modelled surface velocity reduction. Some comment on this is warranted.

5 – The grounding line location of NEGIS characterised by the record of Larsen et al, 2018. It appears that in this paper the grounding line in the model (easily identified) is assumed equivalent to the ice margin location defined by Larsen et al, 2018. However I do not think they are the same thing. My understanding of the Larsen reconstruction is that the distance is measured from the present ice margin i.e. the edge of the ice shelf, and not the present grounding line. From the caption for his Fig 2b – "^{10}Be ages with external uncertainties from outer coast (dark gray) and outside the Little Ice Age moraine (black), and ^{14}C dates from raised marine deposits presently dammed by NEGIS up to 70 km upstream the present ice margin at NG". I am also not clear how the Bennike and Weidick (2001) data have been interpreted in this paper i.e. what "Radiocarbon dates at the NEGIS ocean border" means. I

assume these ages relate to the samples collected around the epi shelf lake Blåsø. These samples are taken proximal, +10 km beyond, the present grounding line. The samples include marine mammal bones (seal and whale) and shells and driftwood. The bones and shells are assumed to indicate that Blåsø is no longer a lake but is fully connected to the fjord with no ice shelf but possibly permanent/semi-permanent sea ice in the fjord. The driftwood indicates that there was no ice shelf at Blåsø and the fjord was of sea ice free in summer. Bennike and Weidick (2001) provide no estimate for a grounding line location just saying that "the open fjord may have extended as far west as the present grounding line, just south of the western end of Blåsø." My reading of Figs 1, 2, 4 and 5S and the text indicates that this paper has assessed the modelled grounding line distance measured from the present location of the GL at 79°N. The validation process has calculated using the goodness of fit based on the "RMSE of simulated to reconstructed grounding line movement at 79°N (79_GL, data from Larsen et al., 2018)". Larsen provides the location of the ice margin (actually the ice shelf margin) not the grounding line. If I am correct then the scoring matrix needs to be recalculated to and the results reassessed. If I am incorrect then the Figures need to be amended and how the ice margin record of Larsen et al, 2018 can be correlated to the grounding line of the model output.

6 – given the potential importance of ice shelves to the flow of their up-stream ice streams, it is surprising that there is no mention in the model, nor in the modelling of an ice shelf? Does the model include an ice shelf? It does not seem so from the surface velocity show in Fig 2, or is the velocity only for grounded ice shown? Some explanation of how the model deals with ice shelves is warranted, what exactly is being shown in Fig 2 and the potential impact of ice shelf loss, for example their loss e.g. Zacharie Isstrom, on the flow of the NEGIS is needed.

Reviewer #3 (Remarks to the Author):

Dear authors,

Thank you for the opportunity to review this manuscript. In general, I found the manuscript enjoyable to read, the results exciting and novel, and the conclusions well-supported. This manuscript will be of interest to many in the glaciology and paleoclimatology communities. The results have important implications for projecting future sea-level rise and highlight an area of ice-sheet model development that has been elusive for decades (representing the flow of large ice-streams and in particular the NEGIS) but is clearly critical for both hindcasting and forecasting ice-sheet dynamic responses to a changing climate. This is an excellent manuscript and with attention to a few key adjustments outlined below, I think this manuscript is very suitable for publication in Nature Communications.

I applaud the authors for their clever experimental design and novel approach to diagnosing the role of ice-stream dynamics in driving thinning in the ice-sheet interior. In my line edits, I have flagged where I think a statement is too strong or slightly over-interpreted, but in general the conclusions drawn here are well supported by a clear experimental design and a careful analysis.

I have one overarching question about the framing in this paper – in some places (like the title) there is a geographic restriction to the NEGIS influence implied (thinning in central Greenland) but in other places (the abstract and the conclusion) the influence of fast ice-stream flow for the full ice sheet is gestured at. I think the authors could refine the framing of this article to be focused just on the NEGIS influence for this catchment alone (or north-central Greenland, or demonstrate exactly where for the ice sheet the NEGIS has a dominant dynamic effect on thinning through the Holocene). After all, just the NEGIS catchment amounts to something like 12% of the entire ice-sheet volume or 1.1 meters so it's a significant portion of the whole Greenland ice mass and thus there is a clear case that we should care about how it behaves dynamically (e.g. Mouginot et al. 2015). Alternatively, if the authors want to maintain the ice-sheet wide perspective, I think they need to show that NEGIS impacts more than just the NGRIP site during the Holocene. Perhaps a map of where in Greenland the ensemble shows divergent surface elevation histories versus where the ensemble members are in greater agreement about the elevation history would demonstrate this nicely, and allow the authors to better support statements such as "Our findings hint at a wider and more dramatic influence of the fast ice-stream flow on the whole ice-sheet behavior than previously expected" and "Such a strong dynamics-induced destabilization of the ice divide warns us about a possible extreme response of the ice-sheet interior to future warming ice loss."

In summary, this is a strong and clear manuscript that advances the field and I hope to see this exciting work published soon with a few slight modifications.

REFERENCES

Mouginot, J., Rignot, E., Scheuchl, B., Fenty, I., Khazendar, A., Morlighem, M., Buzzi, A., and Paden, J., 2015, Fast retreat of Zachariæ Isstrøm, northeast Greenland: Science, v. 350, p. 1357–1361, doi:10.1126/science.aac7111.

LINE EDITS/QUESTIONS

Please note – the PDFs I received did not have line numbers so the page numbers below refer to the page of the PDF.

Main text.

Page 1.

ABSTRACT. "Our findings hint at a wider and more dramatic influence of the fast ice-stream flow on the whole ice-sheet behavior than previously expected." I do not think you have shown fast ice-stream flow impacts the whole-ice sheet behavior, but you have very clearly and directly demonstrated it for the northeast catchment, from the oceanic margin all the way to the ice divide. To me this is impact enough, but if you want to keep your statement as-is then I would like to see a clear demonstration that the impacts of NEGIS reach the whole ice-sheet.

MAIN
"Punctual" – I'm not sure what you mean by this – could you use another word? Punctual sounds like it could mean abrupt, discontinuous, on-time? Do you mean well-dated, or something else?

"~300 km long" check this length for NEGIS? I thought it was closer to 700km.

Page 2. GHF – should this be geothermal heat flow or geothermal heat flux?

"Penetration" – suggest a different word. Propagation?

"Such a strong dynamics-induced destabilization of the ice divide warns us about a possible extreme response of the ice-sheet interior to future warming ice loss." I think this is too strongly worded and I don't think "destabilization" is the right word either. What about "Such a far-inland reach of dynamic thinning in northeast Greenland during a major retreat phase suggests that such processes may also amplify the response of this sector to future warming and ice loss."

“Several tens of meters” based on Figure 1 I think you mean kilometers?

Page 3 “Penetrating” again – could be replaced with “more inland-propagating” or something else? Or is there a reason why you prefer the term “penetrating” here – I may be missing something but to me, NEGIS is not “penetrating” the ice sheet but instead the dynamic controls on the ice-sheet position are allowing the fast-flow to propagate farther and farther upstream. Note – later on page 5 you use “continental intrusion” – I think if you pick one phrase that you think best captures the phenomenon you are talking about it, and use it throughout, that would be helpful for the reader.
“indicating that any other ice stream across the ice sheet has no significant involvement in the NGRIP thinning.” – reword: indicating that no other ice stream across the ice sheet has significant involvement in the NGRIP thinning. What does “other ice stream” refer to here – may be confusing to some readers who are not familiar with the difference between glacier versus ice stream and whether some ice flow features we identify as “glaciers” today may be more aptly considering “ice streams” under paleo flow conditions. What if you reworded this to instead say “indicating that there is no catchment except for NEGIS where ice flow has significant involvement in the NGRIP thinning” or something similar? Also, can you really say this? If you did the experiment to reduce the bedrock by one sigma beneath all of the major flow features in Greenland, would none of them contribute to the NGRIP thinning? Maybe just say “No similar correlation is found for any other GrIS basins in our experiments (Fig. 2S).”
Sentence “although even higher GHF values…” does not make sense to me. Do you mean “ Although even higher GHF values have been suggested in northeast Greenland as attributed to the passage of the Icelandic hotspot (Rogozhina et al., 2016), a GHF of just 57 allows our model…” ?
Check units of GHF – should be mW per m2 ?
Spell “MacGregor”
The paragraph starting “Runs with a very fast ice stream…” Could you add one or two sentences describing any secondary controls on the NGRIP thinning rates from your ensemble, and comment briefly on what that means in terms of the influence of ice-stream dynamics on the thinning? For example in Fig 3S – z0 plays a less important but still influential role on inland thinning rates.
Page 4
“Here, bedrock uplift is due to the sole GrIS mass loss” reword: “Here, bedrock uplift is solely due to GrIS mass loss.”
“Yet, the GIA signal from the Greenland peripheral ice sheets deglaciation” – what ice sheets are you referring to here – you mention the Innuitian, is that what you mean? Why not just say that – “Greenland peripheral ice sheets” is confusing.
“crucial for a correct retreat” is it possible to quantify this following your method – e.g. the median skill score for the ensemble using each of the forcings? This would provide some quantitative foundation for what you mean by “correct retreat”
Page 5.
The final sentence is really important and I suggest some slight changes to wording to really drive your main point home. Your logic here makes sense to me but it’s a very strong statement with big implications so I think it could be important to guide readers through your thought process here.
I don’t really understand what you mean by “destabilization.” I don’t think you have shown that the Holocene warming destabilized northeast Greenland – what you have shown is that dynamic thinning in central Greenland is largely attributable to the development of the proto-NEGIS during the early/mid Holocene. But couldn’t this still be the “steady-state” response of the ice sheet to the transient climate forcing? To me “destabilize” implies a runaway or positive-feedback process which I don’t think you have shown here. I would avoid the word destabilize and try to stick closer to the physical processes that you have studied in this final paragraph. I suggest something like: Our results imply that a future northeast margin retreat stemming from increasing temperatures might similarly provoke tens of meters of dynamic thinning in central Greenland. Given that the present-day NEGIS

velocity pattern is not captured by most ice-sheet models (Goelzer et al., 2018), our results suggest that inland dynamic thinning [in north-central Greenland] is underestimated by future projections and Greenland's contribution to future sea level rise may be greater than currently anticipated.

FIGURES

Figure 1(b), Figure 4

How is "distance from present position at 79ºN" calculated? Along the central flowline for the measured velocity? Consider using "79ºN Glacier" everywhere you refer to this (e.g. Figure 4 caption) so that it is easily differentiated from "79ºN" the geographic line of latitude – for example in Figure 1 caption (e) where is this climate reconstruction from – the outlet of 79ºN glacier?

Figure 2

Please label the colorbar. Consider adding 50 m/a contour from observed velocities on top of the "present day" modelled velocity field – I think that would emphasize that your model does a great job at reproducing NEGIS.

Consider adding to at least on panel the location of Agassiz ice cap as this is referred to throughout the text.

Figure 3

Delete "for valid runs" – modify sentence to read something like: " Thinning rate and shear stress are averaged between the time of Holocene maximum elevation and the present; coloured points follow the colour palette of panel (a); runs that do not meet scoring criteria are shown as grey dots; correlation coefficient and p-value for scored runs are also reported. The best run of the ensemble is shown as a blue triangle."

Figure 4

Why is the spread for the yellow ensemble so much greater than for the blue ensemble? Also these ensembles seem to be diverging already by 12ka but it is difficult to see what the reconstructions are doing at 12ka in the inset. Is it possible to show more of the reconstructions (maybe just to 14ka so it matches the ensemble plot)? Also consider labelling the gray line in (b) directly on the plot as "Agassiz ice cap temperature" or similar – because this does not correspond to plot (a) I found it confusing but I could see from the text why you wanted to include it.

METHODS

Page 10. "It is only one (with another)" what does this mean? Consider "It is one of only two scenarios…" ?

Page 11.

Can you add a figure to the supplement showing how the bed elevation was changed (minus one sigma) within the NEGIS boundaries?

I think this sentence should read: "absolute modelled-observed modern surface elevation DIFFERENCE at NGRIP" ?

How robust are your estimates of the % of thinning attributable to NEGIS propagating inland depending on which of these misfits you use? Is the best-fit scenario the best fit across all of the different misfits, or does one or a few of these misfits dominate the results? Can you say something about why these parameters were chosen to calculate the misfit?

How did you choose the criteria for runs to discard as NGRIP_PD<40m ? Assuming the above edit is right, maybe also just drive it home: "…runs are considered valid as long as the absolute elevation difference between modelled and observed elevations is less than 40 meters (e.g., NGRIP_PD<40m), so that 122…"

Page 12

Fix: Reference climate FROM Box

Page 13 – Karlsson reference should come before Le Meur reference I think?

SUPPLEMENT

Figure S5 – Can you add the blue triangle to each of these panels for your best simulation?

We thank the three anonymous reviewers for the valuable comments and suggestions for our manuscript. Revising the paper has required a large effort, specifically due to an improvement in the methodology that required additional model calibration and tuning. We believe the new set of experiments is improved in many aspects and the results are more robust. Yet it confirms the physical outcomes pointed out in the previous version of our manuscript. We hope that the new manuscript (attached) will be well received by the reviewers.

We agree that some critical points needed to be clarified and others improved. We took advantage of these criticisms to redo all experiments with a newer version of the ice-sheet model Yelmo (v1.801, available on Github under tagged version "ngrip_thinning"), which features several improvements such as:

1. New calving law based on von Mises stress (Lipscomb et al., 2019), which is more suitable for the narrow, elongated Greenland outlet glaciers (Choi et al., 2018);
2. New treatment of ice-free and partially ice-free points (temperature, viscosity and strain rate are extrapolated to those points) to improve stability;
3. Improved viscosity routine in the DIVA solver;
4. Several minor bugs are fixed.

In this revised manuscript we also include major improvements in the methodology:

1. Improvement in the oceanic forcing. As suggested by Reviewer #2, we improved the ocean temperature anomaly in the northeast, making it closer to recent reconstructions from marine sediment cores in the Arctic. The temperature signal now mimics the continuous advection of subsurface warmer Atlantic Water (AW) into the shelf from early to mid Holocene, following the well-constrained evidence from sediment core analysis (Lloyd et al., 2023, Hansen et al., 2022, Davies et al., 2022, Syring et al., 2020). Thus, ocean temperature now increases between 13300 and 10000 years ago to represent the minimum period of presence of AW found in both Westwind Trough and Norske Trough. Then it declines slowly until 7500 years ago. A subsequent cooldown of subsurface waters due to the reduced intrusion of AW in the shelf (Lloyd et al., 2023, Davies et al., 2022, Hansen et al., 2022) is then represented by a constant low temperature anomaly until almost the end of the Holocene (the oceanic temperature anomaly recovers to present values during the last 100 years, see Fig. S11).

2. Improved configuration of the paleo ice stream based on recent findings. We characterise the past northeast fast flow regime as a system of a northern, now-extinct paleo ice stream branch and a southern present-day-like ice stream activated during the Holocene, following Franke et al., 2022. There is large uncertainty around the timing of activation/deactivation of the ice-stream system in the northeast and the processes that might have caused it. Yet, significant restructuring of ice streams' shape and strength is more likely during the initial phase of a deglaciation due to the large ice-sheet geometry reorganisations upon margin retreat. To reproduce such change in configuration during the deglaciation, we start

to deactivate the paleo NEGIS-like ice stream and activate the present-NEGIS at 8 kyr ago by changing the local basal friction. This is also supported by the recent hypothesis of the NEGIS showing a similar configuration to today since 8000 years ago (Gerber et al., 2021).

3. Inclusion of additional perturbation parameters to better cover the ensemble phase space. Seven other perturbation parameters have been added to the simulation ensemble: 1. the ice-ocean heat flux parameter (i.e. κ of Eq. 7), to test the ice-sheet sensitivity to the ocean forcing during the deglaciation; 2. the enhancement factor Ef to explore different dynamic states of the ice sheet due to ice anisotropy; 3. the lithosphere relaxation time τ and 4. the effective elastic thickness of the lithosphere to investigate the role of the isostatic adjustment; 5. the calving rate parameter k_t of the von-Mises-like calving law (Lipscomb et al., 2019); 6. the scaling factor f_p for past temperature anomalies in the precipitation-temperature relationship (similar to β from eq. 8 of Badgeley et al., 2020) and 7. the additional scaling factor f_{min} to be applied to the NEGIS basal friction parameter f_{mid} during the last part of the Holocene to reproduce the switch from the paleo-NEGIS to the present-NEGIS (see Figure 1). The large ensemble now includes a total of 3000 simulations.

4. Inclusion of additional data used to evaluate the model: the ensemble is now evaluated against a total of thirteen individual scores based on paleo and present model-data misfits. In addition to previous misfits, we add now: RMSE of modelled to observed ice-covered area at the LGM (AREA_LGM, data from Lecavalier et al., 2014), and RMSE of modelled-observed deglaciation timings computed and averaged 1. at four sediment core locations from the northeast continental shelf (SEDIM_DEG, data from Hansen et al., 2022, Lloyd et al., 2023, Davies et al., 2022, Syring et al., 2020), 2. at three locations in the outer shelf as calculated from 10Be exposures dates from morain/boulder samples (OUT_MORAIN_DEG, data from Larsen et al., 2018) and 3. at other six locations closer to the present NEGIS margin (INN_MORAIN_DEG, data from Larsen et al., 2018). We also introduce a last binary control on the upstream retreat of 79N glacier: simulations that retreated more than 70 km upstream will present a maximum error and vice versa (see Methods for more details).

Specific reviewers' questions/comments have been answered point by point below.

REVIEWER COMMENTS

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Reviewer #1 (Remarks to the Author):

Main comments

Overall, this is a well-designed study that tackles an interesting feature of Greenland Ice Sheet history. The findings advance our understanding of the ice stream's response to climate change. The methodology mostly appears sound, and the sensitivity experiments and validation process provide confidence in the results. However, there are some aspects of the study and manuscript that should be improved, which I outline below.

1. Proposed mechanism for the drop in ice surface elevation

First, the hypothesis that is being tested regarding the mechanism of the drop in ice surface elevation at NGRIP needs to be made explicit. Second, the glaciological basis should be more clearly outlined. There are several characteristics of ice sheet change, such as grounding line migration, ice surface elevation change at NGRIP, basal shear stress, ice surface velocity change, but what is the relative order of events? Taking the abstract sentence 'Fast ice-stream flow caused by reduced basal shear stresses following the NEGIS formation' as an example, does fast ice-stream flow not dictate the formation of NEGIS?

We agree that the sentence pointed out in the abstract was unclear. Now it has been changed to:

"*Fast ice-stream flow caused by reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning, showing that ice-stream dynamics is one of the main drivers of the NGRIP Holocene surface elevation drop.*".

We also agree that a clear glaciological explanation for the surface elevation drop at NGRIP was missing. To clarify this, part of the Results section has been modified as following:

"*The fast inland retreat simulated at the early Holocene is accompanied by the formation of a paleo-NEGIS draining the northeast and stretching from the northern flank of present-day NEGIS outlet glaciers into the ice-sheet interior (Fig. 2). […] Ice flow propagates upstream in response to thermo-mechanical and geometrical changes occurring during the northeast retreat of the marine-based ice sheet. At the onset of the deglaciation, ice-mass loss at the shelf edge due to rising air and oceanic temperatures weakens buttressing, accelerating inland ice flow. This results in glacier thinning, ice flow speed-up and retreat of the grounding line, which accelerates further the ice flow, promoting ice front recession. Although ice thinning at the margin reduces basal friction (as effective pressure decreases), sliding significantly increases when a local rise in subglacial melt occurs due to increased frictional heating. As the retreat persists, regions showing decreased basal friction conditions allow for flow acceleration farther upstream towards the ice-sheet dome (Fig. 3). Despite the retreat of the northeast sector and rising air temperatures at the early Holocene, we simulate a surface elevation increase in north-central Greenland from 15 to 9.4 kyr ago, mostly due to increased precipitation (Vinther et al., 2009) (Fig. 3f). Then, velocity propagation associated* *with the inland expansion of the paleo-NEGIS induces dynamic thinning of ice at the NGRIP site. The northern summit ice is advected downstream causing the surface elevation to decrease abruptly (Fig. 3a) until the end of the Holocene Thermal Maximum (HTM) (~5 kyr ago). For the last 5 kyr the northeastern margin slowly regrows towards its present position, reducing the increase in NGRIP surface ice velocities and surface elevation dropdown. Yet, the decrease in basal sliding imposed at the present-NEGIS (Fig. 1) ensures continuous drainage of the GrIS northern sector until the present (Fig. 2). The hypothesis of the presence of a large, active ice stream resembling that of today since the mid-Holocene is shared with a recent study of past ice-flow trajectories connecting the EGRIP ice back to its source location (Gerber et al., 2021).*

Moreover, we explain the dynamic effect of the retreat on the ice-sheet interior further in the Discussion section:

"Margin thinning and retreat, driven by early Holocene temperature rises, induces ice thinning at higher elevations, reducing effective pressure, increasing basal sliding, and fostering ice acceleration and further margin retreat. Primarily, dynamic thinning triggers reduced basal friction during the retreat. Still, basal sliding at the stream increases considerably only with a significant basal water increase from enhanced basal frictional heating (Fig. S12). Reduced effective pressure and basal friction are associated with increased basal velocity at the stream region, allowing upstream paleo-NEGIS propagation. The NGRIP dynamic thinning (and surface elevation drop) is therefore the indirect response to margin perturbations via geometry and subglacial water system alterations. In fact, the NGRIP thickness decrease occurs following the decrease in basal friction at EGRIP, which in turn follows the decrease in basal drag averaged at the NEGIS basin due to margin retreat (Fig. S10). It has been shown that, over time spans ranging from centuries to millennia, variations in surface elevation trigger a corresponding reaction in the basal water drainage system (and vice versa) (Karlsson et al., 2015). As a result, adjustments in thickness and velocity can extend several hundred kilometres inland through wave propagation (Williams et al., 2012). Long-term ice-flow speed up can be propagated at almost the highest elevations in the ice sheet as a result of mass loss at the terminus (Wang et al., 2012). This has been observed at ZI recently, where the margin retreat of ~10 km has induced dynamic thinning along the NEGIS more than 200 km upstream (Khan et al., 2022), as well as in some glaciers in west Greenland (Williams et al., 2021, Felikson et al, 2021). Moreover, localised perturbations at the Antarctic ice-shelf margin have been shown to propagate even more than 900 km upstream along the ice stream (Reese et al., 2018). These findings align with our hypothesis that ice acceleration and margin retreat in northeastern Greenland could have induced dynamic thinning more than 600 km from the ice front on centennial to millennial timescales."

2. Sensitivity to ocean forcing

The modelling experiments are heavily focused on atmospheric forcing, principally on air temperature, and there is limited consideration and discussion of ocean forcing. This may be justified in studies such as Briner et al. (2020) where Holocene retreat was mostly across a terrestrial margin, but NEGIS would have had a marine ice margin during its Holocene retreat that would be sensitive to oceanic forcing. An ice stream may respond differently

dependent on whether retreat is primarily atmosphere or ocean driven, and while the simulations fit reconstructions fairly well, they could appear correct for the wrong reasons.
An ocean temperature anomaly derived from the TraCE-21ka climate model is applied as a forcing, and the magnitude of this forcing is varied in a sensitivity experiment. However, 1) How realistic is this forcing? Syring et al. (2020, Paleoceanography and Paleoclimatology) state that warm ocean inflow occurred near the mouth of NEGIS at 10-7.5 kyr ago, which corresponds to the timing of reconstructed retreat and ice surface elevation lowering at NGRIP. This study and potential driver should be discussed. 2) Does the model adequately allow for a sensitivity to ocean forcing? The model resolution is 8 km, yet the width of NEGIS trough (where Holocene retreat occurred) is 20 km, meaning just 2.5 model cells are used to capture the retreat of the grounding line through this important, overdeepened trough. Does sensitivity to ocean forcing increase with a finer model resolution?

We follow the Reviewer's suggestion and we use this rather well-constrained evidence from sediment core analysis to build a homogeneous temperature signal for the whole Northeast marine basin (from the inner to the outer shelf). Deglaciation timings inferred from sediment cores in Northeast Greenland are quite heterogeneous, as nearby locations present asynchronous retreat. Yet, analysis from sediment cores concerning the presence of Atlantic water in the Northeast continental shelf during the Holocene are well in agreement (Lloyd et al., 2023 and reference therein). Presence of Atlantic source water is found already at ~13 ka BP both in the outer shelf (Westwind Trough, piston 39G, Hansen et al., 2022) and middle shelf (Norske Trough, piston. 92G, Davies et al., 2022). This also compares well with new findings suggesting the presence of RAW (Return Atlantic Water) in the inner shelf already at 10.9 ka BP (sediment core PS100/198, Lloyd et al., 2023). Continuous advection of subsurface Atlantic warmer waters is then found in several locations until ~7,5 ka BP, followed by a cooldown of subsurface waters toward the rest of the mid-late Holocene due to the reduced intrusion of Atlantic Waters in the shelf (Lloyd et al., 2023, Davies et al., 2022, Hansen et al., 2022, Syring et al., 2022). The signal we employ shows increased subsurface oceanic temperatures with respect to the glacial condition between 13.3 ka BP (minimum age of presence of AW in the outer shelf (Hansen et al., 2022) and middle shelf (Davies et al., 2022)) and 10 ka BP, linearly decreasing temperatures between 10 ka BP and 7.5 ka BP, and constant low temperatures toward almost the end of the Holocene (the oceanic temperature anomaly recovers to present values during the last 100 years, see Fig. S11). Maximum and minimum anomaly temperatures during the Holocene are defined as +2°C (early Holocene) and -1°C (mid-late Holocene) with respect to the present temperature. An increase of ~3°C from the late glacial to the early Holocene is also supported by records of subsurface ocean temperature in Farm Strait (Werner et al., 2016). Compared to the TraCE21-ka signal used in the first version of the manuscript, our signal warms earlier and cools more abruptly during the mid Holocene (see Figure S11). Unfortunately any millennial-scale and spatial variability associated with TraCE-21ka is now lost. Yet, because of the persistent and early warming associated with the intrusion of RAW in the Northeast troughs, the new oceanic forcing has a higher impact on the early Holocene retreat as compared to our previous results, thus confirming the crucial role of warming ocean waters in the Northeast deglaciation (Lloyd et al., 2023, Smith et al., 2023). With this improved setup we can simulate the retreat from the outer to the inner shelf fairly well, despite the 8 km resolution of the model (see Fig. 4 and 6). Also, the uncertainty of the sensitivity to the oceanic forcing is now tested, as we introduce the oceanic sensitivity parameter κ as a perturbation parameter of the ensemble (see Fig. S4). From this, we see that the oceanic forcing primarily controls timing and magnitude of the northeast margin retreat, which is first triggered by the intrusion of Atlantic Waters along the NEGIS coasts at the early Holocene. A higher κ promotes an early retreat of the northeast sector, contributing to reducing the discrepancy between modelled and reconstructed deglaciation timings from sediment cores from outer and inner shelf. Running our simulations at 4 km resolution would be computationally prohibitive, and we believe it is most important to explore the broader uncertainty in model parameters and boundary forcing as widely as possible.

3. Clarity of manuscript
There are multiple parts of the manuscript where the meaning of the wording is ambiguous, preventing a clear understanding of the motivation, method and interpretation. Several parts of this text are identified below.

Further comments

(unfortunately there are no line numbers to pin-point locations in the text)

Abstract:

- "both reconstructions and models" – Reconstructions from ice cores? Ice sheet models, climate models, or both? Modified as "*both the ice core based-reconstructions and ice-sheet models*".
- "The hypothesis…" is unclear – What hypothesis? That NEGIS has been in a similar configuration to today since 8000 years ago? We added this sentence in the Methods *"… we assume an abrupt relocation of the ice-stream flow from its past to its present position around 8 kyr ago. This is also supported by the recent hypothesis of the NEGIS showing a similar configuration to today since 8000 years ago (Gerber et al., 2021).".*

  Also, the abstract has now been modified to *"The recent hypothesis of a northern paleo Northeast Greenland Ice Stream (NEGIS) that was active during the Holocene and is now extinct indicates a potential early influence of the ice stream on Greenland ice-sheet dynamics.".*

- "thinning gap" – What thinning gap? The wording needs to more clearly link to the previous sentences. We removed this sentence from the abstract.
- "early retreat in northeast Greenland" – here and elsewhere, 'retreat' needs more specific wording – retreat of the grounding line in northeast Greenland? Modified as *"the early grounding-line retreat in northeast Greenland".*

Main

Part 1:

- "Punctual information" – What does this mean? Modified to *"Local information".*
- "surface elevation drop" – This wording may be misunderstood by a reader, and should be more descriptive as the first time it is mentioned. "Abrupt lowering of the ice surface elevation by X over Y years"? Changed as *"abrupt lowering of the ice*

surface elevation by several hundreds of metres (~200 m - 1000 m) over the past 11000 years".

- "data-model mismatch" – It would be useful to first describe the timing and magnitude of this data-model mismatch. Changed to *"Improved climate and isostatic rebound model representations have helped to reduce the early-Holocene (~11 ka ago) data-model mismatch at high-Arctic, peripheral locations from 900 m to only 200 m (Lecavalier et al., 2017)."*
- "meltdown" – I suggest more common wording (e.g. retreat, demise). Modified as "*demise*".
- "Northeast Greenland Ice Stream (NEGIS)" – It would be useful to include a map with the ice stream identified, perhaps with the locations of various reconstructions that are used in other figures. We have included a map showing the NEGIS paleo and present configurations defined in our model simulations (Figure 1). The location of Relative Sea Level reconstructions and moraine/boulder as well as sediment cores for which deglaciation ages have been calculated are shown in Figure 6 and 4, respectively.
- "this aspect"- What aspect? Changed to: "*The inclusion of subglacial hydrology schemes (Beyer et al., 2018) as well as extremely high basal geothermal heat flow (GHF) (Smith-Johnsen et al., 2020) has improved the ability of models to simulate the present NEGIS fast flow".*
- "inversion techniques are not valid…" – Explain why they are not valid, and how that is relevant to this study/your approach. Added *"... surface-velocity assimilation for basal shear stress estimation yields the most accurate basal friction approximation. Yet, assumptions of steady-state basal dynamics can only be made for decades-long simulations. The dynamics of the ice sheet can change considerably as the time scale increases due to not-negligible variations in the drainage system and changes in geometry (Franke et al., 2022)."*
- "Here we explore…" – This sentence could include "(see Methods for details)", or similar, at the end. Added "*see Methods for details*".
- "inland penetration of the NEGIS flow upon margin recession" – Unclear. Modified as *"We suggest that at least ~100 m of the surface elevation drop in north-central Greenland may be explained by the inland development of the paleo NEGIS, triggered by the retreat of the northeast grounding line. "*

Part 2:

- "the large ice cap" – What ice cap? Are you referring to the Greenland Ice Sheet in the northeast region of the continental shelf? Yes. Added *"the large ice cap in the northeast region reached over the continental shelf"*.
- "agreement with reconstructions…" – What aspect is in agreement with what reconstructions? The cited 2011 review paper shows that there are few constraints (at the time of publication) in this region. Added references: Arndt et al., 2015, Arndt et al., 2017, Winkelmann et al., 2010.
- "high Arctic temperatures" – needs to be more specific (e.g. air temperature, annual air temperature). Right. Modified to "*rapid annual air temperature rise*".
- "trigger and drive" – What is meant by "trigger" vs "drive"? We agree this expression is confusing. "Drive" has been removed.

- "ice sheet continental border" – What does this mean? The present-day ice sheet margin/coastline? Modified to *"ice-sheet retreat to today's margin*".
- "the NEGIS margin" – Needs to be more specific. The grounding line or the ice (shelf) margin? Modified to *"the NEGIS ice-shelf front*".
- "Such retreat history is congruent with…" – refer to the relevant figures where the simulations are compared to the data. Added "*(see Fig. 3d)*".
- "ice stream penetrates the interior" – Unclear and confusing wording. Describe the glaciological characteristics. Modified as *"Ice flow propagates upstream in response to thermo-mechanical and geometrical changes occurring during the northeast retreat of the marine-based ice sheet. At the onset of the deglaciation, ice-mass loss at the shelf edge due to rising air and oceanic temperatures weakens buttressing, accelerating inland ice flow. This results in glacier thinning, ice flow speed-up and retreat of the grounding line, which accelerates further the ice flow, promoting ice front recession. Although ice thinning at the margin reduces basal friction (as effective pressure decreases), sliding significantly increases when a local rise in subglacial melt occurs due to increased frictional heating. As the retreat persists, regions showing decreased basal friction conditions allow for flow acceleration farther upstream towards the ice-sheet dome (Fig. 3).*"
- "tail of the stream" - What does this mean? The lower reaches of the ice stream? The terminus? Modified as above.
- "In our simulations,…" - Why does the ice surface elevation increase prior to 9 kyr ago when air temp increases and retreat occurs? Vinther 2009 suggests that the increase in elevation at the beginning of the Holocene is likely due to an increase in precipitation and to the bedrock uplift. In our simulations, the ice surface elevation increases prior to 9 kyr mostly due to increasing precipitation, as the modelled bedrock uplift between 15 and 10 kyr ago is almost negligible (Fig. S9). The weighted mean surface elevation increase modelled at the early Holocene is however lower than that proposed by Vinther et al., 2009. This might be partly due to the fact that the precipitation used to force the model during the deglaciation simulation is lower than the precipitation reconstructed at NGRIP from ice cores (See Fig. S7 solid line in upper panel), as the climatological precipitation at the present is likely too low in central Greenland (Box et al., 2013). We therefore investigate the impact of an increased precipitation in central Greenland that matches better the accumulation signal from ice-core data on our best simulation (Fig S7, dashed line in upper panel). The results of this sensitivity test show that by increasing the precipitation at the early Holocene by 32% with respect to the control scenario, the surface elevation increases by ~20 m (Fig S7, bottom panel). Still, this increase in precipitation in north-central Greenland is not sufficient to cover entirely the data-model mismatch in surface elevation at the beginning of the Holocene.

  Most of the remaining mismatch could be attributed to the constrained bedrock uplift, as we are missing the isostatic response due to the demise of the NAIS. Lecavalier et al., 2014 show a RSL increase of ~20 m from 12 kyr to 8 kyr (~5m/kyr) in north-central Greenland (from -60m to -40m, Fig 11). Our simulations, however, underestimate this uplift at the beginning of the Holocene by half (uplift rate of ~2.3 m/kyr, as ~4.6 m uplift from 12 kyr to 10 kyr ago) (Fig S9, mid panel), thus likely contributing to underestimate the surface elevation increase. Forcing the model with a lower geothermal heat flux would induce slower basal flow at NGRIP, as there would be frozen ice, and higher elevation increase at the early Holocene. However, NGRIP has likely a thawed bed (Dahl Jensen et al., 2003, Mac Gregor et al., 2016), therefore we assume a higher GHF as more accurate. Sensitivity tests on this aspect are shown in Figure S5. These comments have been added to the Discussion section of the manuscript.

- Why is there an offset between the timing of reconstructed and modelled ice surface elevation change?

  The response time of the accumulation zone to an increase in accumulation is millennial for the Greenland Ice Sheet, when other forcings (sea level, ice temperature) are kept unchanged (Cuffey and Patterson 2010, Sec 11.2.3). This means that if accumulation increased around 11.7 kyr ago at NGRIP, as suggested by records, we should expect a complete response in ice thickness at ~9.7 kyr, which is what we simulate. Still, the fast increase in elevation suggested by Vinther et al., 2009 at NGRIP happens in only 1000 years after the reconstructed increase in accumulation (from 11.7 to 10.7 kyr ago). This short response time of the NGRIP surface elevation is likely the combined response of the ice sheet to changes in accumulation at the early Holocene and dynamical thinning upon margin retreat right after. We can try to disentangle this response by looking at two distinct temporal mismatches: the delay in the elevation increase before 10 kyr ago and the delay in the elevation decrease after ~10 kyr ago. For the first offset, we attribute the main cause of the mismatch to a combination of slow and insufficient precipitation change at the early Holocene (affecting thickness increase) and, on a second order, to the underestimated glaciostatic uplift upon the demise of the North American Ice Sheet (NAIS) (affecting bedrock elevation increase). Most of the thickness data-model mismatch in terms of timing and magnitude during the deglaciation is due to the inaccurate representation of the accumulation signal used to force the model (Spector et al., 2019). Therefore, we run an additional simulation with increased precipitation in north-central Greenland to show an increased response of the model in terms of elevation gained at the early Holocene (Fig. S7, see point above). In this experiment we do not vary the rate of change in precipitation, which remains that suggested by the highest scenario simulation of Badgeley et al., 2020. However, other reconstructions (Kindler et al., 2014) would support a faster increase in accumulation at the early Holocene, compared to our reanalysis precipitation signal (Badgeley et al., 2020). A higher rate of change in the accumulation would translate in a higher rate of elevation change, possibly reducing the offset between data and model at the early Holocene. Other factors might also have contributed to the wrong timing in the elevation increase at the early Holocene, such as the GIA response to the demise of surrounding ice sheets, which might have started well earlier during the deglaciation (~15 kyr ago). In our simulations we underestimate the rate of uplift by half due to the missing response to the retreat and disappearance of the NAIS (see previous point). In fact, the huge retreat of the NAIS between 16 ky and 10 kyr could have contributed to an additional bedrock elevation increase in north Greenland, that we are not taking into account. This signal would likely still pace the elevation increase at the dome, possibly reducing the offset between data and model.

The temporal offset in the dropdown is likely attributable to a simulated delay in dynamic thinning following the margin retreat. Since precipitation still increases at NGRIP until ~8 kyr ago, decrease in elevation occurs when dynamical thinning outpaces accumulation. In our simulations this occurs at ~9.8 kyr ago, ~800 years after the onset of the surface elevation decrease suggested by Vinther et al., 2009. We attribute this discrepancy to a delay in the simulated retreat of the inner northeast coast during the early Holocene (Figure 4, right panel). Although the retreat from the outer-middle shelf (before 11 kyr ago) is well simulated, the modelled deglaciation of locations close to the present-day margin is delayed by several centuries (millennia for Lambert Land and Sondre Mellemland). This implies that the impact of the Northeast ice stream in north-central Greenland may have occurred earlier than our simulations indicate, thereby contributing to outpace earlier the elevation increase due to rise in accumulation. Furthermore, the effect of the dynamic propagation from the margin retreat strongly depends on the location where the paleo-stream initiates. It is possible that the streaming area was situated in closer proximity to the ice divide already ~10 kyr ago than what is depicted in Figure 2. In our model, the primary control on the onset of the ice stream is provided by the inland extent of low effective pressure at the ice base (Eq. 4). The predefined value of cf at the NEGIS (f_{mid}), which is subjected to a temporal switch (Figure 1a), governs the potential presence of a stream. However, the actual activation of the stream takes place when the effective pressure reaches a low enough level to reduce friction at the ice base and initiate fast flow, i.e. when the basal water amount increases due to enhanced frictional heating. In our runs, this occurs only around ~10 kyr ago, when the northeast margin is still retreating from the inner shelf, and the increase in basal shear stress produces basal water to lubricate the bed (Fig. 2 and Fig. S12). However, the presence of an active hydrological drainage system at the NEGIS already in the early phase of the deglaciation (~11 kyr ago), with higher water pressure at the bed (lower effective pressure), could have induced an early development of the paleo stream. This cannot be accurately simulated by our model as it takes into account only local changes in the water system (Bueler & Van Pelt 2015). In support to an earlier paleo-stream activation, Franke et al., 2022 suggested that ice stream initiation is likely to happen at the beginning of the deglaciation, when streaming might be triggered by large changes in ice geometry, changes in subglacial water routing (Karlsson et al., 2015) and piracy from different catchments (Brouard et al., 2019). A paleo ice stream reaching far inland already at the early Holocene might have induced early ice advection from north-central Greenland, reducing earlier the surface elevation at NGRIP. Finally, we cannot rule out that the retreat of the northwest coast had a non-negligible impact on the NGRIP elevation dropdown. Although we compute a poor correlation between basal friction in north-northwest Greenland and the NGRIP thinning rate (Fig. S3), our model is not capable of fully representing the northwest dynamics and retreat. We likely simulate a delayed retreat in northwest Greenland, as the temperature anomaly used to drive the model exhibits a lesser early Holocene increase compared to reconstructions, which, on the contrary, indicate a rapid temperature rise occurring prior to 10.5 kyr years ago (Lecavalier et al., 2017). The early regional retreat might have initiated an earlier and stronger dynamic thinning in the northwest, potentially influencing the elevation history in north-central Greenland. Also, we do not take into account the debuttressing effect of the demise of the Innuitian Ice sheet, which could have amplified the retreat from the American continent. Simulating an active ice body in Ellesmere Island connected to Greenland, and inducing an earlier retreat of the northwest coast through a stronger and earlier temperature increase at the early Holocene would likely reduce the temporal offset. This whole discussion has been added to the manuscript in the Discussion section.

Part 3:

- “potential relationship between…” - What is the argument here? That basal sliding state explains the NGRIP elevation change? Modified to *“To test this hypothesis, we investigate the relation between the NEGIS dynamics, as defined by its basal sliding state, and the nearby NGRIP elevation change (Fig. 5).*”.

- “This implies that a faster-sliding and…” - What is the chicken, what is the egg? The wording suggests that basal sliding affects the NGRIP elevation, but this relationship needs a clearer glaciological explanation. What if these characteristics are both effects of retreat, and basal sliding changes are an effect of dynamic thinning? We agree that there is a complex interplay between dynamical thinning and changes in basal friction and that it is difficult to disentangle them as there is a positive feedback between the two via changes in the subglacial water system (Karlsson et al., 2015). In fact, basal sliding can change through time due to changes in the effective pressure (depending on ice thickness and water amount at the ice base) and bedrock elevation, as defined in equations 2 and 3. Therefore, dynamical thinning induced by the ice-sheet retreat might first contribute to decrease the basal friction - by decreasing the effective pressure - and accelerate ice flow. Still, basal friction at the stream reduces considerably only when there is a significant increase in basal water (due to an increase in basal frictional heating) (Fig. S12). The low effective pressure and basal friction are associated with an increased basal velocity at the stream region, which propagates the paleo-NEGIS upstream. The dynamic thinning (and surface elevation dropdown) simulated at NGRIP is therefore the indirect response to margin perturbation through changes in geometry and in the subglacial water system. In fact, the thickness decrease at NGRIP occurs following the decrease in basal friction at EGRIP, which in turn follows the decrease in basal drag averaged at the NEGIS basin due to margin retreat (Fig. S10). This lag between the decrease in effective pressure at the NEGIS basin and the increase in velocity simulated at the NGRIP is also visible in Figure 3. We interpret these results in the sense that it is the decrease in basal friction at the NEGIS - due to a decrease in the effective pressure, mainly due to increased water amount at the ice base - that controls surface decrease at the dome through dynamical thinning. Therefore, the mechanism that we propose is the following, as already pointed out previously:

  “Ice flow propagates upstream in response to thermo-mechanical and geometrical changes occurring during the northeast retreat of the marine-based ice sheet. At the onset of the deglaciation, ice-mass loss at the shelf edge due to rising air and oceanic temperatures weakens buttressing, accelerating inland ice flow. This results in glacier thinning, ice flow speed-up and retreat of the grounding line, which

accelerates further the ice flow, promoting ice front recession. Although ice thinning at the margin reduces basal friction (as effective pressure decreases), sliding significantly increases when a local rise in subglacial melt occurs due to increased frictional heating. As the retreat persists, regions showing decreased basal friction conditions allow for flow acceleration farther upstream towards the ice-sheet dome (Fig. 3). [...] Then, velocity propagation associated with the inland expansion of the paleo-NEGIS induces dynamic thinning of ice at the NGRIP site. The northern summit ice is advected downstream causing the surface elevation to decrease abruptly (Fig. 3a) until the end of the Holocene Thermal Maximum (HTM) (~5 kyr ago)."

Part 4:

- This is clear at the NEGIS margin,…" - Is the air temperature shown in Fig.4b just of the NEGIS margin?

The reviewer is right: Figure 4b shows temperatures at the NEGIS margin (79N) for two different climatologies (yellow and blue lines), but also the temperature reconstructed at the Agassiz site, for comparison (Grey line). We clarified this in the text as follows (Note that Figure 4b now is shown in Figure 6): "*This is clear at the NEGIS margin, where significant discrepancies are present at the Holocene inception (Fig. 6, lower-left panel, yellow and blue lines".*

Part 5:

- "Our late-Holocene results are consistent with…" - What aspect is consistent?

  We clarified this in the text as follows: "*Our late-Holocene NGRIP elevation change is consistent with previous modelling efforts evaluated against RSL estimates.* ".

- "NEGIS continental intrusion" - Unclear.

  Modified as *"Here, we attribute at least half of the estimated NGRIP elevation drop to the dynamic thinning associated with the Northeast retreat as induced by the Holocene warming. Such dynamic thinning is the result of ice-flow acceleration in the northeast triggered by the formation and inland development of the paleo-NEGIS during the last deglaciation.*"

Figure 1:
Note that Figure 1 has now become Figure 3.

- It should be made clear at the start of the caption that the modelled data in blue is from this study.
Added "*Modelled (blue, this work)*".

- Wording of "NEGIS ocean border" and "present margin was ice-free" is not clear.

As also pointed out by Reviewer#2, the inclusion of 14C dates extracted from material retrieved around the epishelf lake Blaso (Bennike and Weidick, 2001) are confusing. We decided to remove these points from Figure 3 and compare our results only to the ice-front retreat curve from Larsen et al., 2018, estimated from a collection of 10Be ages and 14C dates (including those of Bennike and Weidick, 2001) from various material collected around the outer and the inner coast of 79N, Zachariae Isstrøm and Storstrømmen Glacier. Therefore that sentence is removed from the caption.

- "Annual temperature" - specify that this is air temperature. Modified as "*Annual air temperature".*

Figure 2:

- Try for a colorblind friendly sequential color scheme, rather than divergent scheme. Modified with a colorblind friendly palette.
- The NEGIS basin should be identified in this figure, if not elsewhere. We added a sketch of the basins corresponding to the location of the paleo-NEGIS and present-NEGIS to Figure 1.
- "best run" - refer to Figure 3. Modified accordingly.

Figure 3:
Note that Figure 3 has now become Figure 5.

- "coloured following their Holocene maximum thinning" - The total amount of thinning across the Holocene at NGRIP? Modified accordingly.

Figure 4:

How was the dashed red curve determined? I assume that this curve is supposed to be the same as that in Fig. 2a in Larsen et al. 2018, but it does not seem to be exactly the same (e.g. differences in the variations in the slope of the curve, and retreat in the last ~500 years).

Yes, the dashed red/orange curve is modified from the curve shown in Figure 2 of Larsen et al., 2018. The curve has been modified so that the distance of the ice front from the present (y-axis) is described quantitatively instead of the qualitative description (shelf, outer coast, present, smaller than present) used in that paper. This has been done by assuming that the maximum extension of the ice shelf during the Last Glacial Maximum (~20000 years ago) reached the continental shelf break, that is ~300 km from the present ice front, and that the minimum extension of ~70 km from the present was reached ~6500 years ago. The distance of the ice front is then interpolated for the whole deglaciation using WebPlotDigitizer. We make this data available for further usage, although we are aware that this curve is a rough approximation of the ice-margin position throughout the Holocene. In fact, we would like to remark that in our revised manuscript this data is only used for having a visual comparison with our modelled ice-front position (Figure 3, panel d). For the ensemble evaluation we use instead the minimum ages of deglaciation inferred from a collection of samples collected in

Blaso, Lambert Land, Zachariae Isstrøm, Sondre Mellemland, Bloch Nunatakker, Storstrommer, Midgaardsormen (for the inner coast), and Bourbon Oer, Storoen, Kap Amelie (for the outer coast), published in Larsen et al., 2018. See Methods for more details. Also, to better capture the effect of different Holocene air temperature reconstructions on the Northeast margin retreat, we show the modelled versus RSL curves reconstructed for different locations in northeast Greenland (Figure 6).

Methods:

- “the upper mantle is uniformly set to relax in 3000 yr” - Why? Is this over 3000 years from the perturbation, by 3 kyr ago, or over the last 3 kyr? The temporal relaxation time is intended as 3000 years from the perturbation. Now, we include this as a perturbed parameter of our ensemble of simulation (τ), letting it vary from 500 years to 3000 years.
- “RMSE of simulated to reconstructed grounding line movement…” - Is this done using the approximated grounding line position time series from Larsen et al., or against the specific data points for different locations? In the first submission we calculated the RMSE of the ice-front position against the ice-margin retreat curve approximated from Larsen et al., 2018, yes. However, as pointed above, we now calculate the error of our model to replicate the deglaciation timings at different locations in the outer and inner coast around 79N, Zachariae Isstrøm and Storstommen Glacier, as collected by Larsen et al., 2018. 10Be and 14C dates are here intended as minimum deglaciation ages.

Data availability:

- “All Yelmo simulations presented here are available upon request” - The authors should provide the code used to run the experiments, and at least the best fit simulation.

We agree with the reviewer. The model code used to run the ensemble of simulations will be publicly available in Github, as well as the R scripts used for the ensemble analysis and the best fit simulation.

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Reviewer #2 (Remarks to the Author):

The paper investigates a possible explanation for the significant ice sheet elevation drop (~200m) reconstructed during the last deglaciation and through much of the Holocene, from analyses of the NGRIP ice core. It undertakes an ensemble numerical modelling exercise where they assess the dynamics of the Northeast Greenland Ice Stream (NEGIS), and the role it can play in the drawdown of elevation from the ice divide. The model is forced with high temporal resolution (not defined) climate data, has reduced basal friction and high geothermal heat flux and provides a good representation of the velocity pattern of NEGIS. As the ice sheet margin migrates landward during the deglaciation so the onset zone of the

NEGIS migrates farther back towards the ice divide and thus explains about 50% of the ice-core derived elevation change at NGRIP. Overall the findings of the paper support the general consensus of knowledge at the present time.

1 – It is unclear to me that this inference can be so straightforwardly made. During the deglaciation NEGIS evolved from a shelf-based terminal zone some 300 km in length into a more topographically constrained terminal zone (esp 79°N and Zacharie Isstrøm). If in the future the margin steps back into the "basin" area then it is perhaps less clear how the dynamics of the ice stream will evolve. It seems that the boundary conditions may be very different. In addition, Fig 2 e, f, g show the ice margin inboard of the present day and the drawdown of ice at NGRIP continues until ~4 ka (and longer in the ice core reconstruction). There is no modelled recovery in ice elevation presumably associated with the ice margin advance as seen in the ice core reconstruction. So how well the model is capturing the detail of the drawdown is less clear than being able to explain at least half of the total drawdown. Additionally thinking of a forward look is where of course where the numerical modelling will be invaluable. However, without such modelling I think this inference is ill-founded.

We agree with the reviewer that the future destabilisation of the region without showing modelling results that specifically tackle the future behaviour of the NEGIS is misleading. It is true that if the NEGIS basin recovers in the future, the dynamic response of the stream might be different from what we have modelled in our ensemble. However, other modelling work (e.g. Choi et al., 2017) suggests that the outlet glaciers 79N and ZI will continue to retreat for the next decades in response to increased ocean temperature, until topographic barriers are encountered. Melting at the grounding line induced by intrusion of warmer North Atlantic waters in the fjord has been found to be already the major driver of the ZI grounding line recession at the present (An et al., 2021). Conversely, the topographic barrier at the 79N glacier front prevented Atlantic currents an easy access to the cavity in the past (Schaffer et al., 2020). However, warmer waters have been recently observed below the ice shelf and close to the grounding line, causing the ice tongue to lose most of its mass through submarine melting (Wilson et al., 2017). Further thinning of the ice shelf might lead the grounding line in the western side of the glacier to float, allowing an easier access of warmer waters into the ice shelf cavity (Bentley et al., 2023). The potential collapse of the floating tongue will have severe implications for the near-future stability of the glacier, as buttressing force would be less, ice would accelerate and thin, favouring grounding line retreat. Such a mechanism of retreat at the present resembles that shown from our modelling results under conditions of increased ocean and air temperatures, as found during the last deglaciation. We therefore constrain our inference to "a retreat phase" of the glacier and change the sentence at the end of the Introduction to:

"Such a far-inland reach of dynamic thinning in northeast Greenland during a major retreat phase suggests that such processes may also amplify the response of this sector to future warming and ice loss."

Following suggestion from Reviewer #3 we also modify the last sentence to: *"Our results imply that a future northeast margin retreat stemming from increasing temperatures might similarly provoke tens of metres of dynamic thinning in central Greenland. Given that the present-day NEGIS velocity pattern is not captured by most ice-sheet models (Goelzer et al., 2018), our results suggest that inland dynamic thinning in north-central Greenland is* *underestimated by future projections and Greenland's contribution to future sea-level rise may be greater than currently anticipated."*

2 – are these numbers correct. It appears from Fig 2e that the ice margin at 8 ka is further inboard than the current grounding line location and retreat continues until at least 6 ka. The "further recession" seems to be rather more than ~100 ±20 m, which I would have thought to be rather below the model spatial resolution? Indeed, there is no information provided on the spatial resolution at which the model has been run?

The Reviewer is certainly right, we are not talking about metres but kilometres. This was a mistake, as they correctly point out, as we run our ice-sheet model at a spatial resolution of 8 km. This sentence is now corrected and reflects the results shown in Figure 3: our simulations show a retreat of the northeast ice front of about ~110 km (±40 km of standard deviation) between ~7 and 5 ka ago with respect to the present-day margin. Both the timing and magnitude of the retreat are comparable to the Holocene retreat reported by Larsen et al., 2018, as suggested from cosmogenic exposures and radiocarbon dates. Still, our simulations model an earlier retreat (~10 kyr ago) at the present margin of the 79N glacier. This is also visible from the Relative Sea Level (RSL) change modelled at locations close to the present margin location (Figure 6, f-j), which show an earlier drop with respect to data. Yet, the timing of the recession from the outer to the inner shelf is very well in agreement with deglaciation of moraines at the front of the NEGIS outlet glaciers (Larsen et al., 2018) and of marine locations in the northeast shelf derived from gravity core analysis (blue dots of Fig. 4, right panel). We did however note down in the text the spatial resolution of our simulations, as reported at the beginning of the Methods: "*Yelmo is a three-dimensional, thermodynamically coupled, higher-order ice-sheet-shelf model that is here run over the Greenland domain at 8 km resolution.*".

3 – it is unclear why this description is used? The surface velocities depicted in Fig 2 A-D appears to show two well-defined ice streams linked to the broad NEGIS catchment draining thought Westwind and Norske troughs? This fits well with mapping of the sea floor which shows evidence of glacial lineations (e.g. Arndt et al, 2017 http://dx.doi.org/10.1016/j.quascirev.2017.01.018; Olsen et al, 2020 - doi.org/10.5194/tc-14-4475-2020) which in such settings is often liked to ice stream. The fast ice flow seems well structured and topographically controlled by the troughs crossing the continental shelf?

We agree with the reviewer that "unstructured system of fast glaciers" was not the best expression to describe the ice stream configuration during the glacial period, as it was clearly topographically controlled. Yet, the structure of the ice streams at the LGM is now slightly changed for the new best simulation. The two big streams flowing over Westwind and Norske trough are still present, but a third stream appears in the area between the two troughs, feeding the today's location of Zachariae Isstrom and confirming what is suggested by bathymetry data (Arndt et al., 2017). This description has now been added to the manuscript as:

" The ice-stream configuration at the onset of the deglaciation shows three well-defined fast flowing regions draining ice mass from the present-day location of the Nioghalvfjerdsbrae (79N) and Zachariae Isstrom (ZI) outlet glaciers to the shelf break. Two topographically

constrained ice streams flow over the northern and southern troughs, whilst a third stream flows eastward over the shallow bank. The location of this ice-stream system aligns with glacial lineations mapped in the area from bathymetry data (Arndt et al., 2017). An ice stream flowing eastward and draining the shallow bank between the troughs would support the hypothesis of the GrIS stretching towards the shelf edge during the LGM (Larsen et al., 2018)."

4 - The model fails to capture the duration and rate of NGRIP drawdown (see also point 1) and some comment on this is warranted. In light of this, the reduction in modelled surface velocities is likely to be a function of the "steady" NEGIS. The longer and lower rate drawdown reconstructed from NGRIP does not at first sight square with the modelled surface velocity reduction. Some comment on this is warranted.

In the new set of simulations, the northeast margin slowly regrows towards its present position for the last 5 kyr. This reduces the surface ice velocities increase modelled at NGRIP and the surface elevation dropdown. This is in contrast to the ice core reconstruction, where the surface elevation continues to decrease until today. Yet, the decrease in basal sliding imposed at the present-NEGIS (Fig. 1, left panel) ensures a continuous drainage of the northern sector of the ice sheet until present (Fig. 2 and 3), although reduced. We discuss this by adding this paragraph:

"For the period 8-0 kyr, our modelled evolution is found well within the reconstruction uncertainties. Yet, ice-core data show a higher elevation drop throughout the last phase of the Holocene. Although we impose a decreased basal friction coefficient at the present NEGIS for the last simulated 8 kyr, we fail in reproducing the rate of the late-Holocene dropdown. We attribute this discrepancy to the northeast margin advance after the HTM, which slows down the ice flow in the northeast, reducing ice advection and the NGRIP elevation decrease. Still, our late-Holocene NGRIP elevation change is consistent with previous modelling efforts evaluated against RSL estimates (Lecavalier et al., 2013, 2014)."

For a complete discussion on the mismatch in the duration and rate of the NGRIP dropdown please refer to the answers to Reviewer #1. The discussion is also reported as an additional paragraph in the manuscript.

5 – The grounding line location of NEGIS characterised by the record of Larsen et al, 2018. It appears that in this paper the grounding line in the model (easily identified) is assumed equivalent to the ice margin location defined by Larsen et al, 2018. However I do not think they are the same thing. My understanding of the Larsen reconstruction is that the distance is measured from the present ice margin i.e. the edge of the ice shelf, and not the present grounding line. From the caption for his Fig 2b – "^{10}Be ages with external uncertainties from outer coast (dark gray) and outside the Little Ice Age moraine (black), and ^{14}C dates from raised marine deposits presently dammed by NEGIS up to 70 km upstream the present ice margin at NG". I am also not clear how the Bennike and Weidick (2001) data have been interpreted in this paper i.e. what "Radiocarbon dates at the NEGIS ocean border" means. I assume these ages relate to the samples collected around the epishelf lake Blåsø. These samples are taken proximal, +10 km beyond, the present grounding line. The samples include marine mammal bones (seal and whale) and shells and driftwood. The bones and shells are assumed to indicate that Blåsø is no longer a lake but is fully connected to the

fjord with no ice shelf but possibly permanent/semi-permanent sea ice in the fjord. The driftwood indicates that there was no ice shelf at Blåsø and the fjord was of sea ice free in summer. Bennike and Weidick (2001) provide no estimate for a grounding line location just saying that "the open fjord may have extended as far west as the present grounding line, just south of the western end of Blåsø." My reading of Figs 1, 2, 4 and 5S and the text indicates that this paper has assessed the modelled grounding line distance measured from the present location of the GL at 79°N. The validation process has calculated using the goodness of fit based on the "RMSE of simulated to reconstructed grounding line movement at 79°N (79_GL, data from Larsen et al., 2018)". Larsen provides the location of the ice margin (actually the ice shelf margin) not the grounding line. If I am correct then the scoring matrix needs to be recalculated to and the results reassessed. If I am incorrect then the Figures need to be amended and how the ice margin record of Larsen et al, 2018 can be correlated to the grounding line of the model output.

The reviewer is right and we thank them for pointing out this inaccuracy. The distance of the margin position from present at 79°N glacier is now calculated as the distance of the ice-front position and not the grounding line (Fig. 3, panel d). However, we no longer include the distance margin position at 79N in the evaluation process. Instead we include the RMSE of modelled-observed deglaciation timings computed and averaged 1. at four sediment core locations from the northeast continental shelf (SEDIM_DEG, data from Hansen et al., 2022, Lloyd et al., 2023, Davies et al., 2022, Syring et al., 2020), 2. at three locations in the outer shelf as calculated from 10Be exposures dates from morain/boulder samples (OUT_MORAIN_DEG, data from Larsen et al., 2018) and 3. at other six locations closer to the present NEGIS margin (INN_MORAIN_DEG, data from Larsen et al., 2018). Still, we introduce a last binary control on the upstream retreat of 79N glacier: simulations that retreated more than 70 km upstream will present a maximum error and vice versa (see Methods for more details). Moreover, sensitivity tests between different climatologies are now investigated in terms of RSL changes at various locations in the northeast, instead of showing the modelled distance of the margin position from present (Fig. 6). We believe that showing the modelled RSL against observations provides much more information than the glacier margin position at 79N glacier. First, we are now looking at how the model responds to climate for different locations and not only at 79N; second, RSL data are more robust than the qualitative ice-margin fluctuation from Larsen et al., 2018, which is reconstructed from 10Be and 14C data retrieved from various locations around the NEGIS outlet glaciers.

6 – given the potential importance of ice shelves to the flow of their up-stream ice streams, it is surprising that there is no mention in the model, nor in the modelling of an ice shelf? Does the model include an ice shelf? It does not seem so from the surface velocity show in Fig 2, or is the velocity only for grounded ice shown? Some explanation of how the model deals with ice shelves is warranted, what exactly is being shown in Fig 2 and the potential impact of ice shelf loss, for example their loss e.g. Zacharie Isstrom, on the flow of the NEGIS is needed.

Our model is an ice sheet-shelf model (Robinson et al., 2020), therefore it includes ice shelves and it solves their physics. This is now better explained in the Methods as "Y*elmo is a three-dimensional, thermodynamically coupled, higher-order ice-sheet-shelf model that is here run over the Greenland domain at 8 km resolution. The model has been recently improved to adopt the depth-integrated-viscosity approximation (DIVA) velocity scheme*

(Lipscomb et al., 2019, Robinson et al., 2022), a higher-order stress approximation proven to be efficient for multi-millennial simulations. This solver is found to replicate well the flow found in the ice-sheet interior but also that of fast ice streams and of floating ice shelves (Robinson et al., 2022)". We also agree that Figure 2 was misleading, since it was only showing the grounded velocity, as the reviewer correctly points out. We now show the contour of ice shelves with a black line. Finally, a description of the impact of loss of buttressing from ice tongues and sea ice on the NEGIS flow is added in the discussion section.

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Reviewer #3 (Remarks to the Author):

Dear authors,
Thank you for the opportunity to review this manuscript. In general, I found the manuscript enjoyable to read, the results exciting and novel, and the conclusions well-supported. This manuscript will be of interest to many in the glaciology and paleoclimatology communities. The results have important implications for projecting future sea-level rise and highlight an area of ice-sheet model development that has been elusive for decades (representing the flow of large ice-streams and in particular the NEGIS) but is clearly critical for both hindcasting and forecasting ice-sheet dynamic responses to a changing climate. This is an excellent manuscript and with attention to a few key adjustments outlined below, I think this manuscript is very suitable for publication in Nature Communications.
I applaud the authors for their clever experimental design and novel approach to diagnosing the role of ice-stream dynamics in driving thinning in the ice-sheet interior. In my line edits, I have flagged where I think a statement is too strong or slightly over-interpreted, but in general the conclusions drawn here are well supported by a clear experimental design and a careful analysis.
I have one overarching question about the framing in this paper – in some places (like the title) there is a geographic restriction to the NEGIS influence implied (thinning in central Greenland) but in other places (the abstract and the conclusion) the influence of fast ice-stream flow for the full ice sheet is gestured at. I think the authors could refine the framing of this article to be focused just on the NEGIS influence for this catchment alone (or north-central Greenland, or demonstrate exactly where for the ice sheet the NEGIS has a dominant dynamic effect on thinning through the Holocene). After all, just the NEGIS catchment amounts to something like 12% of the entire ice-sheet volume or 1.1 meters so it's a significant portion of the whole Greenland ice mass and thus there is a clear case that we should care about how it behaves dynamically (e.g. Mouginot et al. 2015). Alternatively, if the authors want to maintain the ice-sheet wide perspective, I think they need to show that NEGIS impacts more than just the NGRIP site during the Holocene. Perhaps a map of where in Greenland the ensemble shows divergent surface elevation histories versus where the ensemble members are in greater agreement about the elevation history would demonstrate this nicely, and allow the authors to better support statements such as "Our findings hint at a wider and more dramatic influence of the fast ice-stream flow on the whole ice-sheet behavior than previously expected" and "Such a strong dynamics-induced destabilization of the ice divide warns us about a possible extreme response of the ice-sheet interior to future warming ice loss."

In summary, this is a strong and clear manuscript that advances the field and I hope to see this exciting work published soon with a few slight modifications.

We agree with the reviewer that the impact of the NEGIS on the whole ice sheet should be either better demonstrated or reduced to the northern sectors of the ice sheet. We followed the reviewer's suggestion and we decided to stick to the second option, as a comprehensive analysis of the NEGIS impact on the whole ice sheet would require extensive work that is beyond the scope of our manuscript. Therefore, we reframed the last part of the introduction as suggested by the reviewer, a few lines below: *"Such a far-inland reach of dynamic thinning in northeast Greenland during a major retreat phase suggests that such processes may also amplify the response of this sector to future warming and ice loss."*

REFERENCES

Mouginot, J., Rignot, E., Scheuchl, B., Fenty, I., Khazendar, A., Morlighem, M., Buzzi, A., and Paden, J., 2015, Fast retreat of Zachariæ Isstrøm, northeast Greenland: Science, v. 350, p. 1357–1361, doi:10.1126/science.aac7111.

LINE EDITS/QUESTIONS

Please note – the PDFs I received did not have line numbers so the page numbers below refer to the page of the PDF.

Main text.

Page 1.

ABSTRACT. "Our findings hint at a wider and more dramatic influence of the fast ice-stream flow on the whole ice-sheet behavior than previously expected." I do not think you have shown fast ice-stream flow impacts the whole-ice sheet behavior, but you have very clearly and directly demonstrated it for the northeast catchment, from the oceanic margin all the way to the ice divide. To me this is impact enough, but if you want to keep your statement as-is then I would like to see a clear demonstration that the impacts of NEGIS reach the whole ice-sheet.

We agree that the impact of the NEGIS flow is not demonstrated for the whole ice sheet and we removed that sentence from the abstract. See comment above.

MAIN

"Punctual" – I'm not sure what you mean by this – could you use another word? Punctual sounds like it could mean abrupt, discontinuous, on-time? Do you mean well-dated, or something else? Changed to "local".

"~300 km long" check this length for NEGIS? I thought it was closer to 700km. Right. We changed it to ~600 km long.

Page 2.

GHF – should this be geothermal heat flow or geothermal heat flux?
Despite conventional use of flux, experts propose that flow is the right term (see Section 1.2 of Burton-Johnson et al., 2020a for instance, or Burton-Johnson et al., 2020b). Therefore, we prefer to use the expression "geothermal heat flow" as it is gaining momentum in the scientific community but we agree there is no consensus yet on which is the most correct expression.

"Penetration" – suggest a different word. Propagation?
Changed to "*development of the paleo NEGIS, triggered by the retreat of the northeast grounding line*", according to Reviewer #1.

"Such a strong dynamics-induced destabilization of the ice divide warns us about a possible extreme response of the ice-sheet interior to future warming ice loss." I think this is too strongly worded and I don't think "destabilization" is the right word either. What about "Such a far-inland reach of dynamic thinning in northeast Greenland during a major retreat phase suggests that such processes may also amplify the response of this sector to future warming and ice loss." Changed accordingly, see answer above.

"Several tens of meters" based on Figure 1 I think you mean kilometers? Definitely yes. It was a mistake. We corrected it to kilometres.

Page 3

"Penetrating" again – could be replaced with "more inland-propagating" or something else? Or is there a reason why you prefer the term "penetrating" here – I may be missing something but to me, NEGIS is not "penetrating" the ice sheet but instead the dynamic controls on the ice-sheet position are allowing the fast-flow to propagate farther and farther upstream. Note – later on page 5 you use "continental intrusion" – I think if you pick one phrase that you think best captures the phenomenon you are talking about it, and use it throughout, that would be helpful for the reader.

We agree that penetrating is not the best word to use in this context. This has been changed in Page 3 as to "*This implies that faster sliding in the proximal NEGIS onset zone increases the NGRIP elevation dropdown, which intensifies as the ice stream propagates inland*". We also changed the expression in Page 6 at the end of the Main section as to "*Here, we attribute at least half of the estimated NGRIP elevation drop to the dynamic thinning associated with the Northeast retreat as induced by the Holocene warming.*"

"indicating that any other ice stream across the ice sheet has no significant involvement in the NGRIP thinning." – reword: indicating that no other ice stream across the ice sheet has significant involvement in the NGRIP thinning. What does "other ice stream" refer to here – may be confusing to some readers who are not familiar with the difference between glacier versus ice stream and whether some ice flow features we identify as "glaciers" today may be more aptly considering "ice streams" under paleo flow conditions. What if you reworded this to instead say "indicating that there is no catchment except for NEGIS where ice flow has significant involvement in the NGRIP thinning" or something similar? Also, can you really say this? If you did the experiment to reduce the bedrock by one sigma beneath all of the major flow features in Greenland, would none of them contribute to the NGRIP thinning? Maybe just say "No similar correlation is found for any other GrIS basins in our experiments (Fig. 2S)."

We agree with the reviewer's suggestion and we changed the sentence to "*No similar correlation is found for any other GrIS basins in our experiments (Fig. S3), indicating that there is no other catchment where ice flow plays a significant role in the NGRIP thinning*". We reduced the bedrock at the NEGIS to help the model find the location of the present NEGIS, as the stream is not topographically constrained. Now this workaround is done also for the paleo branch of the ice stream. It would not make sense to decrease the bedrock in all the other catchments, as streams outside the northeast are already quite well represented in our model (Figure 2). By reducing the bedrock there, we would likely induce a faster and more upstream flow which would not be representative of reality. However, even in that case, we would not expect a significant contribution of these ice streams to the NGRIP dynamics, since most of the correlations between the other catchments and NGRIP thinning rate is either zero or negative for this set of simulations (Figure S3).

Sentence "although even higher GHF values…" does not make sense to me. Do you mean " Although even higher GHF values have been suggested in northeast Greenland as attributed to the passage of the Icelandic hotspot (Rogozhina et al., 2016), a GHF of just 57 allows our model…" ? Yes, we mean that. We changed the sentence as *"Despite even higher GHF values suggested in northeast Greenland as attributed to the passage of the Icelandic hotspot (Roghozina et al., 2016), a GHF of just 63 mW/m2 allows our model to simulate a temperate bed at the NEGIS, in line with estimates (MacGregor et al., 2016)".* Note that in the new set of simulations the minimum averaged value of GHF that allows a temperate bed at the NEGIS is 63 mW/m2.

Check units of GHF – should be mW per m2 ? Yes, corrected to mW/m2.

Spell "MacGregor". Corrected.

The paragraph starting "Runs with a very fast ice stream…" Could you add one or two sentences describing any secondary controls on the NGRIP thinning rates from your ensemble, and comment briefly on what that means in terms of the influence of ice-stream dynamics on the thinning? For example in Fig 3S – z0 plays a less important but still influential role on inland thinning rates.

That paragraph has now substantially changed as we run a larger ensemble with 7 more parameters investigated (see the introduction of this document). In this new setup it is even clearer that a first order control on the NGRIP thinning comes from the basal friction coefficient set at the NEGIS (Figure S4), as the other parameters in the friction law show a negligible correlation. Interestingly, also the enhancement factor for shear regions (enh) has a strong control on the NGRIP thinning: the lower is the enhancement factor, the higher is the NGRIP dropdown. In fact a stiffer (less viscous) ice-sheet interior induces low velocities far inland, allowing for a higher surface elevation increase at the dome during the early Holocene. Second order effects come from the ocean sensitivity factor (κ), the lithosphere relaxation time and the ablation parameter itm_b. First and second-order controls of model parameters on the NGRIP surface elevation drop are now added in the manuscript in the Discussion section.

Page 4

"Here, bedrock uplift is due to the sole GrIS mass loss" reword: "Here, bedrock uplift is solely due to GrIS mass loss." Corrected as *"Our approach attributes bedrock uplift solely to GrIS mass loss…".*

"Yet, the GIA signal from the Greenland peripheral ice sheets deglaciation" – what ice sheets are you referring to here – you mention the Innuitian, is that what you mean? Why not just say that – "Greenland peripheral ice sheets" is confusing. Yes, we refer to the Innuitian ice sheet. Corrected accordingly.

"crucial for a correct retreat" is it possible to quantify this following your method – e.g. the median skill score for the ensemble using each of the forcings? This would provide some quantitative foundation for what you mean by "correct retreat".

Following the reviewer's suggestion, we report the lag in the timing of deglaciation modelled by the two ensembles with respect to that reconstructed at marine core locations, morain/boulder locations in the outer coast, and morain/boulder locations in the inner coast close to the present-day margin (see Methods). The lag is calculated as the median of the modelled-reconstructed deglaciation timing of each ensemble (only good simulations are considered), averaged between the members of each category (4 sediment cores, 3 morain/boulder in the outer coast and 6 in the inner coast). The calculated lag in the timing of deglaciation at sediment core locations is 1117 years for B18 and 3483 years for B20; at rock locations in the outer coast it is 2520 years for B18 and 6548 years for B20, and 333 years for B18 and 3500 years for B20 at the present-day margin location. In the median, the temporal lag is reduced for all categories for the B18 ensemble. This confirms that the ensemble forced by B20 has a stronger delay in modelling the northeast retreat. We also added a scatterplot of modelled versus reconstructed deglaciation timing for each specific marine sediment core and moraine locations for the two different climatologies to further prove our claims (Figure 4).

Page 5.

The final sentence is really important and I suggest some slight changes to wording to really drive your main point home. Your logic here makes sense to me but it's a very strong statement with big implications so I think it could be important to guide readers through your thought process here.
I don't really understand what you mean by "destabilization." I don't think you have shown that the Holocene warming destabilized northeast Greenland – what you have shown is that dynamic thinning in central Greenland is largely attributable to the development of the proto-NEGIS during the early/mid Holocene. But couldn't this still be the "steady-state" response of the ice sheet to the transient climate forcing? To me "destabilize" implies a runaway or positive-feedback process which I don't think you have shown here. I would avoid the word destabilize and try to stick closer to the physical processes that you have

studied in this final paragraph. I suggest something like: Our results imply that a future northeast margin retreat stemming from increasing temperatures might similarly provoke tens of meters of dynamic thinning in central Greenland. Given that the present-day NEGIS velocity pattern is not captured by most ice-sheet models (Goelzer et al., 2018), our results suggest that inland dynamic thinning [in north-central Greenland] is underestimated by future projections and Greenland's contribution to future sea level rise may be greater than currently anticipated.

This point was also raised by another reviewer. We agree with both reviewers that this is a strong statement, that we cannot fully demonstrate from our modelling experiments. We have made the decision to modify the paragraph in order to align more closely with the physical mechanisms outlined in our paper, as suggested by the reviewer.

FIGURES

Figure 1(b), Figure 4

How is "distance from present position at 79ºN" calculated? Along the central flowline for the measured velocity? Consider using "79ºN Glacier" everywhere you refer to this (e.g. Figure 4 caption) so that it is easily differentiated from "79ºN" the geographic line of latitude – for example in Figure 1 caption (e) where is this climate reconstruction from – the outlet of 79ºN glacier?

The distance of the 79N ice front from its present day position is calculated along a transect that flows close to the left flank of the glacier (close to Blaso and Midgaardsormen, where data from Larsen et al., 2018 were collected) and extends up to the continental shelf break (to capture the extension during the glacial). This transect is now shown in Figure S1. Figure 4 (and its caption) has now changed and it doesn't show the margin distance anymore.

Figure 2
Please label the colorbar. Done.

Consider adding 50 m/a contour from observed velocities on top of the "present day" modelled velocity field – I think that would emphasize that your model does a great job at reproducing NEGIS.

Now Figure 2 shows additional contours for ice shelves locations. The new "best simulation" shows a clearer NEGIS position as the previous version of the manuscript. Therefore, we decided to not include additional contours to not burden the figure.

Consider adding to at least on panel the location of Agassiz ice cap as this is referred to throughout the text. Done.

Figure 3
Delete "for valid runs" – modify sentence to read something like: " Thinning rate and shear stress are averaged between the time of Holocene maximum elevation and the present; coloured points follow the colour palette of panel (a); runs that do not meet scoring criteria are shown as grey dots; correlation coefficient and p-value for scored runs are also reported.

The best run of the ensemble is shown as a blue triangle.” Done. Note that Figure 3 is now Figure 5 in the revised version of the manuscript.

Figure 4

Why is the spread for the yellow ensemble so much greater than for the blue ensemble? Also these ensembles seem to be diverging already by 12ka but it is difficult to see what the reconstructions are doing at 12ka in the inset. Is it possible to show more of the reconstructions (maybe just to 14ka so it matches the ensemble plot)? Also consider labelling the gray line in (b) directly on the plot as “Agassiz ice cap temperature” or similar – because this does not correspond to plot (a) I found it confusing but I could see from the text why you wanted to include it.

Figure 4 now shows the modelled RSL for various locations in north/northeast Greenland. Yellow and blue shades represent the results for the model forced by the two different climatologies (Buizert et al., 2018 and Badgeley et al., 2020). We believe that showing the modelled RSL against observations provides much more information than the glacier margin position at 79N glacier. First, we are now looking at how the model responds to climate for different locations and not only at 79N; second, RSL data are more robust than the qualitative ice-margin fluctuation from Larsen et al., 2018, which is reconstructed from 10Be and 14C data retrieved from various locations around the NEGIS outlet glaciers.

METHODS

Page 10. “It is only one (with another)” what does this mean? Consider “It is one of only two scenarios…” ? Changed accordingly.

Page 11.

Can you add a figure to the supplement showing how the bed elevation was changed (minus one sigma) within the NEGIS boundaries? The decreased bed elevation has been added to Figure S1 in the supplement.

I think this sentence should read: “absolute modelled-observed modern surface elevation DIFFERENCE at NGRIP” ? Changed accordingly.

How robust are your estimates of the % of thinning attributable to NEGIS propagating inland depending on which of these misfits you use? Is the best-fit scenario the best fit across all of the different misfits, or does one or a few of these misfits dominate the results? Can you say something about why these parameters were chosen to calculate the misfit?

The misfits are equally weighted in the final score (eq. 11) therefore they are equally accounted in the evaluation. The best-fit scenario is then the best fit simulation calculated from the multiplication of all different skill scores (eq. 12). As seen in Fig S6, the best simulation has very high scores for most of the misfits. Still, it shows a greater error in terms of ice thickness averaged for the whole Greenland. This is mainly due to the low enhancement factor set for shearing areas, which ensures a good fit in the northeast (as taken with other parameters calibrated for that catchment), but tends to simulate too much ice for the rest of the ice sheet interior. However, since we are interested in the northeast basin, we do not see this misfit as crucial. This points to the fact that most parameters should be spatially variable (e.g. basal friction coefficient cf, precipitation scaling factor for past temperature anomalies f_p) to fully capture the complexity of the ice-sheet dynamics. Finally, the misfits were calculated taking into account available observations that could cover both the past and present history of the ice sheet. Misfits for the northeast basin were prioritised, because of our regional focus, although several Greenland-wide errors were also taken into account to demonstrate the validity of our simulations at a larger spatial scale.

How did you choose the criteria for runs to discard as NGRIP_PD<40m ? Assuming the above edit is right, maybe also just drive it home: "…runs are considered valid as long as the absolute elevation difference between modelled and observed elevations is less than 40 meters (e.g., NGRIP_PD<40m), so that 122…"

Changed as *"Runs are considered valid as long as the absolute elevation difference between modelled and observed elevations is less than 50 metres (e.g., $NGRIP_{PD}$<50m) so that 563 simulations over 3000 are retained as valid*" (note that now valid runs have NGRIPPD < 50 m). We chose this additional evaluation criterion to narrow the spread in our best results. However, since now we perturb also the Enhancement factor, which mainly describes how fast is the flow depenfding on ice stiffness, and therefore controls the difference between the modelled and the observed surface elevation, this additional criterion does not influence the results too much.

Page 12

Fix: Reference climate FROM Box. Corrected.

Page 13 – Karlsson reference should come before Le Meur reference I think? Corrected.

SUPPLEMENT

Figure S5 – Can you add the blue triangle to each of these panels for your best simulation? Added.

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REVIEWER COMMENTS

Reviewer #2 (Remarks to the Author):

This is a substantially revised version of a paper previously submitted to the journal. It is one of the most comprehensive efforts in the addressing of reviewers comments that I have seen and for this I commend the authors. The aper is investigating the causes of the lowering of the ice divide at NGRIP in the early Holocene as identified in ice core based reconstructions. Numerical models have failed to reproduce this thinning leading to uncertainties on the veracity of either the models or the ice core reconstructions. The results from this study have linked the presence of a “prescribed” larger (than present) northern component of NEGIS which can facilitate drawdown in the ice divide. The modelling results are well validated against palaeo-ice sheet data from both marine and terrestrial environments. It will be of interest to the ice sheet community as a whole and the wider palaeoclimate community.

Overall it is a robust attempt to address how the retreat of the ice margin in the NE sector of the GriS initiates multiple ice streams (the “remnants” of which are still present in the ice sheet) which induce an inboard thinning and speed up that ultimately leads to drawdown of the ice surface at the ice core site of NGRIP. It brings modelling outputs and ice core reconstructions closer together, though it does not resolve the difference between them completely. Validation of the model outputs against the empirical data shows that the best results capture the overall pattern very well, thought some mismatches in timing and magnitude remain. This robust grounding of the modelling in comparison to multiple empirical palaeo-datasets provides some confidence that is it identifying the key controls in the drawdown of ice at NGRIP. There is a rather long final section which discusses the mismatches between the model and reconstructed data which is very honest but is perhaps a little negative overall but as above it does provide confidence in the results and their meaning. It also points towards multiple developments in the modelling which might improve the results further.

I have no major substantive issues with the paper this time around but have provided numerous comments on the manuscript and supplementary material both of which I have uploaded with this review. These really need to be addressed as there are many minor errors/things missing throughout the text and in the figures and statements/sentences/sections which are unclear in the way that they are written. As it stands, they detract significantly from the readability and message that the manuscript is trying to convey.

In conclusion: this paper addresses a significant issue as it is trying to resolve a miss match between a data-based reconstructed numerical model output. It is robust both from the modelling perspective and how the modelling results are assessed using empirical palaeo-datasets linked to the evolving ice sheet geometry. From this perspective I think therefore that it will be looked upon favourably by a number of academic audiences. So I think that this is, in principle, acceptable for publication pending the numerous corrections that are required to the text and the figures.

Reviewer #3 (Remarks to the Author):

The manuscript by Tabone et al. has been thoroughly revised based on feedback from three reviewers, and through thousands of new simulations ran with an improved version of their ice model. I am satisfied with the author’s responses to my comments on a previous version of the manuscript – and in particular I think their efforts to reframe the implications of their findings to be more specific to the northeast sector of the ice sheet have made their conclusions more impactful. All my major concerns with with the previous version of the manuscript have been fully addressed in the new version. I think this manuscript advances our understanding of the history of the Greenland ice sheet by illuminating clear links between ice-sheet dynamics, climate forcing, and deglaciation process that are relevant to projections of Greenland's future.

By expanding their ensemble to include other parameters of interest and many more simulations, they have shown that the ice-flow in northeast Greenland plays a (potentially large) role in ice-surface elevation drop in central Greenland – this is now an even more robust conclusion. They have now validated their simulations against an even greater variety and number of proxy data types/records. I have minor suggestions below, which mostly have to do with the wording of certain phrases, that I think would make some of the arguments clearer. Given the enormous amount of work the authors have done to strengthen the manuscript based on the past round of reviews, I think it should be published with minor revisions.

One thing that strikes me is that there is quite a bit of text, and a new figure, about the switching of NEGIS tributaries at ~8ka. I am not sure how this relates to the central argument of the paper. If your conclusions are largely the same without imposing this switch in the ice stream tributaries, why is it important that you include this in the model set-up? I think I am somewhat biased here because I have read a previous version of this paper that largely had similar results, so my immediate questions are 1) why is this included and 2) does it impact the results? After spending time with the paper I think that the answers to these questions are 1) because our understanding of the ice stream history has changed in the last couple of years and this "tributary-switching" is now the most consistent with that understanding (although see lines 118-125… there is a lot about this approach that could (and should in future work) be altered or tested) and 2) it actually doesn't seem to impact the results in a meaningful way. Obviously the representation of present-day ice flow velocities is improved but in terms of the central argument of the paper I'm just not certain what the connection is and I find it kind of distracting. It took me a while to understand that this was not an experiment that was taking place as part of the ensemble but was rather a new way to set up the model and run all of the experiments, and it didn't have much of an impact on the overall conclusions, even if the details of the ice dynamics/ice history are of course altered by this approach to basal sliding in the northeast. I don't feel strongly that this undermines the integrity of the paper or the conclusions at all, it just makes it a bit more cumbersome to understand because it wasn't immediately clear to me how the choice to represent ice flow history in that way connected with the central conclusions of the paper. This comment could be addressed by modifying the phrase at the end of line 39 to something like "…during the Holocene, in keeping with recent improvements in our understanding of this system." And maybe being clearer in lines 93–97 about how you define "NEGIS basal shear stress" with respect to the area in Figure 1 (i.e., is that the average of the basal shear stress over the whole area in red, yellow and green? Or the entire drainage basin?). Also in figure S8 - why are the dashed lines and the solid lines different before 8ka when presumably the two experiments should diverge, if the only difference is when the basal friction coefficients begin to change (labelled "ts" on the x-axis)? Again as a reader, I have all these questions that come up regarding how you particularly set up this tributary-switching behavior, but then I find it to be not important for the conclusions, so I'm left feeling sort of confused as to why it's there in the first place. Beyond these specific recommendations, I would ask the authors to consider what they want the reader to take away from the manuscript and whether some minor changes in the ordering of figures, or the way they introduce the concept of tributary-switching, might better guide readers towards the core take-away and help the reader avoid confusion.

In any case, I feel that this manuscript is greatly improved and all of my feedback on the earlier version has been thoroughly addressed. I would be happy to see this work published subject to some of the minor revisions suggested below. It is a novel and exciting study that is clearly relevant to our understanding of the connections between ice-margin and ice-interior processes, a topic that impacts our projections for the future of the ice sheet and will be of interest to the broad readership of Nature

Communications.

- -

Abstract – I feel that the abstract is missing a sentence at the end that connects to the wider importance or implications of the work. Is there a sentence you could add that would summarize or anticipate what you have in lines 295-300?

Main – lines 20-21; the use of "high-Arctic" and "high-latitude" here was confusing to me because I don't know if you mean these things interchangeably or if you are intentionally referring to two separate groups of sites – one where the mismatch has been reduced and another where that mismatch has not. Can you clarify this?

Line 30 suggest "basal dynamics are only valid for…"

Line 40 – "high sliding environment" suggest instead "Basal sliding beneath the ice stream is ensuring throughout the Holocene…" or something similar. "High sliding environment" sounds like it means something particular – e.g. what differentiates a "high-sliding environment" from a "low-sliding environment"? If you mean something semi-quantitative or you are doing a comparison here, it would be helpful to define what you mean by "high sliding environment." Otherwise using more generic language like what I suggest might be more clear.

Results

Line 51 – what is meant here by large ice cap in the northeast – can you say that the ice margin in the northeast reached over the continental shelf or do you mean that there was an ice dome/cap in the northeast that was somewhat distinct from the main ice sheet?

Line 59 – I'm confused by the word "shelf" here – this implies you are talking about floating ice, but in the same sentence you clearly refer to retreat of the grounding line – do you mean ice sheet? Or ice margin?

Line 68 – "well-defined, yet bounded" to me these mean the same thing. They are well defined, e.g. they have clear boundaries. What do you mean here?

Line 74 suggest "continues" instead of "persists"

Line 88 "upper range" can you be more clear with what you mean by this? Upper range in terms of they have the most thinning, they are the best fit to the data, or what?

Line 97 "Increases the NGRIP elevation drawdown" confusing because the word "increase" and "drawdown" are separated. "Increases elevation drawdown at NGRIP" is slightly better, but I would prefer "Decreases elevation at NGRIP," "enhances thinning at NGRIP," or something that is uses fewer words.

Lines 119-125 – I think it's a great idea to gesture towards future work that would build on what you've done here, but there is just too much here and it undermines the strength of your study. I would advocate for a single sentence only – i.e. delete from "Also a full-Stokes treatment…at regional/continental scales." I'm agnostic about which specific things you want to recommend for future work, or whether you keep the sentence as-is or make some kind of hybrid sentence, but right now these 6 lines slow down the momentum of the paper at a really inopportune time and it reads as if you are being overly negative about your own work.

Line 134 – some word choice here is confusing. "We find that a faster and higher temperature increase is crucial for the correct retreat." What about something like – "We find that temperature reconstructions that peak earlier (~9ka) and higher (3.5º…) in the northeast sector are crucial for correct…"

Line 142 – suggest "Agassiz ice cap (Fig. 6)"

Line 145 – Where is EGRIP? Could you label it in one of your figures?

Discussion

Line 162 – What do you mean by the Holocene's onset – 11.7 ka or a different definition?

Line 176 suggest "retreat" instead of "regression"

Line 191 suggest "acceleration" instead of "speed up"

Line 203 – What do you mean by "streaming-controlled areas" – places where the ice is streaming, or places like NGRIP where the elevation change is strongly controlled by the dynamics of streaming in the northeast?

Line 209 – delete line break between 209 and 210

Line 221 – "favor soon sliding" – check the wording here, it's not clear what 'soon' in this context means.

Line 221 – should be "leads" rather than "leading"

Line 273 – rather than "8" should be an in-line citation

Line 288 – suggest "compensate for the"

Line 290 – "higher elevation drop" suggest "greater elevation decrease"

Line 291 – suggest "we fail to reproduce"

Methods

Could you briefly describe how the RSL curves are computed somewhere in the methods section? I don't think you need to create a whole separate section but some details on the earth model and approach that was used to calculate the RSL curves in figure 6 would be beneficial. Do these curves include gravitational effects? Or are you taking the output from the simulations in terms of the predicted bedrock elevation and using that as relative sea level?

Figure 4 – label the color bar on the left panel. "Bedrock elevation (m)"

Figure 5 – Make the labels on the colorbar label more regular – e.g. 50, 100, 150…

Figure 6 – I'm curious what the misfit is for the yellow and blue curves for each location – is the blue curve always clearly a better fit? Consider labeling chi-squared values or another quantitative metric you are using to compare with the RSL curves directly on each of the boxes a–k? Visually it looks like the blue curves match the data better and it would be nice to have something quantitative to back this up.

Reviewer #4 (Remarks to the Author):

The Tabone et al submission examines the role of NEGIS (NE Greenland ice stream) activation on inferred ice thinning at NGRIP using the YELMO ice sheet model. As there have been 3 reviews already, from different perspectives, detailing strengths and novelty, I'll focus just on issues I see that need addressing.

Coming from an ice sheet modelling expertise, the core weakness for me is the time-dependent imposition of changes in the basal friction coefficient (Cf) for 3 different NEGIS branches (Fig 1). A dynamically self-consistent demonstration of NEGIS branch switching giving the desired results would be worthy of Nature Comm. publication. But just showing that if you effectively force near shutdown of the NEGIS south branch before 8ka, you can get 100 m or so of Holocene thinning at the geographically proximal NGRIP site, is not that surprising. Furthermore, when I look at eg Fig S5, the thinning starts before the imposed Cf reduction at 8ka, so I'm not clear if this is even necessary, nor exactly what impact it has. Nowhere do you clearly motivate the form nor timing of the imposed time-dependent changes to Cf.

The authors also endeavour to spell out causal chains from marginal retreat to upstream thinning, but they don't provide the detailed evidence to back up their claims (and don't make clear if their claims are hypothesis or based on detailed model analysis). Some thought in to appropriate sensitivity experiments would much more strongly elucidate these causal chains. At the very least, the experimental design and analysis needs to be expanded to include runs with constant basal friction coefficient Cf throughout the run (and for the 3 different values of Cf: fmin, fmid, and 1). This will make much clearer the relative role of stream activation in driving the thinning. I'd be very surprised if these runs weren't already done as a step towards choosing the imposed Cf chronologies. Fig S8 shows some sensitivity tests for the Cf chronology, but they lack the suggested ones that I think would be most enlightening.

More detailed analysis of the ensemble along with appropriate sensitivity experiments would also better support or refute key claims like:
"Here we show that the early grounding-line retreat in northeast Greenland triggers and drives the elevation change at the northern summit."

As detailed below, there are a number of erroneous or misleading
statements that also need to be addressed before this submission would be acceptable for publication.

Lev Tarasov

detailed points

Here we show that the early grounding-line retreat in northeast
Greenland triggers and drives the elevation change at the northern
summit. Fast ice-stream flow caused by reduced basal shear stress
following the northeast retreat explains 55% (± 18%) of the estimated
ice thinning, showing that ice-stream dynamics is one of the main
drivers of the NGRIP Holocene surface elevation drop.

This is misleading, given the missing caveats in the above claims.
More accurately you show that the forced time-dependent imposition of
reduced basal friction coefficient for the southern NEGIS branch
(whose upstream onset is geographically proximal to NGRIP), can reduce
NGRIP elevation. Nowhere do you show that "early grounding-line
retreat in northeast Greenland triggers and drives the elevation
change at the northern summit."

high-latitude ice-elevation changes are mainly driven by the
22 Arctic amplification6 and and glaciostatic effects
Changes in precipitation could also play a key role. Furthermore,
I see nowhere in the cited #6 (Buizert et al), the above claim,
only the statement that there is arctic amplification in the
temperature response to orbital forcing. Buizert et al, provide
no analysis of changes in precipitation or critical self-evaluation
of what aspects of the TRACE results have high confidence given
the coarse resolution (T32) driven by an ice sheet chronology
(ICE-5G) that had a significantly higher Keewatin ice dome than
current reconstructions which potentially significant consequences
on atmospheric circulation.

#
surface-velocity assimilation for basal shear stress estimation
30 yields the most accurate basal friction approximation.
only where the bed is not currently frozen
And that assimilation depends on the assumed temperature profile
of the ice or otherwise ignores the vertical shear deformation of
the ice.

The accurate timing of the deglaciation is captured by using high-temporal
38 resolution climate reconstructions6 to force the model.
high-temporal resolution doesn't imply accurate timing, especially given
the limitations of the TRACE chronology used in reference 6 as described
above

We characterise the past northeast fast flow regime as a system of a

39 northern, now-extinct paleo ice stream and a southern
present-day-like ice stream that activated during the Holocene8
(Fig. 1).
Need to make clear that you are forcing by hand the reduction in
basal friction that enables the streaming.

Figure 1. Switch of NEGIS branches throughout the Holocene. The
friction coefficient cf reduced below the paleo an
the caption needs to make explicitly clear that the first frame is
the imposed basal drag chronology As a start, the caption title
should be "Imposed changes to basal friction coefficient Cf ..."

Fig 1:
No justification for the ramped decreased over 8 kyr is
provided. The sensitivity tests (Fig S8) partly address this by
testing a 4.5 kyr interval of the ramp, but a more confident
sensitivity test to bound uncertainties would take one bounding Cf
chronology with an fmin constant value throughout

Such a far-inland reach of dynamic thinning in northeast Greenland 46
during a major retreat phase suggests that such processes may also
amplify the response of this sector to future warming and ice 47 loss.
so why did it shutdown?

Ice flow propagates upstream in response to thermo-mechanical and geometrical
70 changes occurring during the northeast retreat of the marine-based ice sheet
need to be explicit: in response to imposed cf(x,y,t) basal friction
coefficient changes

(Fig. 3f). Then, velocity propagation associated with the inland
expansion of the paleo-NEGIS induces dynamic thinning of ice at the
NGRIP site
#
And again, you should explicitly add "imposed" to "inland expansion.."

Margin thinning and retreat, driven by early Holocene temperature rises, induces ice thinning at higher elevations, reducing effective pressure, increasing basal sliding, and fostering ice acceleration and further margin retreat. Primarily, dynamic thinning triggers reduced basal friction during the retreat. Still, basal sliding at the stream increases considerably only with a significant basal water increase from enhanced basal frictional heating (Fig. S12). Reduced effective pressure and basal friction are associated with increased basal velocity at the stream region, allowing upstream paleo-NEGIS propagation. The NGRIP dynamic thinning (and surface elevation drop) is therefore the indirect response to margin perturbations via geometry and subglacial water system alteration
Given what is currently presented, I see clear support for the last sentence. The
whole causal chain could be tested with some appropriate sensitivity experiments.

Figure 2. Greenland ice dynamics during the last deglaciation
this figure should include the basal friction coefficient (cf(x,y,t)) masks from fig 1 to
make clear how much of the ice stream extent is driven by the imposed cf chronology.

No similar correlation is found for any other GrIS basins in our 99
experiments (Fig. S3), indicating that there is no other catchment
where ice flow plays a significant role in the NGRIP thinning.
But this is effectively what you've put into the model in your
figure 1, you've only imposed the basal drag reduction in the NEGIS
basin and since you don't show what the "other basins" are, I have
no way to evaluate

And I'm confused by S3 :
Holocene-averaged NGRIP thinning rates as a function of the mean-NEGIS basal shear
stress for different GrIS basins
I don't know how to interpet this given that the basins aren't shown in this paper.

The bed-elevation dependent basal friction coefficient representing
bed roughness at the NEGIS strongly controls the 101 ice-stream basal
conditions, hence the NGRIP thinning (Fig. S4)
So what would cause basal roughness to change over the course of a
few kyr as you impose (fig 1)?

Figure 6. Northeast Greenland Holocene Relative Sea Level
change. Relative Sea Level (RSL) curves for Northeast Greenland
modelled by Yelmo, forced by two different air temperature
reconstructions, and compared to RSL data15, 16, 69, 70
#
In our 102 simulations, the progressive upstream decrease in basal
traction during the ice-margin retreat stems from a decline in the
basal 103 effective pressure due to increased meltwater production
Given that Cf is linearly decreased after 8ka, and that it's not
clear what time interval you are talking about here, how am I
supposed to judge this "progressive upstream decrease" is not solely
due to the imposed Cf decrease?

We find that the northern summit Holocene thinning rate 96 is well
correlated with the NEGIS basal shear stress (R=0.59; Fig. 5b, c),
with the strongest dependence for the early-mid 97 Holocene interval
(Fig. S2).
Fig S2 shows a weaker dependence (nominal R=0.54) not stronger.

The internal switch applied at the mid Holocene (Figure 1) and the
time dependent variables in the friction law 112 (effective pressure
and scaling parameter depending on bedrock elevation)
should state here where details are

complex unsteady dynamic state of the 114 ice stream due to continuous
fluctuations...or variations in the ice viscosity due to changes in
the ice 115 crystal fabric and in the thermal state of the ice
column24

isn't this thermomechanically coupled to compute the thermal state
and it's impact on ice viscosity?

Although our approach approximates the past activation/deactivation of
the northeast ice stream through an a-priori 119 relocation of the
paleo ice stream to its present position, the root causes of such an
internal switch are not investigated. The 120 inclusion of physical
processes important to describe ice-stream motion such as non-local
basal water routing10, and others 121 at smaller scales of more recent
attention, such as spatial variability in ice anisotropy along the ice
stream17 and localised 122 ice deformation arising from realistic
topography roughness25, would permit a better investigation of the
complex NEGIS 123 dynamics in the future
Should explicitly state if there any published evidence to suggest
that these listed processes would operate on the required timescale
and have enough impact to induce such strong switching?
The above cited reference 10 for basal water routing, for instance,
does not show any result suggesting that basal water routing modelling
could drive such a multi-millenial switching timescale. Nor does
ref 17 suggest have any mention of ice anisotropy in relation to
stream activation. Later on you do provide some relevant discussion,
so you should clearly point to it from here.

a-priori 119 relocation
"a-priori" is confusing -> imposed

Computing RSL curves with a simple GIA model is justified as RSL data
in northeast Greenland are unlikely to be 138 affected by the
deglaciation of the North American Ice Sheet5
Though some papers in the past have show limited sensitivity of GRIS
evolution to the simplistic plate lithosphere, relaxing bed GIA
model used here no one has ever claimed that meaningful RSL curves
could be computed by such a model. Furthermore, the geoidal
perturbations have much farther reach than the bed
perturbations. From what I can tell, you complete ignore this key
part of RSL, so your curves have little interpretable value
and are not worth comparing to RSL proxy data.

Runs with an extremely low oceanic forcing ($\kappa \sim 0$) still simulate a
retreat from the continental shelf, 155 potentially causing rapid
thinning at NGRIP (Fig S4). This implies that initial deglaciation in
the northeast was likely sparked 156 by atmospheric warming
What your model needs and what likely actually happened are
not necessarily the same thing.

where cf, a uniform coefficient representing the sliding capacity of
the bed, is decreased through a factor λ depending 314 exponentially
on the bedrock elevation (zb)56, as: eq (3)
wording is confusing (cf is not decreased by lambda, Cb is), but more to point
you need to motivate this choice of relationship especially since you previous state:

The bed-elevation dependent basal friction coefficient representing bed roughness at
You need to spell out reasoning for correlating bed roughness to bed-elevation

τa is the atmosphere transmissivity
how is this computed or set?

The ice sheet is allowed to freely evolve under LGM (20kyr ago)
404 conditions for 50 kyr to reach the thermomechanical equilibrium.
what basis is there to assume thermodynamic equilibrium?
why not spend 50 kyr of model time imposing transient climate and sealevel forcing?

based on paleo and present model-data misfits and then aggregate them
into a single total score.
what about accounting for data and structural uncertainties of the
model?

Each misfit Mi, j, where i is the data class and j is the ensemble
run, is then interpreted as a score Si, j following the 439
equation66:
eq 11 is not appropriate for a Gaussian error model contrary to your earlier claim:
simple averaging skill-score method based on a Gaussian error distribution
You would need to use the mean squared error, not root mean squared error as for M_ij

which is further normalized between 1 (best run) and 0 (worst run).
SO what does say 0.1 mean? total garbage, or bit likely or ?

To further reduce the uncertainty in our model results, we add another
constraint that distinctly separates plausible from 446 implausible
model representations within the ensemble. Following this approach,
runs are considered valid as long as the 447 absolute elevation
difference between modelled and observed elevations is less than 50
metres (e.g., NGRIPPD<50m) so that 448 563 simulations over 3000 are
retained as valid.
How was 50 m chosen? Why not 45 m, or 100 m? And why is this the
only data constraint deemed worth a run rejection?

the law for till hydrology54
this relationship is not a "law".

Fig S8, frame one, given that colour is used, please provide a higher
contrast between Cntrl and Case 3, I can't discern the difference
between the corresponding thick/thin black lines in the plot.

Fig 4: how does one "observe" a pre-historical era deglaciation time?
Figure caption should be "inferred deglaciation time"

Fig S4: please either (prefered) state what the parameters are ('q' means nothing) or
indicate where a table of YELMO parameters is in the paper (I couldn't find one,
just a list embedded in text..)

Fig S5; include time ticks in each frame, and add grid lines so that one can
easily read off the timing of each the thinning onset

Fig S6: Individual scores versus mean-Holocene NEGIS basal shear stresses for best runs
should also include scores of rest of 3000 member ensemble in say a transparent grey
underprint to convey what range your whole ensemble explored.

Fig S12:
All variables are computed on a location in the centre
of the present NEGIS.
where exactly?

Fig S12. Glaciological explanation of the effect of margin retreat on
dynamical thinning in the northeast.
You are expecting a lot of many readers to interpret this
"Glaciological explanation" with very limited guidance (only
reference in text is to the role of enhance basal meltwater
production

Reviewer #2 (Remarks to the Author):

This is a substantially revised version of a paper previously submitted to the journal. It is one of the most comprehensive efforts in the addressing of reviewers' comments that I have seen and for this I commend the authors. The paper is investigating the causes of the lowering of the ice divide at NGRIP in the early Holocene as identified in ice core based reconstructions. Numerical models have failed to reproduce this thinning leading to uncertainties on the veracity of either the models or the ice core reconstructions. The results from this study have linked the presence of a “prescribed” larger (than present) northern component of NEGIS which can facilitate drawdown in the ice divide. The modelling results are well validated against palaeo-ice sheet data from both marine and terrestrial environments. It will be of interest to the ice sheet community as a whole and the wider palaeoclimate community.

Overall it is a robust attempt to address how the retreat of the ice margin in the NE sector of the GriS initiates multiple ice streams (the “remnants” of which are still present in the ice sheet) which induce an inboard thinning and speed up that ultimately leads to drawdown of the ice surface at the ice core site of NGRIP. It brings modelling outputs and ice core reconstructions closer together, though it does not resolve the difference between them completely. Validation of the model outputs against the empirical data shows that the best results capture the overall pattern very well, though some mismatches in timing and magnitude remain. This robust grounding of the modelling in comparison to multiple empirical palaeo-datasets provides some confidence that is it identifying the key controls in the drawdown of ice at NGRIP. There is a rather long final section which discusses the mismatches between the model and reconstructed data which is very honest but is perhaps a little negative overall but as above it does provide confidence in the results and their meaning. It also points towards multiple developments in the modelling which might improve the results further.

I have no major substantive issues with the paper this time around but have provided numerous comments on the manuscript and supplementary material both of which I have uploaded with this review. These really need to be addressed as there are many minor errors/things missing throughout the text and in the figures and statements/sentences/sections which are unclear in the way that they are written. As it stands, they detract significantly from the readability and message that the manuscript is trying to convey.

In conclusion: this paper addresses a significant issue as it is trying to resolve a mismatch between a data-based reconstructed numerical model output. It is robust both from the modelling perspective and how the modelling results are assessed using empirical palaeo-datasets linked to the evolving ice sheet geometry. From this perspective I think therefore that it will be looked upon favourably by a number of academic audiences. So I think that this is, in principle, acceptable for publication pending the numerous corrections that are required to the text and the figures.

We thank the reviewer for their positive comments. We have now addressed all the remaining issues they have pointed out. We hope the final version of the manuscript will be positively received.

We report here the main changes we made, point by point, also according to the other reviewers’ requests. Please note that the figure numbers refer to the current submission of the manuscript, unless stated differently.

Line 40: changed to “Basal lubrication is promoted by reducing the bed-dependent basal friction coefficient and by imposing sufficiently high GHF which results in high sliding rates for the ice stream system throughout the Holocene”.

Line 42-45: now changed to “We find that the NGRIP thinning is initiated by the NEGIS onset and upstream propagation triggered by the early Holocene ice-sheet retreat in northeast Greenland and supported by increased meltwater production and reduced basal friction. The far inland development of the present-like NEGIS during the last 8 kyr, here fostered by imposing a time-dependent reduced basal friction coefficient along the ice stream, drives the dynamic thinning throughout the Holocene, explaining at least ~100 m of the surface elevation drop in north-central Greenland. ".

Line 50-57: a zoomed-in section of the northeast region of the ice sheet showing surface velocities as in Figure 1 (previously Fig. 2) is now added to the Supplementary Material (Fig. S2).

Line 99: we now introduced a map with the catchments in Fig. S7.

Line 107: changed to "The high basal melting rates modelled at the same region are also in agreement with recent work".

Line 113-115: This sentence was also commented on by Reviewer#4 and we acknowledge it was unclear. Here we wanted to emphasise that a more complex representation of the basal hydrological system feeding back to the basal sliding law and a higher spatial model resolution would certainly improve the representation of the complex dynamic state of the ice stream. As pointed out by the other reviewer, the model is thermomechanically coupled so that variations in the ice viscosity due to changes in the thermal state are actually already taken into account.

We now modified this sentence to "However, these approximations, the coarse model resolution and the simple hydrology model here employed are otherwise insufficient to reproduce the complex unsteady dynamic state of the ice stream due to continuous fluctuations in the basal water system."

Line 115: modified to "Such complex behaviour of the NEGIS observed at the present might still reflect the response to large geometry adjustments that occurred during the last deglaciation, ...".

Line 158: modified it to "Sensitivity tests using different climatologies highlight a primary control of atmospheric temperatures on inner shelf recession timing (Fig. 3, e.g. Sondre Mellemland, Lambert Land)". The deglaciation timings for these two locations in the inner shelf are in fact very much dependent on the paleo atmospheric temperature applied to the ice-sheet model.

Line 167-168: modified to "In the case of ZI, air temperature warming decades ago reduced sea ice, promoting calving and floating tongue collapse, decreasing buttressing on the glacier."

Line 199: modified to "Ice anisotropy, controlled by ice temperature and stress variations, exerts a major control on the NGRIP dropdown during the Holocene (Figure S8)."

Line 205: modified to "Although runs with slower ice flow show greater accuracy in predicting ice thickness within the northeast catchment, they simulate too much ice in the ice sheet interior at the present (Fig. S10)."

Figure 1: yes, modified to "the paleo-NEGIS is assumed to be composed of the northern, central and southern branch." . This figure is now Fig. S1.

Figure 2: yes, black lines are grounding lines, therefore velocities outside them are ice-shelf velocities. This has been added to the caption "Black lines represent the grounding line and velocities outside these represent ice shelf velocities.". This figure is now Figure 1.

Figure 4: added the title of the legend to the left panel (Bedrock elevation (m)). This figure is now Figure 3.

Figure 5: Now Figure 4. Added a), b) and c) to the panels. Also, we modified part of the caption to " NGRIP thinning rates (b) and final scores, where 1 represents the best run (c), for each model run as a function of the mean-NEGIS basal shear stress. "

Figure S3: now Fig. S7. We added a panel with a map showing the basins considered for the analysis. We Adjusted the units for the fourth plot and we changed the last sentence to “Note that the northeast basin (in blue) does not include the paleo NEGIS catchment (in yellow).”.

Figure S4: Now Fig. S8. We added a table (Table 1) in the supplementary material showing symbols, units, range of values and description of the model parameters perturbed in the ensemble.

Figure S6: Now Fig. S10. Caption changed to “Individual scores for key parameters versus mean-Holocene NEGIS basal shear stresses for all runs”.

Figure S7: Now Fig. S14. Added to the caption “reconstructions from Vinther et al., 2009 (~12-0 kyr ago) and Lecavalier et al., 2013 (8-0 kyr ago) are shown with their mean elevations (thick and dashed green lines, respectively) and 1-σ uncertainties (darker and lighter shaded green, respectively).”

Figure S9: Now Fig. S15. Caption changed to “Surface elevation (upper panel), bedrock elevation anomalies (middle panel) and ice thickness anomalies (lower panel) with respect to the present modelled at the NGRIP site for the last 15 kyr by our simulation ensemble. Thick blue lines represent the score-weighted ensemble mean, while shadow areas represent its 1-σ uncertainties.”

References:

Vinther et al., 2009, Holocene thinning of the greenland ice sheet. Nature, 46, 385–388.

Lecavalier et al., 2013, Revised estimates of greenland ice sheet thinning histories based on ice-core records. Quat. Sci. Rev. 63, 73–82.

Reviewer #3 (Remarks to the Author):

The manuscript by Tabone et al. has been thoroughly revised based on feedback from three reviewers, and through thousands of new simulations ran with an improved version of their ice model. I am satisfied with the author's responses to my comments on a previous version of the manuscript – and in particular I think their efforts to reframe the implications of their findings to be more specific to the northeast sector of the ice sheet have made their conclusions more impactful. All my major concerns with the previous version of the manuscript have been fully addressed in the new version. I think this manuscript advances our understanding of the history of the Greenland ice sheet by illuminating clear links between ice-sheet dynamics, climate forcing, and deglaciation process that are relevant to projections of Greenland's future.

By expanding their ensemble to include other parameters of interest and many more simulations, they have shown that the ice-flow in northeast Greenland plays a (potentially large) role in ice-surface elevation drop in central Greenland – this is now an even more robust conclusion. They have now validated their simulations against an even greater variety and number of proxy data types/records. I have minor suggestions below, which mostly have to do with the wording of certain phrases, that I think would make some of the arguments clearer. Given the enormous amount of work the authors have done to strengthen the manuscript based on the past round of reviews, I think it should be published with minor revisions.

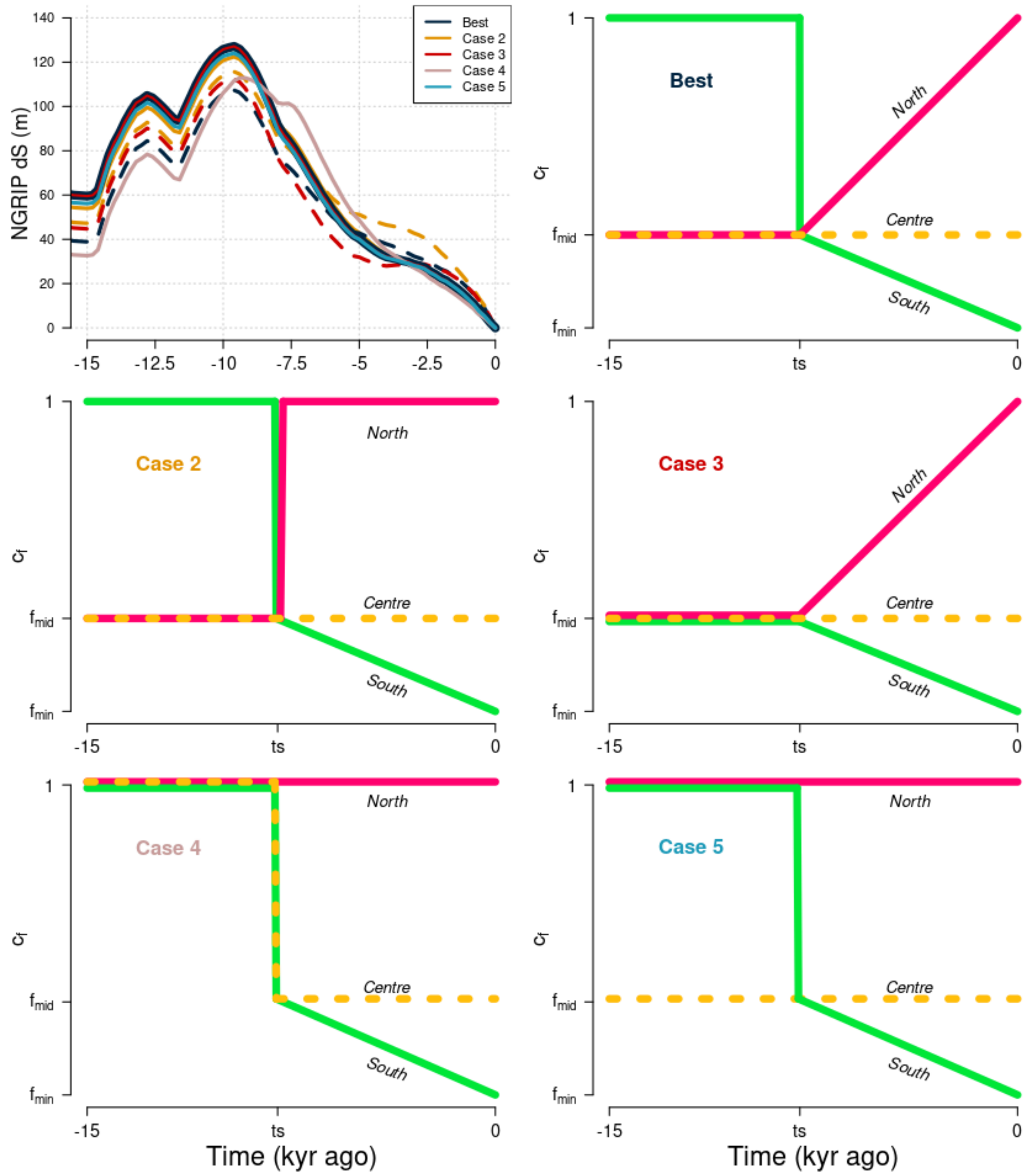
We thank the reviewer for their kind comments and we are grateful that our additional work could be so positively received. We address below the remaining concerns, point by point. Please, note that the figure numbers refer to the new submission of the manuscript, unless stated differently.

One thing that strikes me is that there is quite a bit of text, and a new figure, about the switching of NEGIS tributaries at ~8ka. I am not sure how this relates to the central argument of the paper. If your conclusions are largely the same without imposing this switch in the ice stream tributaries, why is it important that you include this in the model set-up? I think I am somewhat biased here because I have read a previous version of this paper that largely had similar results, so my immediate questions are 1) why is this included and 2) does it impact the results? After spending time with the paper I think that the answers to these questions are 1) because our understanding of the ice stream history has changed in the last couple of years and this "tributary-switching" is now the most consistent with that understanding (although see lines 118-125… there is a lot about this approach that could (and should in future work) be altered or tested) and 2) it actually doesn't seem to impact the results in a meaningful way. Obviously the representation of present-day ice flow velocities is improved but in terms of the central argument of the paper I'm just not certain what the connection is and I find it kind of distracting. It took me a while to understand that this was not an experiment that was taking place as part of the ensemble but was rather a new way to set up the model and run all of the experiments, and it didn't have much of an impact on the overall conclusions, even if the details of the ice dynamics/ice history are of course altered by this approach to basal sliding in the northeast. I don't feel strongly that this undermines the integrity of the paper or the conclusions at all, it just makes it a bit more cumbersome to understand because it wasn't immediately clear to me how the choice to represent ice flow history in that way connected with the central conclusions of the paper. This comment could be addressed by modifying the phrase at the end of line 39 to something like "…during the Holocene, in keeping with recent improvements in our understanding of this system."

We thank the reviewer for their valuable point of view regarding the inclusion of a switch in the ice stream tributaries in our revised version of the manuscript. We will address their concerns, also inviting them to follow the discussion with Reviewer#4, where we extensively explain our choice through new sensitivity simulations and plots.

The question "why is the switch included?" is somehow already answered by the reviewer. Indeed, we did decide to include an additional branch, imposing a switch between the catchments, to follow the new findings from Franke et al., 2022. We consider this discovery to be a significant advancement in our comprehension of the past behaviour of the NEGIS. As such, we believe it essential to incorporate this finding into our methodology to ensure that our work remains current and aligned with the latest advancements in the field. As correctly understood by the reviewer, the whole ensemble was redone with this new setup, as shown in Figure S1 (previously Fig. 1).

The question “does this switch impact the results?” is also somehow answered by the reviewer themself. We agree with them that the inclusion of a northern branch active for part of the Holocene and then shut down does not impact our results meaningfully. To demonstrate this we have done several additional sensitivity tests. First, we added two cases to Fig. S16 (previously Fig. S8) to show that the northern branch can be kept inactive for the whole simulation and still produce a comparable response at NGRIP during the Holocene. Below we show the updated Fig. S16:



By comparing the control run to Case 5, it can be noted that the presence or not of a northern branch during the first part of the Holocene doesn’t impact the surface elevation drop modelled at NGRIP (the difference in the elevation drop is only of a few metres). This means that the dynamic response of the NGRIP site to the NEGIS formation upon margin retreat is independent from the existence of a northern paleo branch. Still, as discussed above, we decided to include it anyway in our methods to follow the new findings of Franke et al., 2022. However, to avoid centering the discussion on the switch between branches we decided to shift previous Figure 1 (Imposed changes to basal friction coefficient cf beneath the NEGIS branches throughout the Holocene) to the Supplementary Material (now Fig. S1).

Also, the timing of the switch is not crucial in our results. In fact, decreasing the basal friction coefficient cf at 8 kyr or at 4.5 kyr ago in the southern branch does not significantly impact the NGRIP response in terms of elevation change. This can be observed by comparing solid to dashed lines for every case. Still, such a switch, imposed in our simulations by decreasing cf for the last part of the Holocene, ensures increasing ice stream velocities until the end of the run, even though the ice sheet re-advances during the Neoglacial. That explains why the present-day ice velocity at the NEGIS southern branch has improved compared to the previous submission. Still, this reduction in cf applied at the late Holocene is minimal as it only accounts for ~13% of its initial value f_mid (see comments to Reviewer#4).

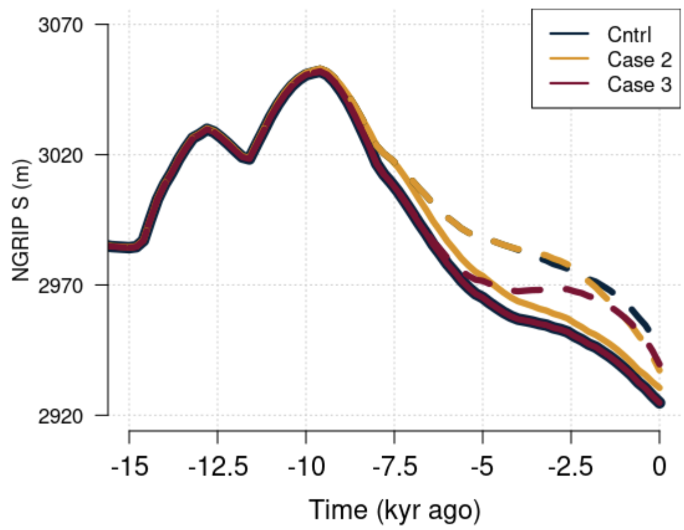
Also, we modified the last sentence as "We characterise the past northeast fast flow regime as a system of a northern, now-extinct paleo ice stream and a southern present-day-like ice stream that activated during the Holocene by imposing a time-dependent basal friction change below the NEGIS (Fig. S1). Such a stream configuration is in line with recent improvements in our understanding of this system.".

And maybe being clearer in lines 93–97 about how you define "NEGIS basal shear stress" with respect to the area in Figure 1 (i.e., is that the average of the basal shear stress over the whole area in red, yellow and green? Or the entire drainage basin?).

Yes, it is calculated as the average of the basal shear stress over the whole area in red, yellow and green. We now made it clearer in the manuscript.

Also in figure S8 - why are the dashed lines and the solid lines different before 8ka when presumably the two experiments should diverge, if the only difference is when the basal friction coefficients begin to change (labelled "ts" on the x-axis)?

The dashed and solid lines appear to diverge before "ts" because we are showing surface elevation anomalies with respect to the present. We show below the same plot, but for absolute surface elevations. Here it can be noted that runs with different "switching times" start to diverge after 8 kyr, as expected.



Again as a reader, I have all these questions that come up regarding how you particularly set up this tributary-switching behavior, but then I find it to be not important for the conclusions, so I'm left feeling sort of confused as to why it's there in the first place. Beyond these specific recommendations, I would ask the authors to consider what they want the reader to take away from the manuscript and whether some minor changes in the ordering of figures, or the way they introduce the concept of tributary-switching, might better guide readers towards the core take-away and help the reader avoid confusion.

We agree that the central conclusion of the paper might be blurred by such a complex setup. We therefore decided to limit the discussion about the switch between branches to the minimum, and focus more on the glaciological processes that occur during the northeast retreat, independently from the form and timing of such a switch. To this aim, we also shifted Figure 1 to the Supplementary Material (now Fig. S1). A large discussion on the impacts that this has on the NGRIP dropdown is anyway backed up by numerous figures and descriptions in the Supplem. Material.

In any case, I feel that this manuscript is greatly improved and all of my feedback on the earlier version has been thoroughly addressed. I would be happy to see this work published subject to some of the minor revisions suggested below. It is a novel and exciting study that is clearly relevant to our understanding of the connections between ice-margin and ice-interior processes, a topic that impacts our projections for the future of the ice sheet and will be of interest to the broad readership of Nature Communications.

We appreciate the reviewer's positive summary.

Abstract – I feel that the abstract is missing a sentence at the end that connects to the wider importance or implications of the work. Is there a sentence you could add that would summarize or anticipate what you have in lines 295-300?

Added "Our findings show that the ice-flow in northeast Greenland plays a large role in ice-surface elevation drop in central Greenland."

Main – lines 20-21; the use of "high-Arctic" and "high-latitude" here was confusing to me because I don't know if you mean these things interchangeably or if you are intentionally referring to two separate groups of sites – one where the mismatch has been reduced and another where that mismatch has not. Can you clarify this?

They are used as synonyms here. We modified the sentence to "Nevertheless, these features cannot explain the reconstructed thinning at ice-core sites at lower latitudes, as high-Arctic ice-elevation changes are mainly driven by the polar amplification and glaciostatic effects resulting from the demise of the nearby North American ice sheets and changes in precipitation."

Line 30 suggest "basal dynamics are only valid for…"

Changed accordingly.

Line 40 – "high sliding environment" suggests instead "Basal sliding beneath the ice stream is ensured throughout the Holocene…" or something similar. "High sliding environment" sounds like it means something particular – e.g. what differentiates a "high-sliding environment" from a "low-sliding environment"? If you mean something semi-quantitative or you are doing a comparison here, it would be helpful to define what you mean by "high sliding environment." Otherwise using more generic language like what I suggest might be more clear.

Changed to "Basal lubrication is promoted by reducing the bed-dependent basal friction coefficient and by imposing sufficiently high GHF which results in high sliding rates for the ice stream system throughout the Holocene. " following Reviewer#2.

Results
Line 51 – what is meant here by large ice cap in the northeast – can you say that the ice margin in the northeast reached over the continental shelf or do you mean that there was an ice dome/cap in the northeast that was somewhat distinct from the main ice sheet?

We mean that the ice margin in the northeast extended over the continental shelf. Changed to “At the end of the Last Glacial Maximum (LGM), the grounded ice sheet extended over the continental shelf, in agreement with reconstructions”.

Line 59 – I’m confused by the word “shelf” here – this implies you are talking about floating ice, but in the same sentence you clearly refer to retreat of the grounding line – do you mean ice sheet? Or ice margin?

We mean ice margin. Changed to “In our simulations, the NEGIS ice front retreats from the outer to the inner coast between 11.7 and 10.2 kyr ago…”

Line 68 – “well-defined, yet bounded” to me these mean the same thing. They are well defined, e.g. they have clear boundaries. What do you mean here?

Removed “yet bounded”.

Line 74 suggest “continues” instead of “persists”

Changed accordingly.

Line 88 “upper range” can you be more clear with what you mean by this? Upper range in terms of they have the most thinning, they are the best fit to the data, or what?

We are referring to those runs which present the highest thinning within the best runs of the ensemble. Changed to “Yet, the model scenario showing the highest thinning within the score-weighted 1-σ uncertainty shows a maximum elevation drop of 139 m, indicating that 73% of the total estimated thinning is reproduced. ”.

Line 97 “Increases the NGRIP elevation drawdown” confusing because the word “increase” and “drawdown” are separated. “Increases elevation drawdown at NGRIP” is slightly better, but I would prefer “Decreases elevation at NGRIP,” “enhances thinning at NGRIP,” or something that uses fewer words.

Changed to “enhances thinning at NGRIP”.

Lines 119-125 – I think it’s a great idea to gesture towards future work that would build on what you’ve done here, but there is just too much here and it undermines the strength of your study. I would advocate for a single sentence only – i.e. delete from “Also a full-Stokes treatment…at regional/continental scales.” I’m agnostic about which specific things you want to recommend for future work, or whether you keep the sentence as-is or make some kind of hybrid sentence, but right now these 6 lines slow down the momentum of the paper at a really inopportune time and it reads as if you are being overly negative about your own work.

This point was also raised by Reviewer #4. Since we address the limitations of our approach further in the discussion, we decided to remove most of this paragraph to avoid an overly negative self-criticism about our work. The paragraph now reads: “The inclusion of physical processes important for describing present-day ice-stream behaviour, such as non-local basal water routing could allow for a better investigation of the complex NEGIS dynamics in the future. Such complex behaviour of the NEGIS observed at the present might still reflect the response to large geometry adjustments that occurred during the last deglaciation, likely associated with a substantial change in the streaming activity in northeast Greenland during the Holocene”. The sentence “Although our approach approximates the past activation/deactivation of the northeast ice stream through an imposed relocation of the paleo ice stream to its present position, the root causes of such an internal switch are not investigated.” is now added to the Method section.

Line 134 – some word choice here is confusing. “We find that a faster and higher temperature increase is crucial for the correct retreat.” What about something like – “We find that temperature reconstructions that peak earlier (~9ka) and higher (3.5º…) in the northeast sector are crucial for correct…”

Changed accordingly.

Line 142 – suggest “Agassiz ice cap (Fig. 6)”

Changed as “Agassiz ice cap (Fig. 5).”

Line 145 – Where is EGRIP? Could you label it in one of your figures?

Added point for EGRIP in Fig. S11.

Discussion

Line 162 – What do you mean by the Holocene’s onset – 11.7 ka or a different definition?

This has been changed to “Abundant evidence from sediment core analysis supports the presence of Atlantic Waters (AW) on the outer continental shelf as early as ~13.4 kyr ago (Hansen et al., 2022, Davies et al., 2022) and on the inner shelf during the early Holocene (~11 kyr ago - Lloyd et al., 2023, Syring et al., 2020). This hints at the potentially significant role of warmer ocean temperatures in the disintegration of the northeast ice sheet’s floating margin and grounding line migration during the last deglaciation.”.

Line 176 suggest “retreat” instead of “regression”

Changed accordingly.

Line 191 suggest “acceleration” instead of “speed up”

Changed accordingly.

Line 203 – What do you mean by “streaming-controlled areas” – places where the ice is streaming, or places like NGRIP where the elevation change is strongly controlled by the dynamics of streaming in the northeast?

We mean regions where the ice is streaming. Changed to “Still, our simulations show higher ice viscosity for ice stream regions. ”

Line 209 – delete line break between 209 and 210

Done.

Line 221 – “favor soon sliding” – check the wording here, it’s not clear what ‘soon’ in this context means.

Changed to “earlier”.

Line 221 – should be “leads” rather than “leading”

Changed accordingly.

Line 273 – rather than “8” should be an in-line citation

Done.

Line 288 – suggest “compensate for the”

Done.

Line 290 – “higher elevation drop” suggest “greater elevation decrease”

Changed accordingly.

Line 291 – suggest “we fail to reproduce”

Changed accordingly.

Methods

Could you briefly describe how the RSL curves are computed somewhere in the methods section? I don’t think you need to create a whole separate section but some details on the earth model and approach that was used to calculate the RSL curves in figure 6 would be beneficial. Do these curves include gravitational effects? Or are you taking the output from the simulations in terms of the predicted bedrock elevation and using that as relative sea level?

We calculate the RSL curves by subtracting the modelled bedrock elevation anomaly (with respect to its present elevation) from the global sea-level signal relative to present (from Waelbroeck et al., 2002). Changes in bedrock elevations are computed by the ELRA (Elastic Lithosphere Relaxing Asthenosphere) model. We do know that this is a crude approximation, as we do not account for 1) gravitational effects between the ice sheet and the sea level, which affect the shape of the geoid at the marine margin of the ice sheet, thus the local sea-level height, 2) the solid Earth response due to non-local ice loading/unloading and 3) perturbation in the geoid due to far reaching changes in mass changes. However, we do not seek here either to provide a new estimate of the RSL change around the northeast margin or to validate our model against RSL data. Rather we want to use the RSL reconstructions only as an additional proxy to investigate the deglaciation timings in the northeast modelled by our ice-sheet model as forced by two different climatic forcings. Figure 6 is therefore used for this scope, i.e. evaluating the performance of the two climatologies in shaping the deglaciation chronology. Please, follow the discussion with Reviewer #4 for further details.

We modified the Method section by adding this paragraph: “Bedrock elevation changes are computed via the Elastic Lithosphere Relaxing Asthenosphere (ELRA) scheme. This simple but effective glacial isostatic adjustment (GIA) scheme assumes viscoelastic deformation of the crust from the elastic behaviour of the lithosphere and the ability of the asthenosphere to relax upon ice load changes […]. The modelled bedrock elevation change with respect to the present is used to calculate the RSL curves of Figure 5 by subtracting the glaciostatic effect from the mean global sea-level signal from Waelbroeck et al., 2002, which is also imposed as a forcing to the ice-sheet model. As mentioned in the main text, such curves solely reflect the local deformation of the solid Earth, omitting a portion of the signal associated with sea-level spatial variability arising from both local and non-local perturbations in the geoid, and the response of solid Earth deformation to far reaching mass changes. Consequently, our modelled RSL curves should be regarded solely as proxies for local variations in the ice load of Greenland.”

Figure 4 – label the color bar on the left panel. “Bedrock elevation (m)”

Changed accordingly.

Figure 5 – Make the labels on the colorbar label more regular – e.g. 50, 100, 150…

Changed accordingly.

Figure 6 – I'm curious what the misfit is for the yellow and blue curves for each location – is the blue curve always clearly a better fit? Consider labeling chi-squared values or another quantitative metric you are using to compare with the RSL curves directly on each of the boxes a–k? Visually it looks like the blue curves match the data better and it would be nice to have something quantitative to back this up.

We have now calculated the root mean square error between the observed and the modelled RSL for the two curves. The values are shown now for each panel for both the climatic data assimilation scenario (yellow numbers) and those reconstructed from climate modelling and adjusted with ice core data (blue numbers). The RMSEs clearly show the better performance of the reconstructed climatology adjusted with ice core data compared to the other.

References:

Franke et al., 2022, Holocene ice-stream shutdown and drainage basin reconfiguration in northeast greenland. Nat. Geosci. 15, 995–1001.

Davies et al. 2022, "Linkages between ocean circulation and the northeast greenland ice stream in the early holocene". Quat. Sci. Rev. 286, 107530.

Hansen et al. 2022, Deglacial to mid holocene environmental conditions on the northeastern greenland shelf, western fram strait. Quat. Sci. Rev. 293, 107704.

Lloyd et al. 2023, Ice-ocean interactions at the northeast greenland ice stream (negis) over the past 11,000 years. Quat. Sci. Rev. 308, 108068.

Syring et al., Holocene interactions between glacier retreat, sea ice formation, and atlantic water advection at the inner northeast greenland continental shelf. Paleoceanogr. Paleoclimatology 35, e2020PA004019.

Waelbroeck et al., 2002, "Sea-level and deep water temperature changes derived from benthic foraminifera isotopic records." Quaternary science reviews 21.1-3: 295-305.

Reviewer #4 (Remarks to the Author):

The Tabone et al submission examines the role of NEGIS (NE Greenland ice stream) activation on inferred ice thinning at NGRIP using the YELMO ice sheet model. As there have been 3 reviews already, from different perspectives, detailing strengths and novelty, I'll focus just on issues I see that need addressing.
Coming from an ice sheet modelling expertise, the core weakness for me is the time-dependent imposition of changes in the basal friction coefficient (Cf) for 3 different NEGIS branches (Fig 1). A dynamically self-consistent demonstration of NEGIS branch switching giving the desired results would be worthy of Nature Comm. publication.

We thank Lev Tarasov for his comments. We agree that a "dynamically self-consistent demonstration of the NEGIS branch switching" would be the best way to demonstrate our conclusions. Unfortunately, this is very hard to reproduce in current ice-sheet models, as the complex interplay between ice flow, hydrology and basal shear stress, which is likely what drives such an internal switch, is strongly dependent on the parameterisations used and the applied spatial resolution, all known limitations of continental ice-sheet models. We decided to manually apply such a switch to be consistent with the novel findings of Franke et al., 2022, which suggest a reorganisation of the NEGIS branches throughout the Holocene, and to follow previous work implying that ice-stream systems might be temporally variable, as switch in the flow regimes can occur after an initial perturbation (e.g. Syag et al., 2011, Conway et al., 2002). Still, our knowledge about the mechanisms that trigger the reorganisation of these flow regimes is incomplete, and beyond the scope of our work. On the other hand, we show that such an internal switch is not crucial to recreate our results (see comments below and new Figure S16 - previous Fig. S8). This suggests that what drives our conclusions is not the switching between branches, but rather the interplay between the Northeast (NE) retreat, basal conditions and ice dynamics. Our setup still allows us to robustly conclude that the ice flow in northeast Greenland plays a large role in ice-surface elevation drop in central Greenland.

We continue the discussion below, answering point by point. Please, note that the figure numbers refer to the current submission of the manuscript, unless stated differently.

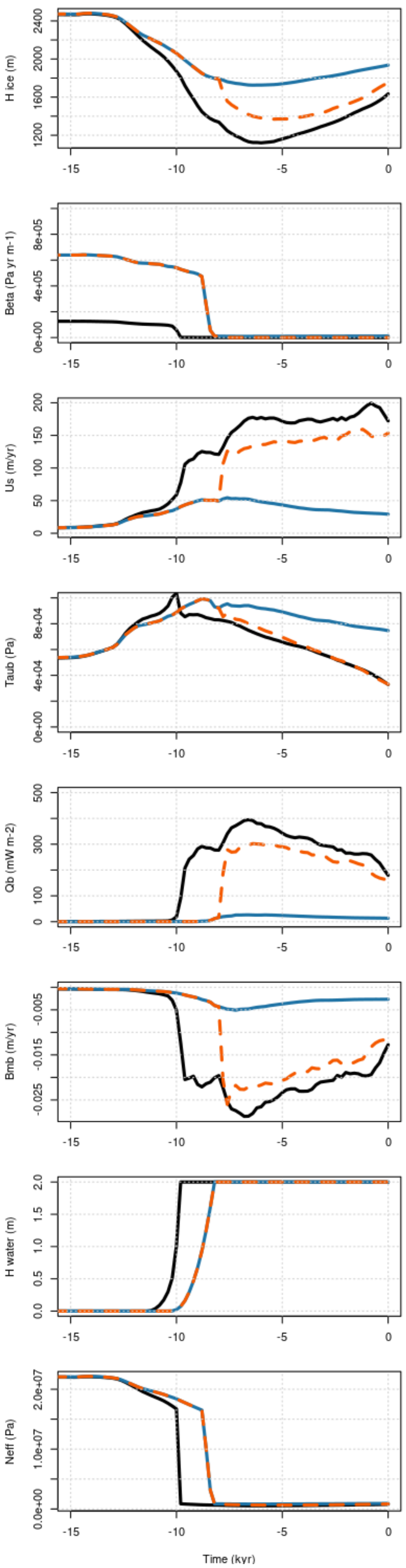
But just showing that if you effectively force near shutdown of the NEGIS south branch before 8ka, you can get 100 m or so of Holocene thinning at the geographically proximal NGRIP site, is not that surprising.

In Figure S16 (previous S8), case 3, we show that the shutdown of the NEGIS southern branch before 8 ka and a subsequent activation after 8 kyr is not mandatory to recreate the NGRIP dropdown. In fact, our model still shows a NGRIP thinning when the three NEGIS basins (north, central, south) have been active already before 8 kyr ago. This is because reducing the friction coefficient beneath the NEGIS is just a precondition for an effective reduction of basal friction, which only occurs when the Northeast retreats. In fact, as explained in the manuscript: "At the onset of the deglaciation, ice-mass loss at the shelf edge due to rising air and oceanic temperatures (Fig. S3) weakens buttressing, accelerating inland ice flow. This results in glacier thinning, ice flow speed-up and retreat of the grounding line, which further accelerates the ice flow, promoting ice front recession. Although ice thinning at the margin reduces basal friction (as effective pressure decreases), sliding significantly increases when a local rise in subglacial melt occurs due to increased frictional heating (Fig. S4). As the retreat continues, regions showing decreased basal friction conditions allow for flow acceleration farther upstream towards the ice divide". An extensive explanation on the impact of the NEGIS activation and on the form and timing of the stream on the NEGIS dropdown follows below.

Furthermore, when I look at eg Fig S5, the thinning starts before the imposed Cf reduction at 8ka, so I'm not clear if this is even necessary, nor exactly what impact it has. Nowhere do you clearly motivate the form nor timing of the imposed time-dependent changes to Cf.

We take advantage of this comment to explain better the processes behind the activation of the NEGIS in our runs, the effects it has on the NGRIP dropdown and to which degree are important the form and timing of the ice stream. To this aim, we compare three different runs: the best run of our ensemble, an additional simulation (hereafter the unperturbed simulation) where cf is kept constant and equal to 1, as suggested by the reviewer, for the three branches, so that the basal friction at the NEGIS is not reduced artificially and there is no effect related to the switch, and a third run, (hereafter the delayed run) for which cf has been kept constant to 1 before 8 kyr ago (as the unperturbed run) and then reduced to f_min afterwards.

Which is the role of imposing a reduced cf in the ice stream activation?



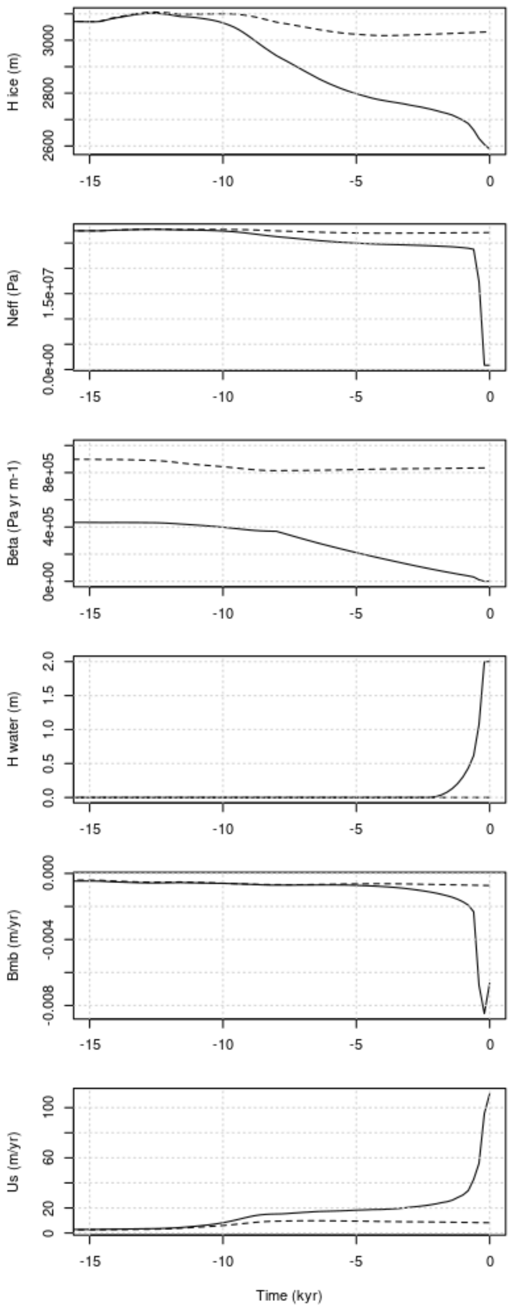
We show on the left the evolution of a point in the centre of the NEGIS southern branch (see green diamond of new Fig. S11) throughout the deglaciation for the three above-mentioned runs (the best run with black solid lines, the unperturbed run with blue solid lines and the delayed run with dashed orange lines).

Let's look first at the unperturbed run (cf=1), represented here by the blue solid lines. We recognise four different periods of interests: before 8.8 kyr ago, between 8.8 and 8.2 kyr ago, between 8.2 and 7.4 kyr ago and after 7.4 kyr ago. In the first period, as the marine margin retreats at the beginning of the deglaciation, velocity increases inland through velocity propagation upon margin retreat. This causes dynamic thinning at the centre of the NEGIS (decreased ice thickness), slightly reducing the effective pressure and the spatially variable basal friction coefficient (beta, where Tau_b = beta*u_b). Although beta is decreasing at this stage, velocity is increasing faster due to propagation of velocities, thus the basal shear stress increases (tau_b). This slightly increases basal heat production (Q_b), which causes an increase in basal melt rate (bmb) and an increase in the water amount (H water). Between 8.8 and 8.2 kyr ago, the till is then close to saturation as a result of increasing basal melt production. High values of basal water drastically decrease the effective pressure and beta, while they drastically increase the basal velocity. At 8.2 kyr ago, the till is saturated, beta and effective pressure are very low, but they keep decreasing as the basal meltwater increases (due to increasing basal heat production). This causes the basal velocity to increase. At 7.4 kyr ago the retreat of the NE sector in correspondence with the southern branch of the NEGIS stops. This decelerates the flow, reducing basal stresses, basal heat production and melt rate, leading to an increase in beta and a decrease in basal velocities. Summarising, the velocity increase in NE sector is first triggered by the ice front retreat during the deglaciation and, then, enhanced by increased meltwater production, causing higher basal sliding, and further velocity increase until the margin stops retreating.

But what happens when we decrease cf?
Let's look at the differences between the best (black solid lines) and the unperturbed runs (blue solid lines). For the best run, the response of the ice sheet to the initial perturbation (margin retreat) is less constrained, as compared to the unperturbed run, as it can be seen by modelled higher velocities. This is because the imposed lower cf promotes sliding, further reducing the basal friction coefficient beta. Then, the most notable differences come

around 10 kyr ago, when the processes that drive inland ice acceleration come into play. The chain of processes for the best run, compared to the unperturbed run, could be described in this way: initial imposed lower cf leads to a greater increase in ice speed around 12-11 kyr ago (when the ice-sheet margin retreats inside the continent and the stream starts to form), which leads to a more pronounced dynamic thinning (lower ice thickness), stronger decrease in effective pressure and a stronger increase in basal sliding. Increase in basal velocities causes a prompt increase in basal water production already after 10 kyr through higher frictional heating. High values of basal water drastically decrease the effective pressure and beta, while they keep increasing the flow velocity. The role of decreasing cf with respect to the unperturbed run is therefore twofold, for a given point: it makes the hydrological system reach the saturation earlier through an increased basal meltwater production, and it amplifies the increase in ice velocity once the system is saturated through a greater decrease in basal friction, boosting dynamic thinning and velocity increase at higher elevations.

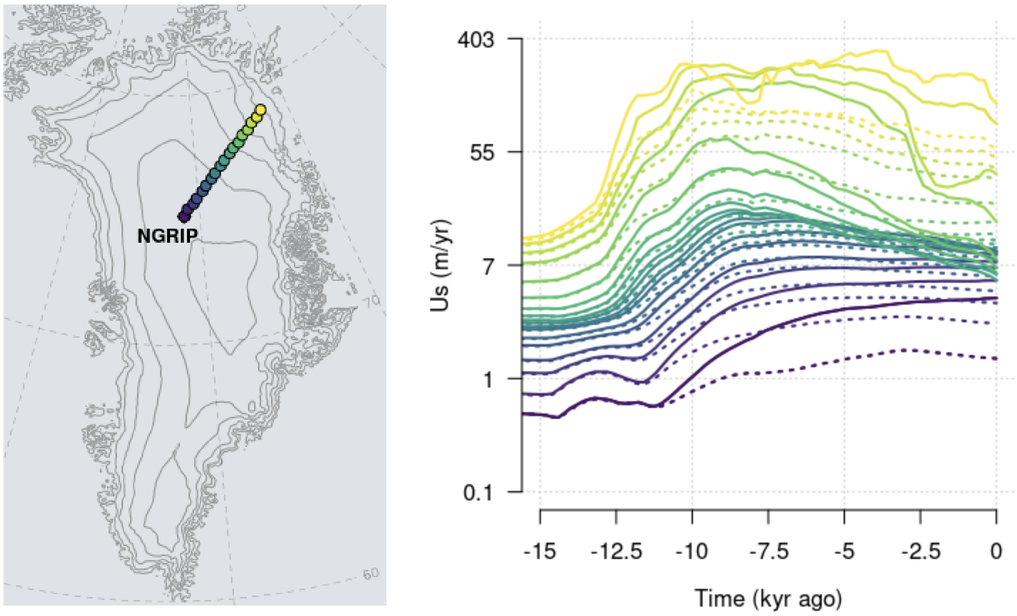
It is also interesting to compare the results for the “delayed” run (orange dashed lines). By applying a reduced basal friction coefficient beneath the NEGIS only after 8 kyr leads to a prompt velocity increase due to an increase in melt water production (as the till is already saturated, as H water is already at its maximum), that is sustained until the end of the Holocene.



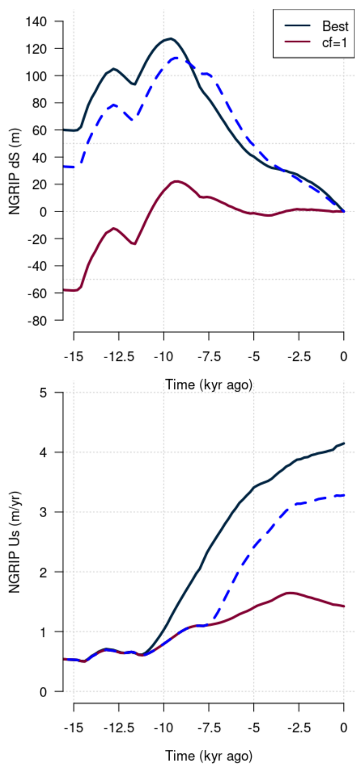
The reduced effect of mechanisms and feedbacks acting along the NEGIS for the unperturbed run are even more evident at EGRIP (figure to the left) as the stream is not able to stretch far upstream due to the imposed high basal friction coefficient (see best run in solid and unperturbed run in dashed lines).

[How is the NGRIP thinning triggered?]

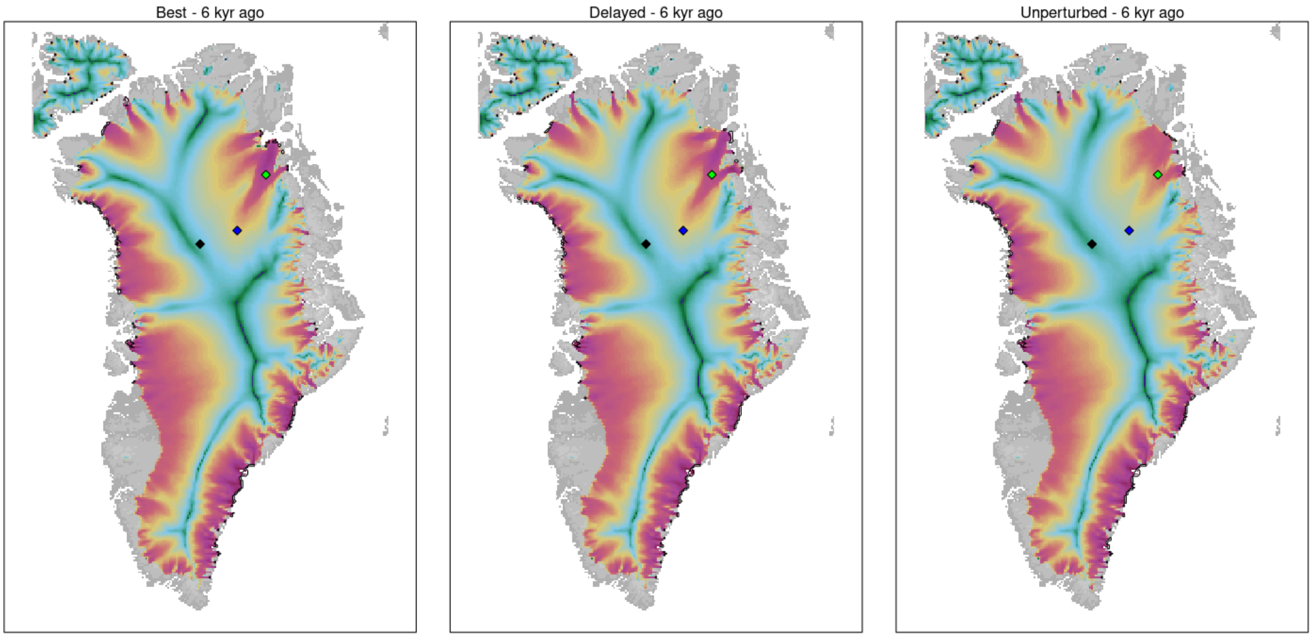
Now, let's look at which effect these processes have on NGRIP at the early Holocene. We show here the surface velocity modelled through time for different points along a transect that connects NGRIP to the NE ice-ocean margin for the two simulations (best run: solid lines; unperturbed run: dashed lines).



Here it can be noted that, independently of whether the basal friction coefficient is reduced or not beneath the NEGIS, the ice acceleration starts earlier at the outlet margins and then slowly propagates inland and with a lower magnitude, through velocity propagation. This shows that such an increase in velocities towards NGRIP around 11 kyr is the direct effect of margin retreat, and independent from the presence of an active NEGIS. In other words, *it is the increase in NGRIP velocity around 11 kyr through dynamic adjustment along the NE sector upon margin retreat that triggers the dynamic thinning at NGRIP*. This is seen in the figure below, where we show the surface elevation drop and surface velocity modelled at NGRIP for 3 different tests: the best run, in black, the unperturbed run (cf=1), in purple and the "delayed" run, for which the NEGIS is completely inactive until 8 kyr and then the central and southern branches are switched on, shown here with a blue dashed line.



The thinning at NGRIP starts before the imposed reduction at 8 kyr, as it is triggered by the retreat of the Northeast sector, which, through velocity propagation, causes a dynamic response in the ice sheet interior, increasing velocities at NGRIP at around ~11 ka, and causing surface elevation drop with a delayed response at around 9.8 kyr. This occurs independent from how the NEGIS is treated, as the beginning of the dropdown is due to the velocity increase at the NGRIP site, initially due only to dynamic adjustment through the NE sector upon margin retreat. However, reducing the cf at the NEGIS indeed speeds up the flow throughout the NE sector, and at NGRIP too, enhancing the thinning in central Greenland throughout the Holocene. Applying a reduction in cf at the NEGIS only after 8 kyr ("delayed" run) slightly reduces the NGRIP dropdown compared to the control run, yet successfully drives the thinning through the rest of the Holocene. This is because basal conditions at the NEGIS at 8 kyr are already favourable for sliding (the till is already saturated by water, which reduces basal friction), thus an abrupt reduction of cf causes a further decrease in the basal friction, causing a prompt and significant increase in velocities (as explained above).

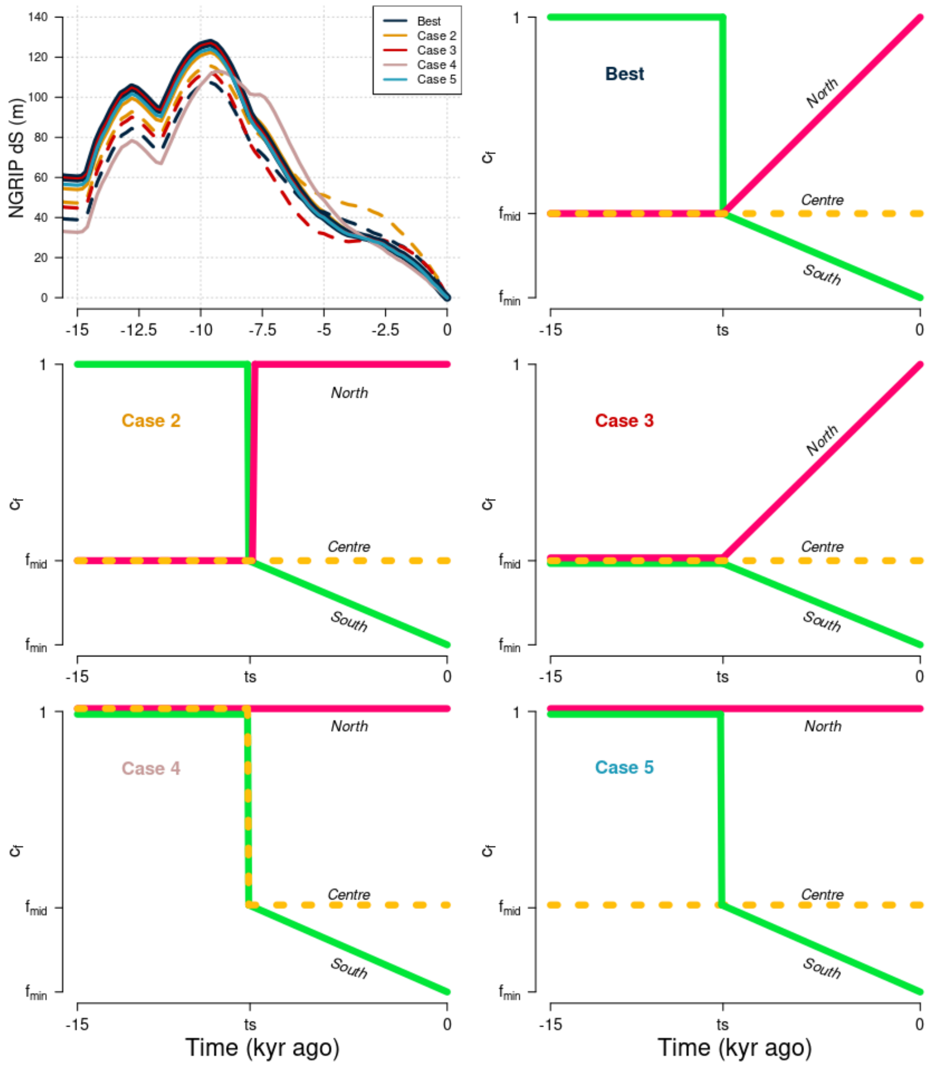


The magnitude of the response of the ice flow depending on the imposed cf can also be seen by looking at the surface velocities modelled at 6 kyr for the three experiments (best, delayed and unperturbed run, see figure above): the unperturbed run shows an ice stream that is not well developed inland at 6 kyr as the other two runs, constraining the thinning in north central Greenland. By activating the NEGIS at 8 kyr, the model still has time to develop a fast stream in correspondence with the NEGIS, through the processes mentioned above, that remains active for the rest of the Holocene and drives the retreat.

These examples show that 1) what *triggers* the NGRIP thinning is the NE retreat and the dynamic adjustment associated with it, independent from the applied NEGIS basal friction coefficient, 2) but what *drives* it throughout the Holocene are the basal conditions imposed at the NEGIS, here favoured to the imposed reduced basal friction coefficient. Technically, setting basal conditions that favour a faster flow (cf < 1) before the onset of thinning (~10 kyr ago) enhances the thinning for the first thousand of years, but is not mandatory to drive the retreat, as long as a reduced basal friction coefficient is imposed throughout the rest of the Holocene, allowing faster flow (see Case 4 of the figure S16). Meaningfully, this shows that an active NEGIS throughout the Holocene, as supported by reconstructions (Franke et al., 2022 and Gerber et al., 2021) is a key factor in driving the NGRIP dropdown.

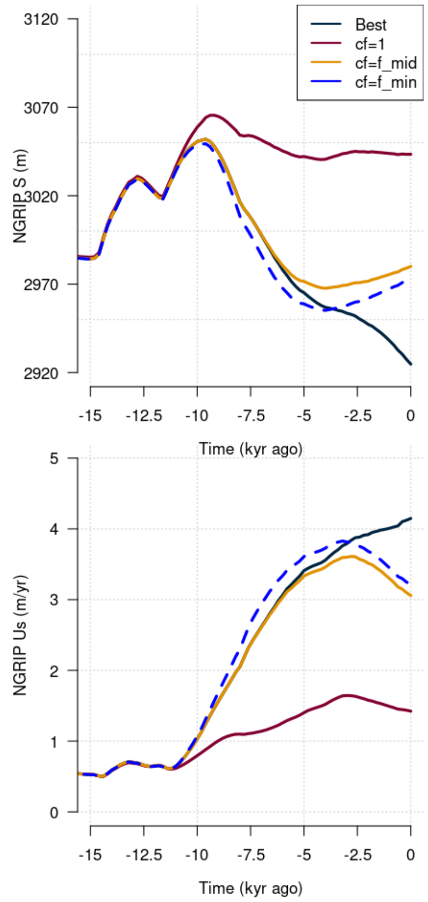
<u>Role of timing and form of switch between branches</u>

The existence of a switch between branches (north, centre and south) or its timing is of secondary importance. This was partly shown in Figure S16 (previous S8), where the application of a switch at 4.5 kyr (dashed lines) or at 8 kyr (solid lines) doesn't impact the results significantly. Now, two new cases added to Fig. S16 (case 4 and 5, figure below) show that even though the northern branch is kept inactive for the whole run, the NGRIP thinning takes place anyway due to the activation of the present-like NEGIS, which occurs through increased basal water and reduced effective pressure.



Although the presence of an active northern branch for part of the Holocene does not impact the results, we decide to include it anyway in our methods to follow the new findings of Franke et al. (2022). Summarising, what *triggers* the NGRIP thinning around 9.8 kyr is the dynamic adjustment caused by the early Holocene NE retreat through velocity propagation, but what *drives* it for the rest of the Holocene is the boosted dynamic adjustment due to the presence of an active stream in the Northeast sector, no matter its form or time of activation.

However, the imposition of an additional linear reduction in cf for the southern branch for the last part of the Holocene is needed to ensure that, during the Neoglacial, the ice stream velocities remain high even though the ice sheet re-advances. This can be seen in the figure below, where surface elevation drop and velocities at NGRIP are shown for the best run (in solid black) and for the three runs, where cf=1, cf=f_mid and cf= f_min are kept constant for the whole run.



The linear reduction of cf at the late Holocene is of course a technical workaround to keep the stream in its present form until the end of the run, as current numerical models cannot represent the activation of the stream fully physically. We decide to apply this technical workaround as we know from reconstruction that the NEGIS should have been active and with a present-like form for the last 8 kyr (Gerber et al., 2021). It should be noted, however, that the reduction from f_mid to f_min in the best run is minimal as f_mid=0.30, and f_min=0.26, thus cf is reduced by only a ~13% of its initial value.

The elongated form of the ice stream, propagating several hundreds of km inland, is clearly crucial in the NGRIP dropdown as it ensures a sustained dynamic adjustment in north-central Greenland. As described above, and as seen in 2D velocity plots at 6 kyr, an unperturbed run restricts the inland velocity propagation to lower elevations, limiting the effect of the stream activity at the NGRIP site. Again, a NEGIS with a present-like form for most of the Holocene is also suggested by reconstructions (Gerber et al., 2021, Franke et al., 2022), therefore we think our modelling strategy is well justified.

We have now added most of these Figures and explanations to the Supplementary Material and Methods. We think it might clarify the choice of timing and form of the stream and the associated mechanisms responsible for the NGRIP dropdown.

The authors also endeavour to spell out causal chains from marginal retreat to upstream thinning, but they don't provide the detailed evidence to back up their claims (and don't make clear if their claims are hypothesis or based on detailed model analysis). Some thought in to appropriate sensitivity experiments would much more strongly elucidate these causal chains. At the very least, the experimental design and analysis needs to be expanded to include runs with constant basal friction coefficient Cf throughout the run (and for the 3 different values of Cf: fmin, fmid, and 1). This will make much clearer the relative role of stream activation in driving the thinning. I'd be very surprised if these runs weren't already done as a step towards choosing the imposed Cf chronologies. Fig S8 shows some sensitivity tests for the Cf chronology, but they lack the suggested ones that I think would be most enlightening. More detailed analysis of the ensemble along with appropriate sensitivity experiments would also better support or refute key claims like: "Here we show that the early grounding-line retreat in northeast Greenland triggers and drives the elevation change at the northern summit."

We thank the reviewer again for his valuable comments. We hope to have clarified all these aspects now in the paragraphs above. We also modified the manuscript accordingly. We believe the manuscript has greatly improved.

As detailed below, there are a number of erroneous or misleading statements that also need to be addressed before this submission would be acceptable for publication.

Lev Tarasov

detailed points

“Here we show that the early grounding-line retreat in northeast Greenland triggers and drives the elevation change at the northern summit. Fast ice-stream flow caused by reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning, showing that ice-stream dynamics is one of the main drivers of the NGRIP Holocene surface elevation drop.” This is misleading, given the missing caveats in the above claims. More accurately you show that the forced time-dependent imposition of reduced basal friction coefficient for the southern NEGIS branch (whose upstream onset is geographically proximal to NGRIP), can reduce NGRIP elevation. Nowhere do you show that "early grounding-line retreat in northeast Greenland triggers and drives the elevation change at the northern summit.”

We agree that this sentence might be misleading. However, we have shown above that what triggers the NGRIP dropdown is the early margin retreat in northeast Greenland. For the driving part, we showed the dependency on the timing for the reduced cf is not a key factor of the dropdown, as long as the cf is slightly reduced throughout the Holocene to sustain the existence of the ice stream until the present day: this is shown in Fig S16, where for case 3 the southern branch is *always active* (with an imposed low cf, and then slightly reduced either at 8 kyr or at 4.5 kyr ago), still the NGRIP elevation is reduced as in the best run.

However, as discussed above, it is true that we impose a reduction in cf at some point in the Holocene to ensure that the propagation of velocities during the NE retreat will suffice to maintain an ice stream at the late Holocene that resembles that of today, as suggested by reconstructions. In other words, as long as the form of the NEGIS resembles that of today during the Holocene, the NGRIP site suffers a sustained surface elevation dropdown in our model.

We clarify this aspect by modifying the sentence to “Here we show that the early grounding-line retreat in northeast Greenland triggers the elevation change at the northern summit. Fast ice-stream flow caused by transiently imposed reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning, showing that ice-stream dynamics is one of the main drivers of the NGRIP Holocene surface elevation drop.”.

“high-latitude ice-elevation changes are mainly driven by the Arctic amplification and glaciostatic effects” Changes in precipitation could also play a key role. Furthermore, I see nowhere in the cited (Buizert et al), the above claim, only the statement that there is arctic amplification in the temperature response to orbital forcing. Buizert et al, provide no analysis of changes in precipitation or critical self-evaluation of what aspects of the TRACE results have high confidence given the coarse resolution (T32) driven by an ice sheet chronology (ICE-5G) that had a significantly higher Keewatin ice dome than current reconstructions which potentially significant consequences on atmospheric circulation.

Modified as “high-Arctic ice-elevation changes are mainly driven by the polar amplification, glaciostatic effects resulting from the demise of the nearby North American ice sheets (Lecavalier et al., 2017) and changes in precipitation (Vinther et al., 2009)”. We removed the citation from Buizert et al. 2018.

“surface-velocity assimilation for basal shear stress estimation yields the most accurate basal friction approximation.” Only where the bed is not currently frozen and that assimilation depends on the assumed temperature profile of the ice or otherwise ignores the vertical shear deformation of the ice.

Modified to “surface-velocity assimilation for basal shear stress estimation yields the most accurate basal friction approximation, although only where the bed is currently temperate, assuming a specific temperature profile.”

“The accurate timing of the deglaciation is captured by using high-temporal resolution climate reconstructions6 to force the model.” High-temporal resolution doesn't imply accurate timing, especially given the limitations of the TRACE chronology used in reference 6 as described above.

Modified to "The accurate timing of the deglaciation is ensured by evaluating the model results against deglaciation timings inferred from marine sediment records from the Northeast Greenland continental shelf."

"We characterise the past northeast fast flow regime as a system of a northern, now-extinct paleo ice stream and a southern present-day-like ice stream that activated during the Holocene (Fig. 1)."Need to make clear that you are forcing by hand the reduction in basal friction that enables the streaming.

Modified to "We characterise the past northeast fast flow regime as a system of a northern, now-extinct paleo ice stream and a southern present-day-like ice stream that activated during the Holocene by imposing a time-depending basal friction change beneath the NEGIS (Fig. S1). Such a stream configuration is in line with recent improvements in our understanding of this system."

"Figure 1. Switch of NEGIS branches throughout the Holocene. The friction coefficient cf reduced below the paleo an…". The caption needs to make explicitly clear that the first frame is the imposed basal drag chronology. As a start, the caption title should be "Imposed changes to basal friction coefficient Cf …"

Now Figure S1. Modified to "Imposed changes to basal friction coefficient Cf beneath the NEGIS branches throughout the Holocene: …"

Fig 1: No justification for the ramped decreased over 8 kyr is provided. The sensitivity tests (Fig S8) partly address this by testing a 4.5 kyr interval of the ramp, but a more confident sensitivity test to bound uncertainties would take one bounding Cf chronology with an fmin constant value throughout.

We have provided the results of three additional sensitivity tests with constant cf (cf=1, cf=f_mid and cf=f_min) above. This figure has been now added to the Supplementary material and the main text has been changed accordingly to make the further reduction beneath the southern branch after 8 kyr clearer. Furthermore, we added this sentence to the caption of Fig. S1 (previous Fig. 1, now in the Supplementary Material): "A linear reduction (increase) after 8 kyr is imposed to the southern (northern) branch to ensure a present-like shape of the NEGIS throughout the end of the Holocene".

"Such a far-inland reach of dynamic thinning in northeast Greenland during a major retreat phase suggests that such processes may also amplify the response of this sector to future warming and ice loss." So why did it shutdown?

Investigating the causes of the shutdown of the northern branch during the Holocene is out of the scope of our work. Still, in this paragraph we refer here to "paleo NEGIS" as the system of streams that were active during the Holocene, not only to the northern branch that is now inactive. Still, such a far-inland reach of dynamic thinning is related to the shape of the NEGIS, close to its present form.

To clarify this point, and following also Reviewer#2's suggestion, we modified the sentence to "The far inland development of the present-like NEGIS during the last 8 kyr, here fostered by imposing a time-dependent reduced basal friction coefficient along the ice stream, drives the dynamic thinning throughout the Holocene, explaining at least ~100 m of the surface elevation drop in north-central Greenland. Such a far-inland reach of dynamic thinning in northeast Greenland during a major retreat phase suggests that such processes may also amplify the response of this sector to future warming and ice loss."

"Ice flow propagates upstream in response to thermo-mechanical and geometrical changes occurring during the northeast retreat of the marine-based ice sheet". Need to be explicit: in response to imposed cf(x,y,t) basal friction coefficient changes.

Modified to “Increased sliding velocities propagate upstream in response to thermo-mechanical and geometrical changes occurring during the retreat of the marine-based ice sheet, through an imposed basal friction coefficient reduction through time.” Please, also note that cf is not spatially dependent within a basin, therefore cf=cf(t).

“(Fig. 3f). Then, velocity propagation associated with the inland expansion of the paleo-NEGIS induces dynamic thinning of ice at the NGRIP site”. And again, you should explicitly add "imposed" to "inland expansion.."

Modified to “(Fig. 2f). Then, the velocity propagation associated with the imposed inland expansion of the paleo-NEGIS induces dynamic thinning of ice at the NGRIP site…”

“Margin thinning and retreat, driven by early Holocene temperature rises, induces ice thinning at higher elevations, reducing effective pressure, increasing basal sliding, and fostering ice acceleration and further margin retreat. Primarily, dynamic thinning triggers reduced basal friction during the retreat. Still, basal sliding at the stream increases considerably only with a significant basal water increase from enhanced basal frictional heating (Fig. S12). Reduced effective pressure and basal friction are associated with increased basal velocity at the stream region, allowing upstream paleo-NEGIS propagation. The NGRIP dynamic thinning (and surface elevation drop) is therefore the indirect response to margin perturbations via geometry and subglacial water system alteration” Given what is currently presented, I see clear support for the last sentence. The whole causal chain could be tested with some appropriate sensitivity experiments.

Please, refer to the extensive discussion about the chain of events reported above in response to the general comments. This paragraph is now supported by new figures added in the supplement material.

“Figure 2. Greenland ice dynamics during the last deglaciation.” This figure should include the basal friction coefficient (cf(x,y,t)) masks from fig 1 to make clear how much of the ice stream extent is driven by the imposed cf chronology.

Now Figure 1. As cf is a spatially homogeneous coefficient, with the form and timing shown already in Fig. S1, we believe it is not necessary to add a mask to Figure 1.

“No similar correlation is found for any other GrIS basins in our experiments (Fig. S3), indicating that there is no other catchment where ice flow plays a significant role in the NGRIP thinning.” But this is effectively what you've put into the model in your figure 1, you've only imposed the basal drag reduction in the NEGIS basin and since you don't show what the "other basins" are, I have no way to evaluate.

We reduced the friction coefficient at the NEGIS to help the model reproduce the shape of the present NEGIS, as the stream is not topographically constrained. It would not make sense to decrease the friction coefficient in all the other catchments, as streams outside the northeast are already quite well represented in our model (Figure 1). By reducing the friction coefficient there, we would likely induce a faster and more upstream flow which would not be representative of reality. However, even in that case, we would not expect a significant contribution of these ice streams to the NGRIP dynamics, since most of the correlations between the other catchments and NGRIP thinning rate is either zero or negative for this set of simulations (Figure S7).

And I'm confused by S3: “Holocene-averaged NGRIP thinning rates as a function of the mean-NEGIS basal shear stress for different GrIS basins”. I don't know how to interpret this given that the basins aren't shown in this paper.

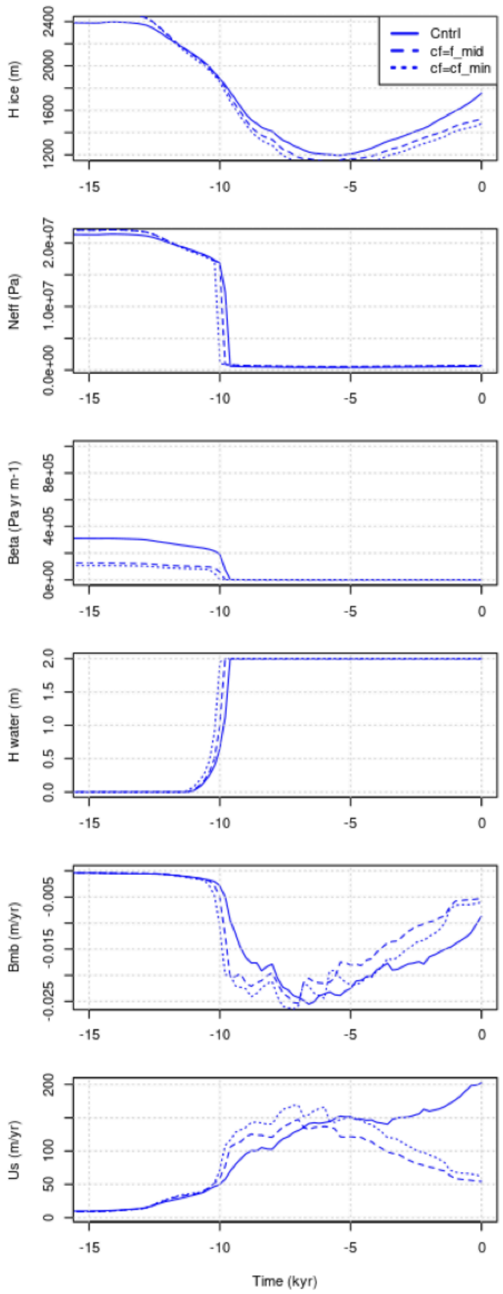
Now Figure S7. Agreed, the caption was wrong. We refer to the basins shown in Zwally et al., 2012. Now it has been modified to “Holocene-averaged NGRIP thinning rates as a function of the mean basal shear stress for different GrIS basins (Zwally et al., 2012).” We are grateful to the reviewer for pointing out this error.

“The bed-elevation dependent basal friction coefficient representing bed roughness at the NEGIS strongly controls the ice-stream basal conditions, hence the NGRIP thinning (Fig. S4)”. So what would cause basal roughness to change over the course of a few kyr as you impose (fig 1)?

The bed roughness might have changed throughout the millennia as the bed might have been deformed by sediment transportation as a result of glacial drift and/or surface melt (Andersen et al., 2024). Investigating the causes of such a change are beyond the scope of our work.

However, we add this sentence to the discussion to open possibilities: “The bed-elevation dependent basal friction coefficient representing bed roughness at the NEGIS strongly controls the ice-stream basal conditions, hence the NGRIP thinning (Fig. S8). Such a change in basal roughness, as imposed in Fig. S1, could be due to the effect of sediment transportation and deposition below the stream through glacial drift and subglacial water routing, acting at long timescales.“

“Figure 6. Northeast Greenland Holocene Relative Sea Level change. Relative Sea Level (RSL) curves for Northeast Greenland modelled by Yelmo, forced by two different air temperature reconstructions, and compared to RSL data. In our simulations, the progressive upstream decrease in basal traction during the ice-margin retreat stems from a decline in the basal effective pressure due to increased meltwater production”. Given that Cf is linearly decreased after 8ka, and that it's not clear what time interval you are talking about here, how am I supposed to judge this "progressive upstream decrease" is not solely due to the imposed Cf decrease?



It is difficult to define a precise time interval here, as the decline in effective pressure due to increased meltwater production occurs at different times depending on the site along the NEGIS we are looking at. In fact, as seen in Fig. S4, the great decline in basal friction occurs around 10 kyr ago in a central point of the NEGIS, while it occurs several thousand years later at the tail of the stream in the best run (see figure of Pag. 3 of this review response). Since cf is reduced everywhere along the NEGIS at 8 kyr, the upstream decrease in basal traction cannot only be the response to the cf reduction. There are in fact other mechanisms at play, as explained above extensively. The reduction in cf applied at 8 kyr in the best run slightly decreases the basal friction, reducing the decrease in basal melt rate, and ensuring a sustained increase in velocity for the last 5 kyr of the simulation (as seen on the left).

To clarify this point, we changed this sentence to: “In our simulations, the initial increase in velocity is associated with dynamic adjustment due to ice-margin retreat (Fig. S5), which causes a decline in the basal effective pressure and friction and an increase in meltwater production stemming from increased frictional heating. Sustained basal melt rates at the early Holocene saturate the basal till within a few thousand years, promoting a progressive upstream decrease in basal traction and faster flow (Fig. S4)”.

“We find that the northern summit Holocene thinning rate is well correlated with the NEGIS basal shear stress (R=0.59; Fig. 5b, c), with the strongest dependence for the early-mid Holocene interval (Fig. S2).” Fig S2 shows a weaker dependence (nominal R=0.54) not stronger.

We agree that that sentence was misleading. Here we wanted to show that the dependence is stronger before 6 kyr (r=0.54) than after (r=0.18). We now changed it to: “We find that the NGRIP Holocene thinning rate is well correlated with the NEGIS basal shear stress (R=0.59; Fig. 4b, c), calculated as the average between all the NEGIS catchments (northern, central and southern branches), with the strongest dependence for the early-mid Holocene interval with respect to the late Holocene (Fig. S6).”

“The internal switch applied at the mid Holocene (Figure 1) and the time dependent variables in the friction law 112 (effective pressure and scaling parameter depending on bedrock elevation)”. It should state here where details are.

Agreed. Modified it to: “The internal switch applied at the mid Holocene (Fig. S1) and the time dependent variables in the friction law (effective pressure, see Eq. 4, and scaling parameter λ depending on bedrock elevation, see Eq. 3) …”

“complex unsteady dynamic state of the ice stream due to continuous fluctuations...or variations in the ice viscosity due to changes in the ice crystal fabric and in the thermal state of the ice column”. Isn't this thermomechanically coupled to compute the thermal state and it's impact on ice viscosity?

The reviewer is right: the model is thermomechanically coupled and changes in the ice viscosity are actually computed through changes in the thermal state. We therefore removed the last part of the sentence and we now changed it to “However, these approximations, the coarse model resolution and the simple hydrology model here employed are otherwise insufficient to reproduce the complex unsteady dynamic state of the ice stream due to continuous fluctuations in the basal water system.”

“Although our approach approximates the past activation/deactivation of the northeast ice stream through an a-priori relocation of the paleo ice stream to its present position, the root causes of such an internal switch are not investigated. The inclusion of physical processes important to describe ice-stream motion such as non-local basal water routing, and others at smaller scales of more recent attention, such as spatial variability in ice anisotropy along the ice stream and localised ice deformation arising from realistic topography roughness, would permit a better investigation of the complex NEGIS dynamics in the future”. Should explicitly state if there any published evidence to suggest that these listed processes would operate on the required timescale and have enough impact to induce such strong switching? The above cited reference 10 for basal water routing, for instance, does not show any result suggesting that basal water routing modelling could drive such a multi-millenial switching timescale. Nor does ref 17 suggest have any mention of ice anisotropy in relation to stream activation. Later on you do provide some relevant discussion, so you should clearly point to it from here.

Here we wanted to emphasise that several limitations in our approach, such as the usage of a local basal drainage parameterisation, or the coarse resolution for basal topography, might hamper a correct representation of the NEGIS and its complex dynamics. We didn’t want to address such limitations as responsible for the model not being able to drive a multi-millennial switching, since, as pointed out by the reviewer, there is no proof that such processes would be able to resolve such a dynamical switch. Since we address the limitations of our approach further in the discussion, we decided to follow the suggestion of Reviewer #3 and we deleted most of this paragraph.

The paragraph now reads as: “The inclusion of physical processes important for describing the present-day ice-stream behaviour such as non-local basal water routing, could allow for a better investigation of the complex NEGIS dynamics in the future.”

“a-priori relocation”. "a-priori" is confusing -> imposed

Modified to “imposed relocation”.

“Computing RSL curves with a simple GIA model is justified as RSL data in northeast Greenland are unlikely to be affected by the deglaciation of the North American Ice Sheet”. Though some papers in the past have show limited sensitivity of GRIS evolution to the simplistic plate lithosphere, relaxing bed GIA model used here no one has ever claimed that meaningful RSL curves could be computed by such a model. Furthermore, the geoidal perturbations have much farther reach than the bed perturbations. From what I can tell, you complete ignore this key part of RSL, so your curves have little interpretable value and are not worth comparing to RSL proxy data.

We agree with the reviewer, computing the RSL curves with the ELRA model as GIA model is a crude approach, as we do not account for spatial variability in the sea level as well as non-local changes in the Earth deformation. More precisely, we do not account for 1) the solid Earth response due to non-local ice loading/unloading, 2) perturbation in the geoid due to far reaching changes in global mass and 3) gravitational effects between the ice sheet and the sea level, which affect the shape of the geoid at the marine margin of the ice sheet, thus the local sea-level height. We agree that a more reliable estimate of RSL changes would be provided either by the inclusion of a more complex GIA model capable of computing local and non-local Earth deformation, changes in sea level by directly solving the sea level equation, and changes in the gravitational effect (e.g. in Lecavalier et al., 2014), or by running a fully coupled ice-sheet - sea level model (De Boer et al., 2017).

However, as answered to Reviewer#3, we do not seek here either to provide a new estimate of the RSL change around the northeast margin or to validate our model against RSL data. Rather we want to use the RSL reconstructions only as an additional proxy to investigate the northeast deglaciation chronology as modelled by our ice-sheet model being forced by two different climatic forcings. Since the sea level signal is equal for both model ensembles, differences in our modelled RSL arise only from changes in the glaciostatic response to variations in the northeast ice load. Thus, our modelled RSL curves are here interpreted as a proxy for margin retreat chronology: a different rate of temperature increase during the early Holocene affects the timing of deglaciation, thus the isostatic response to ice unload, thus the RSL change. Figure 5 (previous Fig. 6) is therefore used for this scope, i.e. evaluating the performance of the two climatologies in shaping the deglaciation chronology. In this analysis we see that the delayed ice unload/margin retreat in the northeast modelled by the data-assimilation driven climatology (Badgeley et al., 2020) results in a systematic delay in the RSL drop at the early Holocene (Fig. 5), as it is caused by a delayed isostatic uplift. One could simply show the comparison between the two RSL curves, however we think that an additional comparison against RSL data provides a better metric to evaluate the performance of the two climatologies in the northeast.

We modified lines 135-138 to clarify these points: “Relative Sea Level (RSL) curves computed for various locations in northeast Greenland confirm this hypothesis, with the simulations forced by the default climatology generally following the RSL dropdown at the early Holocene (Fig. 5). Still, locations close to the present-day NEGIS margin show an unexpected negative RSL for the late Holocene (Fig. 5 f-l). The northeast margin retreat is simulated ~40 km farther upstream than suggested by data (Fig. 2d), inducing a stronger isostatic response than observed. Given the simplicity of our GIA model, such RSL curves must be considered with caution, and used solely for comparative purposes between the two climatologies. Notably, our model does not account for spatial sea-level variations (e.g. caused by geoidal perturbations due to non-local and local gravitational effects) or global changes in Earth deformation (i.e. due to the North American Ice Sheet). Consequently, the modelled RSL differences are solely attributable to variations in the glaciostatic response to local ice load changes and should be viewed as an approximation of margin retreat chronology.”

We also added this paragraph in the Method section to clarify how the RSL curves are calculated: “Bedrock elevation changes are computed via the Elastic Lithosphere Relaxing Asthenosphere (ELRA) scheme. This simple but effective glacial isostatic adjustment (GIA) scheme assumes viscoelastic deformation of the crust from the elastic behaviour of the lithosphere and the ability of the asthenosphere to relax upon ice load changes […]. The modelled bedrock elevation change with respect to the present is used to calculate the RSL curves of Figure 5 by subtracting the glaciostatic effect from the mean global sea-level signal from Waelbroeck et al., 2002, which is also imposed as a forcing to the ice-sheet model. As mentioned in the main text, such curves solely reflect the local deformation of the solid Earth, omitting a portion of the signal associated with sea-level spatial variability arising from both local and non-local perturbations in the geoid, and the response of solid Earth deformation to far reaching mass changes. Consequently, our modelled RSL curves should be regarded solely as proxies for local variations in the ice load of Greenland."

"Runs with an extremely low oceanic forcing (κ ~ 0) still simulate a retreat from the continental shelf, potentially causing rapid thinning at NGRIP (Fig S4). This implies that initial deglaciation in the northeast was likely sparked 156 by atmospheric warming. What your model needs and what likely actually happened are not necessarily the same thing.

We clarify this point, by modifying this sentence as "Runs with an extremely low oceanic forcing (κ ~ 0) still simulate a retreat from the continental shelf, potentially causing rapid thinning at NGRIP (Fig. S8). This implies that in our model initial deglaciation in the northeast is first triggered by atmospheric warming".

"where cf, a uniform coefficient representing the sliding capacity of the bed, is decreased through a factor λ depending 314 exponentially on the bedrock elevation (zb)56, as: eq (3)". Wording is confusing (cf is not decreased by lambda, Cb is), but more to point you need to motivate this choice of relationship especially since you previous state: The bed-elevation dependent basal friction coefficient representing bed roughness at… You need to spell out reasoning for correlating bed roughness to bed-elevation.

Agreed. Modified to "where cf is a unitless coefficient representing the local bed characteristics. The bed-dependent term is decreased through a factor lambda depending exponentially on the bedrock elevation zb, as: …, with z1=400 m and z0 as tunable parameter. This relationship ensures that ice flows faster in topographic depressions (Blasco et al., 2021)" .

"τa is the atmosphere transmissivity". How is this computed or set?

The atmospheric transmissivity (tau_a) is calculated as
tau_a=0.46+0.00006 (zs)^0.5
Where zs is the surface elevation. This equation has been added to the Methods. Please refer to Robinson et al., 2010 for further details.

"The ice sheet is allowed to freely evolve under LGM (20kyr ago) conditions for 50 kyr to reach the thermomechanical equilibrium." What basis is there to assume thermodynamic equilibrium? Why not spend 50 kyr of model time imposing transient climate and sealevel forcing?

We agree that imposing a transient climate and sea level forcing for 50 kyr would be more realistic. However, transient climate from Buizert et al., 2018 is only available for the last 22 kyr. Also, initialising the model for several thousand years assuming the ice sheet to be in steady-state thermal equilibrium is common practice (Goelzer et al., 2018) and in our case provides a realistic modelled LGM state, which is the start of our investigated simulations.

"based on paleo and present model-data misfits and then aggregate them into a single total score." What about accounting for data and structural uncertainties of the model?

In contrast to e.g. Pollard et al. (2016), Albrecht et al. (2020) and Briggs and Tarasov (2013), here we do not include data and structural model uncertainties in the misfit calculation. We agree this is a limitation of our skill score methodology. However, our work is not focused on such a technique, as the previously mentioned papers are, as we here want to make use of a simple statistical technique to rank our simulations. Thus, we do believe that the inclusion of uncertainties in the scoring methodology would not impact the overall results of our work. We add this sentence to clarify this limitation “However, in contrast to Pollard et al. (2016), here we do not account for data and structure model uncertainties in the misfit calculation.”

“Each misfit Mi, j, where i is the data class and j is the ensemble run, is then interpreted as a score Si, j following the equation:”. Eq 11 is not appropriate for a Gaussian error model contrary to your earlier claim: simple averaging skill-score method based on a Gaussian error distribution. You would need to use the mean squared error, not root mean squared error as for M_ij.

We thank the reviewer for his comment. He is fully right. We have computed the misfits and the score again to account for the mean-square-errors instead of the root mean square errors.

“which is further normalized between 1 (best run) and 0 (worst run).” So what does say 0.1 mean? total garbage, or bit likely or ?

We added a brief description to how we should interpret the score: “…which is further normalised between 1 (best run) and 0 (worst run). We assume a good total score if it is higher than the product of the mean score for each misfit for best runs, i.e. ~1e-7 (Fig. S10). ”

“To further reduce the uncertainty in our model results, we add another constraint that distinctly separates plausible from implausible model representations within the ensemble. Following this approach, runs are considered valid as long as the absolute elevation difference between modelled and observed elevations is less than metres (e.g., NGRIPPD<50m) so that 563 simulations over 3000 are retained as valid.” How was 50 m chosen? Why not 45 m, or 100 m? And why is this the only data constraint deemed worth a run rejection?

Of course the choice of this value is arguable. We could have chosen larger values (100 m, 150 m) for example, but we decided to keep it to 50 m to impose a tighter constraint on the ensemble at the present day. This allows us to reward simulations exhibiting an exceptionally accurate fit to present-day elevation. Having a high confidence in surface elevation data is justified as errors in surface elevation data are around 10 m for ice-covered regions (https://nsidc.org/sites/default/files/gimp-atbd-version2_0.pdf). This additional constraint could have been included as an additional misfit/score calculation, but we deliberately kept it separate as we wanted it to have a bigger weight in our statistics. Alternatively, we could have integrated the misfit relative to the observed present NGRIP surface elevation into the skill score calculation, assigning a total weight to it and adjusting the weights of the other partial scores accordingly. However, the results would have been similar.

We added this sentence “This additional constraint rewards simulations exhibiting an accurate fit to present-day elevation data, whose errors in the estimate are usually below 10 m.”.

“the law for till hydrology” This relationship is not a “law".

Modified to “the relationship for till hydrology”.

Fig S8, frame one, given that colour is used, please provide a higher contrast between Cntrl and Case 3, I can't discern the difference between the corresponding thick/thin black lines in the plot.

Now Fig. S16. Modified accordingly.

Fig 4: how does one "observe" a pre-historical era deglaciation time? Figure caption should be "inferred deglaciation time”

Now Fig. 3. Modified accordingly.

Fig S4: please either (prefered) state what the parameters are ('q' means nothing) or indicate where a table of YELMO parameters is in the paper (I couldn't find one, just a list embedded in text..)

Now Fig. S8. We added a table (Table S1) with important Yelmo parameters in the Supplementary Material specifying the parameter description, symbols, units and values explored in the ensemble.

Fig S5; include time ticks in each frame, and add grid lines so that one can easily read off the timing of each thinning onset.

Now Fig. S9. Done.

Fig S6: Individual scores versus mean-Holocene NEGIS basal shear stresses for best runs should also include scores of rest of 3000 member ensemble in say a transparent grey underprint to convey what range your whole ensemble explored.

Now Fig. S10. We added the scores of all ensemble members to Figure S10. Coloured points refer to the best runs and follow the colour palette of Figure 4 (thinning rates), while grey dots refer to the discarded runs.

Fig S12: “All variables are computed on a location in the centre of the present NEGIS.” Where exactly?

Now Fig. S4. Included the point on the map of Figure S11.

Fig S12. “Glaciological explanation of the effect of margin retreat on dynamical thinning in the northeast.” You are expecting a lot of many readers to interpret this "Glaciological explanation" with very limited guidance (only reference in text is to the role of enhance basal meltwater production).

Now Fig. S4. We have now added an extensive description of the processes to the figure caption. Also we improved the description of the chain of events responsible for the NGRIP dropdown in the main text, which is now supported by several new figures in the Supplementary Material.

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REVIEWER COMMENTS

Reviewer #2 (Remarks to the Author):

I am not going to repeat the overview of the paper that I have previously provided. Suffice to say that the authors have again made a significant effort to address comments by myself and the other two referees, especially the multiple queries raised by reviewer 4. I remain happy that the overall take home from the paper is important and of significance to a broad community encompassing ice sheets, climate and ice-ocean interactions. It addresses an important issue regarding an explanation for the elevation reduction in the central region of the Greenland Ice Sheet since the LGM and also highlights the dynamism of ice streams over time i.e. the change in configuration of NEGIS across the Holocene.

As previously I have no major substantive issues with the paper but have provided numerous comments on the manuscript and supplementary material both of which I have uploaded with this review. Some of these are minor and some more significant and as with the last version I would say that these really need to be addressed. They detract significantly from the readability and message that the manuscript is trying to convey.
In conclusion: I think that this is, in principle, acceptable for publication pending the numerous corrections that are required to the text and the figures.

Four other points:

1. References – need correct formatting i.e. all in lowercase!

2. Imposition of the switch to cfmin for NEGIS south occurs at 8ka but you describe it controlling thinning throughout the Holocene this statement has a temporal mismatch. This wording needs changed.
3. Maybe I have missed it but I am unclear the relationship between cf, f(min/mid) and cf? Are they different parameters or are they the same? If the former, then their relationships need to be explained and if the latter then they need to be homogenised!
4. There is a mixing of the terminology used to describe the different components of the deglaciation and the area. There seems to be variously:
a. discussion of the northeast margin as a whole i.e. I take this to be the whole ice margin and associated regional ice drainage.
b. discussions about NEGIS as a whole i.e. the ice stream branches and all outlets
c. discussion of the specific contemporary outlets of NEGIS i.e. 79N and ZI. For example see lines 280/81.

While I can see the need for these differentiations it would be really useful if the terminology could be clarified initially and then followed throughout. This would make it much easier to follow.

Reviewer #3 (Remarks to the Author):

The manuscript by Tabone et al. demonstrates a key role for northeast Greenland ice stream dynamics in driving Holocene thinning of the central Greenland ice sheet. Reviewing this manuscript for the third time, I find that the authors have thoroughly addressed all of my concerns and carefully considered all of my suggestions. After considering the other reviews and the authors' responses to them, I think that the authors have done a remarkable job at considering a wide range of feedback and running additional simulations that have only strengthened their core message, clarified their methodology, and made their results more robust. I have read the revised manuscript in detail, and I believe it will be of great interest to a wide range of geoscientists and is perfectly suited to the readership of Nature Communications. I believe that this manuscript, and the approach Tabone et al. have taken herein, represents a novel and exciting step forward for our understanding of the history of the Greenland ice sheet with important implications for the future, and will influence future work in this field for years to come. For these reasons I would advocate strongly for the publication of this manuscript in more or less its present state, subject to considering the extremely minor suggestions included below:

Abstract - I suggest removing the word "the" from the phrase "northeast Greenland triggers the elevation change at the northern summit"

Line 95 - I suggest modifying to: "indicating that up to 73% of the..."

Line 224: I prefer the word "simulations" rather than "runs." They are basically interchangable and it is mostly a preference but I find the former to be slightly more intuitive to non-experts and suggest the authors consider making this change throughout the manuscript.

Line 259: I suggest spelling out RCM. There are already a lot of acronyms and you don't use this one many times, so just spell it out.

Line 287: Suggest rewording "In our simulations, this occurs around 10 kyr..."

Reviewer #4 (Remarks to the Author):

though the authors have addressed most of my concerns, there
are a few key concerns that have not been adequately (or at all)
addressed. I've also noted a few other issues (not sure if due
to revision changes or if I just missed them before, given the
lack of a track changes document).

in detail:

The response to my RSL concerns is self-contradictory:

However, as answered to Reviewer#3, we do not seek here either to
provide a new estimate of the RSL change around the northeast margin
or to validate our model against RSL data.
...
Thus, our modelled RSL curves are here interpreted as a proxy for
margin retreat chronology: a different rate of temperature increase
during the early Holocene affects the timing of deglaciation, thus the
isostatic response to ice unload, thus the RSL change. Figure 5
(previous Fig. 6) is therefore used for this scope, i.e. evaluating
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chronology

You are not just using calculating RSL as a proxy for margin retreat
when you subsequently state:

One could simply show the comparison between the two RSL curves, however we think that an additional comparison against RSL data provides a better metric to evaluate the performance of the two climatologies in the northeast.

You further compound this communicated misconception by the
addition of RMSE scores for modelled RSL versus observations, which
implies that consistency between model and data would have a near
0 RMSE score which is not case when the modelled RSL is biased
by the lack of accounting of Geoidal and non-local loading
effects.

The RSL dataplots are also misplotted, as most of the RSL datapoints
along the NE GRIS coast are either molluscs or terrestrial samples,
giving only 1 way constraints. Eg, most mollusc samples only indicate
sealevel was above the datapoint (assuming mollusc sample was in a
living position and sample elevation was accurately measured),
possibly 100 or more above. If you are going to plot the RSL data, you
need to plot them with indicative error bars.

The p values in figures S6:S8 are meaningless, as they presume
the data follow f(x)= ax+b + independent Gaussian noise which is clearly
not the case for model output. So they need to be removed.

Pinpointing the key driver of ice-sheet retreat in northeast Greenland during the last deglaciation is challenging. Despite simultaneous air and ocean temperature increases around ~15 kyr ago (Fig. S3), sustained simulated submarine melting occurs only later (~13 kyr ago).

This is misleading. As apparent in your fig S3, your subshelf melt
is only active for forced annual ocean T > -0.5C, which is only the
case after ~13 kyr ago. And I do not understand how pinpointing
an aspect of model response to inputs is "challenging", when the SSM
is purely a function of a single input (Tocean) and presumably ice thickness.

@ At the very least, the experimental design and analysis needs to be expanded to
@ include runs with constant basal friction coefficient Cf throughout the run (and
@ for the 3 different values of Cf: fmin, fmid, and 1)
# My above original comment was only partially addressed. In Fig S16,
# though a number of North, and South Branch basal friction
# coefficient forcing scenarious are tested, there is a blatant missing
# case of South Branch continuously at fmin (say with Case 2
# chronologies for the other 2 branches). (unless there is a very
# different response, you could replace the current case 3 by the
# above to retain your 3X2 layout). Without doing this test, I don't
# see how your claim in the abstract:
Fast ice-stream flow caused by transiently imposed reduced basal shear stress
following the northeast retreat explains 55% (± 18%) of the estimated ice thinning,
is supported, especially given that your current case 3, with South branch at

fmed -> fmin has > 100 m thinning at NGRIP

The bed-elevation dependent basal friction coefficient representing bed roughness at the NEGIS.. Such a change in basal roughness, as imposed in Fig. S1, could be due to the effect of sediment transportation and deposition below the stream through glacial drift and subglacial water routing, acting at long timescales.

The description is confusing and inconsistent. Does your imposed
basal friction coefficient change only represent changes in bed
roughness OR does it also represent potential changes in subglacial
water routing OR changes in subglacial drainage mode which can
strongly affect basal water pressure (cavity versus channelized)?
How have you ruled out the latter?

REVIEWER COMMENTS

We thank the three reviewers for taking the time to review our manuscript again. Please, find our point by point answers below.

Reviewer #2 (Remarks to the Author):

I am not going to repeat the overview of the paper that I have previously provided. Suffice to say that the authors have again made a significant effort to address comments by myself and the other two referees, especially the multiple queries raised by reviewer 4. I remain happy that the overall take home from the paper is important and of significance to a broad community encompassing ice sheets, climate and ice-ocean interactions. It addresses an important issue regarding an explanation for the elevation reduction in the central region of the Greenland Ice Sheet since the LGM and also highlights the dynamism of ice streams over time i.e. the change in configuration of NEGIS across the Holocene.

As previously I have no major substantive issues with the paper but have provided numerous comments on the manuscript and supplementary material both of which I have uploaded with this review. Some of these are minor and some more significant and as with the last version I would say that these really need to be addressed. They detract significantly from the readability and message that the manuscript is trying to convey.
In conclusion: I think that this is, in principle, acceptable for publication pending the numerous corrections that are required to the text and the figures.

We thank the reviewer again for their positive and constructive comments. Detailed responses to their remaining points follow below.

Four other points:

1. References – need correct formatting i.e. all in lowercase!

Corrected.

2. Imposition of the switch to cfmin for NEGIS south occurs at 8ka but you describe it controlling thinning throughout the Holocene this statement has a temporal mismatch. This wording needs changed.

We agree with the reviewer, this wording may be confusing. We have now replaced all “throughout the Holocene” as either “during the Holocene” or “during the last 8 kyr” or “during most of the Holocene” throughout the text, depending on the context. Please, refer to the new version of the manuscript for further details.

3. Maybe I have missed it but I am unclear the relationship between cf, f(min/mid) and cf? Are they different parameters or are they the same? If the former, then their relationships need to be explained and if the latter then they need to be homogenised!

We are sorry for this misunderstanding. The friction coefficient c_f can be set either to 1 or to f_{mid} or to f_{min}, depending on the considered stream branch and time (see Fig. S1 and Fig. S16). However, both f_{mid} and f_{min} are perturbed in our experiments, as described in Table S1. To clarify this point we modified this sentence in the Methods: "Here, c_f is reduced to better capture the system of paleo and present ice-streams (Fig. S1) … Therefore, in our simulations c_f can be set either to 1, or f_{mid} or f_{min} depending on the considered NEGIS branch and time in the Holocene (Fig. S1). The parameters f_{mid} and f_{min} are perturbed in our simulations (see Table S1)."

4. There is a mixing of the terminology used to describe the different components of the deglaciation and the area. There seems to be variously:
a. discussion of the northeast margin as a whole i.e. I take this to be the whole ice margin and associated regional ice drainage.
b. discussions about NEGIS as a whole i.e. the ice stream branches and all outlets
c. discussion of the specific contemporary outlets of NEGIS i.e. 79N and ZI. For example see lines 280/81.

While I can see the need for these differentiations it would be really useful if the terminology could be clarified initially and then followed throughout. This would make it much easier to follow.

The definition of the NEGIS (ice stream branches and outlets) is now included in this sentence in the introduction: "The proximity of the 600 km-long Northeast Greenland Ice Stream (NEGIS, defined here as all ice stream branches and all outlet glaciers) to the ice divide at the present, as well as the presence of another well-defined NEGIS-like stream stretching north from the 79N glacier far to the interior during the early Holocene, suggest a possible far-reaching influence of the ice-stream dynamics during the Holocene (Fig. S1)".

For "ice margin" we generally intend the "ice front" (marine terminating or land terminating): thus we changed the term "ice margin" to "ice front" throughout the text to be clearer. The term "margin" now used to refer to the "ice front and regional ice drainage" instead. This is clarified in the Results section as: "We propose that the mechanism behind the early-Holocene GrIS north-central thinning is its dynamic adjustment to the NEGIS-induced inland flow acceleration during margin retreat (i.e., the retreat of the ice front and associated drainage basin)."

Lines 280/281 refer to locations that are not specifically related to a NEGIS outlet glacier but at the ice-sheet current margin position, therefore no changes have been made.

Reviewer #3 (Remarks to the Author):

The manuscript by Tabone et al. demonstrates a key role for northeast Greenland ice stream dynamics in driving Holocene thinning of the central Greenland ice sheet. Reviewing this manuscript for the third time, I find that the authors have thoroughly addressed all of my concerns and carefully considered all of my suggestions. After considering the other reviews and the authors' responses to them, I think that the authors have done a remarkable job at considering a wide range of feedback and running additional simulations that have only strengthened their core message, clarified their methodology, and made their results more robust. I have read the revised manuscript in detail, and I believe it will be of great interest to a wide range of geoscientists and is perfectly suited to the readership of Nature Communications. I believe that this manuscript, and the approach Tabone et al. have taken herein, represents a novel and exciting step forward for our understanding of the history of the Greenland ice sheet with important implications for the future, and will influence future work in this field for years to come. For these reasons I would advocate strongly for the publication of this manuscript in more or less its present state, subject to considering the extremely minor suggestions included below:

We thank the reviewer again for their kind review and we are glad our work was so positively received. Point by point responses follow below.

Abstract - I suggest removing the word "the" from the phrase "northeast Greenland triggers the elevation change at the northern summit"

Corrected.

Line 95 - I suggest modifying to: "indicating that up to 73% of the..."

Corrected.

Line 224: I prefer the word "simulations" rather than "runs." They are basically interchangable and it is mostly a preference but I find the former to be slightly more intuitive to non-experts and suggest the authors consider making this change throughout the manuscript.

Corrected throughout the manuscript.

Line 259: I suggest spelling out RCM. There are already a lot of acronyms and you don't use this one many times, so just spell it out.

Corrected.

Line 287: Suggest rewording "In our simulations, this occurs around 10 kyr..."

Modified.

Reviewer #4 (Remarks to the Author):

Though the authors have addressed most of my concerns, there are a few key concerns that have not been adequately (or at all) addressed. I've also noted a few other issues (not sure if due to revision changes or if I just missed them before, given the lack of a track changes document).

in detail:

The response to my RSL concerns is self-contradictory:

“However, as answered to Reviewer#3, we do not seek here either to provide a new estimate of the RSL change around the northeast margin or to validate our model against RSL data.
...
Thus, our modelled RSL curves are here interpreted as a proxy for margin retreat chronology: a different rate of temperature increase during the early Holocene affects the timing of deglaciation, thus the isostatic response to ice unload, thus the RSL change. Figure 5 (previous Fig. 6) is therefore used for this scope, i.e. evaluating the performance of the two climatologies in shaping the deglaciation chronology”.

You are not just using calculating RSL as a proxy for margin retreat when you subsequently state: “One could simply show the comparison between the two RSL curves, however we think that an additional comparison against RSL data provides a better metric to evaluate the performance of the two climatologies in the northeast.”

We do interpret the RSL curve as a proxy for margin retreat. However, as stated before, since the timing of the margin retreat is primarily driven by the timing of the Holocene temperature increase used to force the model, such RSL curves can also be interpreted as tools to evaluate “the performance of the two climatologies in shaping the deglaciation chronology”. We think we already gave a detailed answer to this concern in the previous review round.

You further compound this communicated misconception by the addition of RMSE scores for modelled RSL versus observations, which implies that consistency between model and data would have a near 0 RMSE score which is not case when the modelled RSL is biased by the lack of accounting of Geoidal and non-local loading effects.

In spite of not capturing the non-local and geoidal effects, the comparison between modelled and observed RSL is still informative as argued below and in the previous reviews. The RMSE was added to each panel as requested by Reviewer #3 in the previous submission.

The RSL dataplots are also misplotted, as most of the RSL datapoints along the NE GRIS coast are either molluscs or terrestrial samples, giving only 1 way constraints. Eg, most mollusc samples only indicate sealevel was above the datapoint (assuming mollusc sample was in a living position and sample elevation was accurately measured), possibly 100 or more above. If you are going to plot the RSL data, you need to plot them with indicative error bars.

The p values in figures S6:S8 are meaningless, as they presume the data follow f(x)= ax+b + independent Gaussian noise which is clearly not the case for model output. So they need to be removed.

We are sorry we couldn't convince the reviewer in our last response, especially since the other two reviewers were positive about this analysis. We still think that a comparison between the different RSL curves is a valid tool to evaluate the performance of the two paleo climatologies through how they shape the timing of the margin retreat, however we decided to remove Figure 5 and its description from the main manuscript to avoid further misinterpretations from the reviewer. Now the two climatologies are evaluated only by comparing the observed to the modelled deglaciation timings from sediment cores in the Northeast (Figure 4).

"Pinpointing the key driver of ice-sheet retreat in northeast Greenland during the last deglaciation is challenging. Despite simultaneous air and ocean temperature increases around ~15 kyr ago (Fig. S3), sustained simulated submarine melting occurs only later (~13 kyr ago)." This is misleading. As apparent in your fig S3, your subshelf melt is only active for forced annual ocean T > -0.5C, which is only the case after ~13 kyr ago. And I do not understand how pinpointing an aspect of model response to inputs is "challenging", when the SSM is purely a function of a single input (Tocean) and presumably ice thickness.

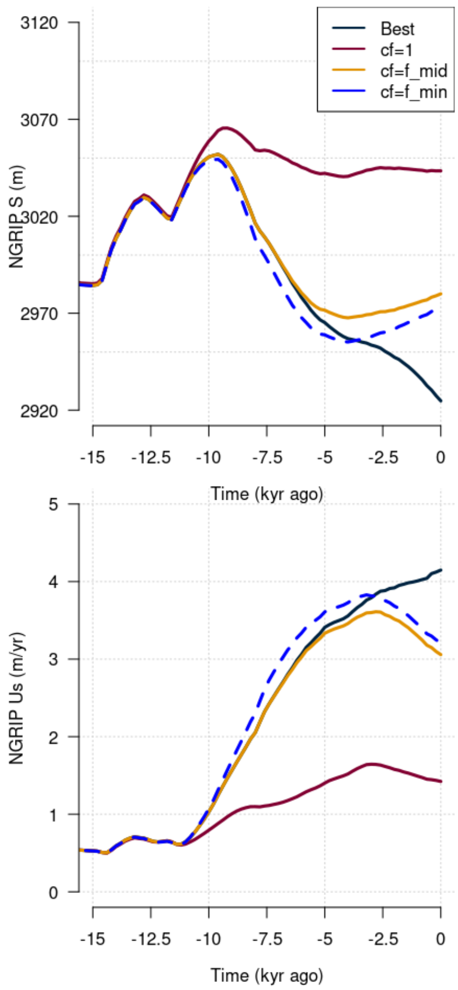
Yes, subshelf melting starts in the northeast only when oceanic temperature anomalies increase by 2K after 13.3 kyr ago. This is due to how our parameterisation is built (Eq. 7), with B_{ref}=1 m/yr, κ=9.6 m/yr/K (for the best run) and an imposed oceanic temperature anomaly at the Northeast from 13.3 kyr ago equal to +2K (See Methods). We have now modified the sentence to clarify this point: "Simultaneous air and ocean temperature increases take place around ~15 kyr ago (Fig. S3), but submarine melting only occurs from 13.3 kyr ago, when the oceanic temperature anomalies increase by 2K from their glacial value (see Methods).". We also removed the sentence "Pinpointing the key driver of ice-sheet retreat in northeast Greenland during the last deglaciation is challenging".

@ At the very least, the experimental design and analysis needs to be expanded to include runs with constant basal friction coefficient Cf throughout the run (and for the 3 different values of Cf: fmin, fmid, and 1). My above original comment was only partially addressed. In Fig S16, though a number of North, and South Branch basal friction coefficient forcing scenarious are tested, there is a blatant missing case of South Branch continuously at fmin (say with Case 2 chronologies for the other 2 branches). (unless there is a very different response, you could replace the current case 3 by the above to retain your 3X2 layout). Without doing this test, I don't see how your claim in the abstract:
"Fast ice-stream flow caused by transiently imposed reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning,"

is supported, especially given that your current case 3, with South branch at fmed -> fmin has > 100 m thinning at NGRIP.

The reviewer's concern is actually already tackled in our previous response. In fact, Figure S13 shows the NGRIP surface elevation and velocity for the three above mentioned cases: constant basal friction coefficient c_f throughout the run for all branches and for the three different values of c_f: f_{min}, f_{mid} and 1. This analysis was done in addition to Figure S16, which instead explores the different types of switch between branches. In Figure S16 we show that the northern and the central branches have little influence in the NGRIP dropdown, while it is the southern branch which is driving the NGRIP surface elevation change (please refer to our previous answer for further details). In short, Figure S13 in the Supplementary material shows the effect of having a constant c_f set at the southern branch for the whole run.

We paste here the text we wrote in our previous response to the reviewer: "... the imposition of an additional linear reduction in c_f for the southern branch for the last part of the Holocene is needed to ensure that, during the Neoglacial, the ice stream velocities remain high even though the ice sheet re-advances. This can be seen in the figure below, where surface elevation drop and velocities at NGRIP are shown for the best run (in solid black) and for the three runs, where c_f=1, c_f=f_{mid} and c_f=f_{min} are kept constant for the whole run.



The claim in the abstract “Fast ice-stream flow caused by transiently imposed reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning” is supported by a statistical analysis we perform with an ensemble of simulations following the upper-right switching scenario shown in Figure S16.

Moreover, as reported in the previous response to reviewers: “The linear reduction of c_f at the late Holocene is of course a technical workaround to keep the stream in its present form until the end of the run, as current numerical models cannot represent the activation of the stream fully physically. We decide to apply this technical workaround as we know from reconstruction that the NEGIS should have been active and with a present-like form for the last 8 kyr (Gerber et al., 2021). It should be noted, however, that the reduction from f_{mid} to f_{min} in the best run is minimal as f_{mid}=0.30, and f_{min}=0.26, thus c_f is reduced by only a ~13% of its initial value.”

To explicitly clarify that the weighted mean modelled NGRIP ice thinning depends, of course, on the nature of how c_f is imposed at the NEGIS, as extensively explained in our previous resubmission, we decided to modify the sentence to “In our simulations, fast ice-stream flow caused by transiently imposed reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning”. We also think that the previous reviewer’s suggestion of including the expression “transiently imposed reduced basal shear stress” helps to clarify this claim.

Please, refer to the previous answer to reviewers for further details on why we decided to apply such a switch and why it is consistent with evidence.

“The bed-elevation dependent basal friction coefficient representing bed roughness at the NEGIS.. Such a change in basal roughness, as imposed in Fig. S1, could be due to the effect of sediment transportation and deposition below the stream through glacial drift and subglacial water routing, acting at long timescales.” The description is confusing and inconsistent. Does your imposed basal friction coefficient change only represent changes in bed roughness OR does it also represent potential changes in subglacial water routing OR changes in subglacial drainage mode which can strongly affect basal water pressure (cavity versus channelized)? How have you ruled out the latter?

The imposed changes in the basal friction coefficient represent any transient processes at the base of the ice sheet not explicitly represented in the model. These could include changes to bed roughness due to sediment transport and deposition, as well as changes in subglacial drainage and routing. We modified the paragraph to avoid confusion as: “The bed-elevation dependent basal friction coefficient strongly controls the ice-stream basal conditions, hence the NGRIP thinning (Fig. S8). The imposed changes in the basal friction coefficient (Fig. S1) represent any transient processes at the base of the ice sheet not explicitly represented bin the model. These could include changes to bed roughness due to sediment transport and deposition, as well as changes in subglacial drainage and routing.”.

REVIEWERS' COMMENTS

Reviewer #2 (Remarks to the Author):

I am not going to repeat the overview of the paper that I have previously provided. Suffice to say I remain happy that the overall take home from the paper is important and of significance to a broad community encompassing ice sheets, climate and ice-ocean interactions. It addresses an important issue regarding an explanation for the elevation reduction in the central region of the Greenland Ice Sheet since the LGM and also highlights the dynamism of ice streams over time i.e. the change in configuration of NEGIS across the Holocene.

As previously I have no major substantive issues with the paper but have provided some minor suggestions on the manuscript and supplementary material both of which I have uploaded with this review. I flag a few minor issues below as well.

In conclusion: I think that this is now acceptable for publication.

Minor comments:
Fig S9 – bottom panel - the three colours represent the three GHF prescribed in the model expts – 63, 50 and 45 mWm-2. So where then is the "uniform GHF of 50 mWm-2) applied to? Is it the rest of the catchment beyond the ice stream? Please clarify.

Fig 3 right panel – indicate the grey line is 1:1. What are the grey dotted lines?

Fig S17 = line 4 of the caption – there is no lower-left panel?

Line 167 do you mean northeast ice sheet as in the line above?

Line 182 – replace dropdown with lowering?

Lines 165 to 183 – see highlighted sections – consistency in description please

Line 217 – E_{stream} – Is not shown as a variable in Table S1

Line 238-39 – the is awkward please reword.

Reviewer #3 (Remarks to the Author):

The manuscript "Holocene thinning in central Greenland controlled by the Northeast Greenland Ice Stream" by Tabone et al. presents a test of the hypothesis that the activation of fast ice-flow in northeast Greenland during the early Holocene significantly contributed to ice thinning in the deep interior of the ice sheet. The manuscript has been strengthened considerably in response to comments from previous reviewers, and the present version represents a compromise that preserves the main points of the study while responding to divergent input from different reviewers. The central difference between this version and the last one I reviewed is that one of the figures and some of the arguments related to evaluating their model simulations against relative sea-level data has been removed. This is unfortunate because I found this analysis compelling, especially in terms of illustrating the implications of using different climatologies to force the ice-sheet model. I see that this choice was made in response to comments from another reviewer and I understand the author's decision to remove this analysis. It does not compromise the key take-aways of the paper at all.

In general I find the methodology robust, the analysis careful and detailed, and the interpretation compelling. As I have said before I think this is an important contribution that will be of broad interest to the glaciology and paleoclimate communities, among others interested in the history of Greenland. As such, I encourage publication in its present state.

I have a few minor suggestions that are purely for readability. I want to emphasize that these are all purely suggestions and up to the discretion of the authors to consider as they wish. I have read this paper now a number of times, so some of this may be that I am just fatigued of seeing the same sentences – which I am sure the authors themselves understand. But, there are some places where I feel that the language could be slightly adjusted to draw the reader in more. These suggestions are:

Abstract – I think that the beginning could be strengthened to be more clear. I've suggested a rewrite to the first several sentences that I think strengthens the logic; feel free to take or leave any of it. The second half is unchanged (from "In our simulations…").

Suggested rewrite: Ice-core records from the interior of the Greenland ice sheet suggest widespread thinning during the Holocene. However, the recurring underestimation of this thinning in numerical models raises concerns about the both the veracity of such reconstructions and the reliability of glaciological models. Recent work suggests the 8000-year-old Northeast Greenland Ice Stream (NEGIS), including a now-extinct northern tributary, may have been an early influence on Greenland ice-sheet dynamics. Yet, the inaccurate reproduction of NEGIS-like dynamics in most models hampers investigation of whether this feature played a role in Holocene ice-sheet thinning. Here we show that grounding-line retreat in northeast Greenland triggers elevation changes at the northern summit via ice-dynamic effects modulated by the paleo NEGIS system. In our simulations, fast ice-stream flow caused by transiently imposed reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning, showing that ice-stream dynamics is one of the main drivers of the NGRIP Holocene surface elevation drop. Our findings show that the ice-flow in northeast Greenland plays a large role in ice-surface elevation changes in central Greenland.
Line 16/17 – the Holocene is classically defined as the last 11,700 years; consider adjusting the language here accordingly?

Line 19 "…correctly capturing this thinning…"

Line 21 – check capitalization of "High Arctic" and consider instead "high latitude"

Line 29 – "…far-reaching influence of ice-stream dynamics…"

Line 39 remove "To this end,"

Line 41 "see Methods for details"

Line 47 suggest rewrite: "We find that NGRIP thinning is initiated by NEGIS onset, enhanced by upstream propagation following early Holocene ice-sheet retreat in northeast Greenland, and maintained by increased meltwater production and reduced basal friction."

Line 50 suggest replace "fostered" with "achieved"

Line 57 "extends" instead of "extended" as I believe you are talking about your simulations here, which you talk about in present tense throughout

Line 140 avoid "Holocene inception" – use "early Holocene" or "beginning of the Holocene" instead

Line 303 - this paragraph is confusing as currently written. Instead of "last phase of the Holocene" I suggest you say "the last 2ka" or however long you are talking about when you refer to "late-Holocene." You could also say, "…greater elevation decrease throughout the last phase of the Holocene (e.g. the last ~2ka)."

The last paragraph of the manuscript is fantastic. It concisely summarizes your findings and interpretations, and makes their importance crystal clear for future studies. It ends the main manuscript on a great note!

Reviewer #4 (Remarks to the Author):

I'm happy to report that all of my remaining concerns have been addressed by the authors. The study jointly constrains the past behaviour and impact of the NEGIS ice stream on NGRIP Holocene elevation. This will be of interest to much of the paleo community along with those interested in ongoing and future ice sheet contributions to sea level rise. I see it as therefore appropriate for publication in Nature Communications.

REVIEWER COMMENTS

We thank the three reviewers for taking the time to review our manuscript again. Please, find our point by point answers below.

Reviewer #2 (Remarks to the Author):

I am not going to repeat the overview of the paper that I have previously provided. Suffice to say I remain happy that the overall take home from the paper is important and of significance to a broad community encompassing ice sheets, climate and ice-ocean interactions. It addresses an important issue regarding an explanation for the elevation reduction in the central region of the Greenland Ice Sheet since the LGM and also highlights the dynamism of ice streams over time i.e. the change in configuration of NEGIS across the Holocene.

As previously I have no major substantive issues with the paper but have provided some minor suggestions on the manuscript and supplementary material both of which I have uploaded with this review. I flag a few minor issues below as well.

In conclusion: I think that this is now acceptable for publication.

We thank the reviewer again for their positive and constructive comments. Detailed responses to their remaining points follow below.

Minor comments:
Fig S9 – bottom panel - the three colours represent the three GHF prescribed in the model expts – 63, 50 and 45 mWm-2. So where then is the "uniform GHF of 50 mWm-2) applied to? Is it the rest of the catchment beyond the ice stream? Please clarify.

Now modified to "ice acceleration at NGRIP modelled for different GHF fields prescribed in the model (Martos et al., 2018 (M18), Shapiro & Ritzwoller 2004 (S04) and a uniform GHF of 50 mW/m2, applied to the whole GrIS) (bottom panel)."

Fig 3 right panel – indicate the grey line is 1:1. What are the grey dotted lines?

We added "The 1:1 line in the right panel is shown in grey. Dotted and solid error bars represent uncertainties in deglaciation timings for sediment cores and moraines, respectively."

Fig S17 = line 4 of the caption – there is no lower-left panel?

True. Corrected.

Line 167 do you mean northeast ice sheet as in the line above?

Yes. Corrected to "northeast ice sheet's retreat".

Line 182 – replace dropdown with lowering?

Done.

Lines 165 to 183 – see highlighted sections – consistency in description please

Replaced with “northeast ice sheet”.

Line 217 – Estream – Is not shown as a variable in Table S1

Tables S1 only shows the parameters that are perturbed in our ensemble of simulations. Instead, Estream is kept fixed for all simulations and equal to 1, as explained in the Methods. Still, we are grateful to the reviewer as this comment helped us to spot a typo in the Methods: Eshear is set to 1 and not to 0.7 as previously written. We added “Still, our simulations show higher ice viscosity for ice stream regions (Estream=1, Eshear > 1, see Methods and Table S1)” to clarify this point.

Line 238-39 – the is awkward please reword.

Rephrased as “This would agree with the presence of a higher mantle viscosity in central Greenland”.

Reviewer #3 (Remarks to the Author):

The manuscript “Holocene thinning in central Greenland controlled by the Northeast Greenland Ice Stream” by Tabone et al. presents a test of the hypothesis that the activation of fast ice-flow in northeast Greenland during the early Holocene significantly contributed to ice thinning in the deep interior of the ice sheet. The manuscript has been strengthened considerably in response to comments from previous reviewers, and the present version represents a compromise that preserves the main points of the study while responding to divergent input from different reviewers. The central difference between this version and the last one I reviewed is that one of the figures and some of the arguments related to evaluating their model simulations against relative sea-level data has been removed. This is unfortunate because I found this analysis compelling, especially in terms of illustrating the implications of using different climatologies to force the ice-sheet model. I see that this choice was made in response to comments from another reviewer and I understand the author’s decision to remove this analysis. It does not compromise the key take-aways of the paper at all.

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We thank the reviewer again for their kind review and we are glad our work was so positively received. Point by point responses follow below.

I have a few minor suggestions that are purely for readability. I want to emphasize that these are all purely suggestions and up to the discretion of the authors to consider as they wish. I have read this paper now a number of times, so some of this may be that I am just fatigued of seeing the same sentences – which I am sure the authors themselves understand. But, there are some places where I feel that the language could be slightly adjusted to draw the reader in more. These suggestions are:

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Suggested rewrite: Ice-core records from the interior of the Greenland ice sheet suggest widespread thinning during the Holocene. However, the recurring underestimation of this thinning in numerical models raises concerns about both the veracity of such reconstructions and the reliability of glaciological models. Recent work suggests the 8000-year-old Northeast Greenland Ice Stream (NEGIS), including a now-extinct northern tributary, may have been an early influence on Greenland ice-sheet dynamics. Yet, the inaccurate reproduction of NEGIS-like dynamics in most models hampers investigation of whether this feature played a role in Holocene ice-sheet thinning. Here we show that grounding-line retreat in northeast Greenland triggers elevation changes at the northern summit via ice-dynamic effects modulated by the paleo NEGIS system. In our simulations, fast ice-stream flow caused by transiently imposed reduced basal shear stress following the northeast retreat explains 55% (± 18%) of the estimated ice thinning, showing that ice-stream dynamics is one of the main drivers of the NGRIP Holocene surface elevation drop. Our findings show that the ice-flow in northeast Greenland plays a large role in ice-surface elevation changes in central Greenland.

We thank the reviewer for their rewriting. We decided to change the abstract as they suggested, as we found it more compelling.

Line 16/17 – the Holocene is classically defined as the last 11,700 years; consider adjusting the language here accordingly?

We removed the term "Holocene".

Line 19 "…correctly capturing this thinning…"

Corrected.

Line 21 – check capitalization of “High Arctic” and consider instead “high latitude”

This was a change made to satisfy another reviewer’s request. We decided to leave it as it is.

Line 29 – “…far-reaching influence of ice-stream dynamics…”

Corrected.

Line 39 remove “To this end,”

Corrected.

Line 41 “see Methods for details”

Corrected.

Line 47 suggest rewrite: “We find that NGRIP thinning is initiated by NEGIS onset, enhanced by upstream propagation following early Holocene ice-sheet retreat in northeast Greenland, and maintained by increased meltwater production and reduced basal friction.”

Changed accordingly.

Line 50 suggest replace “fostered” with “achieved”

We kept it as it is, as the NEGIS upstream propagation can in principle occur without reducing the basal friction beneath the NEGIS. The ice stream would simply not propagate as far in the interior.

Line 57 “extends” instead of “extended” as I believe you are talking about your simulations here, which you talk about in present tense throughout

Corrected.

Line 140 avoid “Holocene inception” – use “early Holocene” or “beginning of the Holocene” instead

Changed to “early Holocene”.

Line 303 - this paragraph is confusing as currently written. Instead of “last phase of the Holocene” I suggest you say “the last 2ka” or however long you are talking about when you refer to “late-Holocene.” You could also say, “…greater elevation decrease throughout the last phase of the Holocene (e.g. the last ~2ka).”

Corrected.

The last paragraph of the manuscript is fantastic. It concisely summarizes your findings and interpretations, and makes their importance crystal clear for future studies. It ends the main manuscript on a great note!

We thank the reviewer again for their kind comments.

Reviewer #4 (Remarks to the Author):

I'm happy to report that all of my remaining concerns have been addressed by the authors. The study jointly constrains the past behaviour and impact of the NEGIS ice stream on NGRIP Holocene elevation. This will be of interest to much of the paleo community along with those interested in ongoing and future ice sheet contributions to sea level rise. I see it as therefore appropriate for publication in Nature Communications.

We thank the reviewer for their final positive comments. We are glad we could satisfy their requests and make the manuscript clearer.