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Peer Review File

Recent ice melt above a mantle plume track is accelerating the uplift of Southeast Greenland

Corresponding Author: Dr Maaike Weerdesteijn

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Weerdesteijn,

Your manuscript titled "Recent ice melt above a mantle plume track is accelerating the uplift of southeast Greenland" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. Specifically, we ask you to:

1. Improve the robustness of your analysis by considering the plume track's impact on horizontal crustal motions and uplift rates from GNSS sites within the ice loading area and provide analysis of bedrock uplift rates at earlier times.

2. Provide a comprehensive explanation for the 'elastic' loading and viscosity approach used in numerical models and its performance during altimetric mass balance after 1992.

3. Provide strong evidence for the magnitude and geometry of the inferred asthenospheric low-viscosity zone and compellingly demonstrate the compatibility of plume tracks and low lithospheric thicknesses and viscosities from GNSS observations with longer timescale constraints, such as Holocene sea-level records.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

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Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Dr Alireza Bahadori
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This manuscript finds that incorporating a low viscosity upper mantle plume track into 3D GIA models for southeast Greenland can help to explain rapid uplift rates that have been observed in the region. The conclusions are novel as regional-scale GIA modeling that accounts for strong heterogeneity in solid Earth structure has not been completed for SE Greenland. At this point, very few studies have considered the impact of regional-scale variability in solid Earth structure on GIA; therefore, findings from this study should motivate other GIA modeling studies to consider higher-resolution solid Earth structure. Although it may not be explicitly stated in the manuscript, this study also points towards the importance of improving constraints on solid Earth structure in SE Greenland for advancing our understanding of GIA in the region. I have included general and line specific comments below.

General Comments:

1. Is it possible to consider the effect of the plume track on horizontal crustal motions as well? Several GIA studies have shown that accounting lateral heterogeneity has a large impact on predicted horizontal crustal motions. It would strengthen the manuscript if the authors investigated the effect of the plume track on horizontal crustal motions. Of course, GIA model predictions could also be compared to observed horizontal crustal motions.

2. While the authors justify their chosen solid Earth configurations based on findings from [63, 64, 9], the argument for adopting the given solid Earth configurations could be strengthened if the authors consider findings from seismic imaging studies as well, such as Pourpoint et al. (2018). Using various seismic methods, the Pourpoint et al. (2018) study places constraints on lithospheric structure across Greenland. The authors acknowledge that solid Earth structure is not well-constrained in this region, which makes it especially critical to pull constraints on solid Earth structure from all available geophysical studies.

3. There are several GNSS sites located just outside of the “area of interest” but within the “ice loading area” shown in Figure 1. Why were uplift rates from these sites not considered in the analysis? Might it be informative to compare predicted uplift rates to observed rates at these sites even if they are located just outside the “area of interest”?

4. As I alluded to earlier, it would probably be fruitful to explicitly state that results from this study should motivate further efforts to place improved constraints on solid Earth structure in SE Greenland.

Line specific comments:

L 77-78; L 582-583: How were the plume track widths of 200, 400, and 600 km chosen? Were these plume track widths also based on findings from the Martos et al. [9] study?

L 79: A reference to justify the adopted lithospheric thicknesses would be helpful (i.e., 30, 60, 90 km) – perhaps look to the Pourpoint et al. (2018) study

L 80: “lithospheric” should be just “lithosphere”

L 584: Please add units to 5x1020 here. Also please add a reference to justify the adopted upper mantle viscosity (for example reference Lecavalier et al., 2014).

L365: What is the viscosity adopted for this full low-viscosity layer? Is it 1x1018? Please make it clearer what the viscosity of the low-viscosity layer is.

L 569 Earth structure and variations section: Can you please specify the lower mantle viscosity somewhere in this section?

Reviewer #2 (Remarks to the Author):

Review of “Recent ice melt above a mantle plume track is accelerating the uplift of southeast Greenland” by Maaike F. M. Weerdesteijn and Clinton P. Conrad

General Comments

This manuscript details a new, state of the art computational model of ice change-related loading to better explain the rapid uplift rates at 4 sites in west Greenland that has been given in past research. These sites were identified in a Science Advances paper by Abbas Khan in 2016 and modeled via a locally applicable 1-D Earth structure. The work by

Weerdesteijn and Conrad presented here is clearly a nice, and very rational, advancement of the modeling, and they appear to have taken sufficient care in constructing the model ice load changes, that I think the work deserves to be published.

I do, however, have several concerns that need to be answered. Since my Detailed Comments flush these issues out with considerable granularity, I will only summarize the most important one here. The explanation of the 'elastic' loading is confusing. A Green's function approach is discussed as the technique for the altimetric period, yet on lines 456-457, it is stated that the same linear viscosity is used for all three modeled time periods. It is important, if not critical, to clarify. I assume the model uses Maxwell viscoelasticity, which is a simple linear viscoelasticity. Other linear viscoelastic constitutive relations would include the one proposed by Paxman and the one used in a recent paper by Ivins in J. Geodesy. These are 'higher order' linear viscoelastic rheologies, and are distinct from a non-linear rheology such as recently used by Blank et al.

I place an indicator (*) on each detailed comment that I believe are essential to properly address in the revision.

Detailed Comments

Abstract.

Line 23. Sentence beginning "This rapid viscous response … is not usually considered …" is incorrect. There is at least 25 years of literature discussing this. I suggest deleting this sentence and then picking up with the next, changing it to "When such regions occur beneath marine terminating outlet glaciers the vertical uplift may influence the future stability of the entire ice catchment basin upstream."

In reference to the Discussion, I have more to say about this.

Introduction: I am surprised that no mention is made of the novel paper Mark Fahnestock published in Science.

Line 29. Generally, the Earth is a planet, and as such it is capitalized, like Venus, Io, Europa, etc.

Line 34. A recent study by Kappelsberger reveals that ICE-6G is a somewhat poor choice for an ice history forward model. Huy3 might be a better choice (Lecavalier), according to these authors. Possibly consider adding the Kappelsberger to the reference list.

Lines 49-52. This statement about local as opposed to distant viscosity was shown (in detail) also by Klemann et al in modeling the 3-D viscosity around Patagonia. Please add this to the reference [15].

Lines 89-90. The study by [25] is a perfectly good one, but it is but one among a number. Ines Otosaka has presented an intercomparison of altimetry-based Greenland mass balance over 2002-2019 in her Figure 3b. Please give the error estimates that are reported therein. She also gives mass balance estimates from altimetry on an annual basis.

Line 99. " … uplifts all of Greenland". For a weak zone, one might expect that in some part of the solution space there will be subsidence that results from this more recent load changes (e.g., see Figure 3b of Ivins and James 2004, from Patagonia models). Might recommend saying "dominantly uplifts Greenland crust".

Line 116. My recollection is that these are "formally adopted IGS station acronyms".

Lines 146-148. This was shown ("… greatest uplift rates occur near rapidly deglaciating regions …") by Lange et al. (2014) for Patagonia: a case wherein the location of the most rapidly changing ice is unambiguous. I think this should be noted.

Line 165. Might suggest replacing the reference to "second millennium" to "Post-1000 CE", and maybe even a more compact acronym of the authors choosing, since it is repeated throughout the manuscript.

Line 172. I rather tend believe this statement, generally. Except that if one adopts the view taken by the ANU series of models and analysis, in which the lower mantle viscosity increases by over one order of magnitude. If this is a robust statement, then it would need quantification by actual modeling viscosity profiles that deviate substantially from VM5i, as used here. I leave it to the authors as to how to address this issue. It might be an important caveat, and maybe not. It's an open question.

Line 175. I suggest rephrasing the sentence, as the " …, followed by …" is confusing. Perhaps start: "The largest predicted uplift rates occur when imposing a thin lithosphere …"

Line 192-193. This seems contrary to lines 172-173. I think you mean for the last glacial cycle. Better call that out explicitly.

Lines 197-206. This material could be shortened. The main point is to deal with the four GNSS stations, and this is somewhat of a side note. Figure 2 might be deleted or set aside to the Supplemental material.

Comment on the Figure captions: The caption titles in bold seem very long.

Lines 328 -335. Figure 6 is very well conceived. Can the authors explain how these results might change had compressibility and density stratification been imposed in the model? Incompressibility tends to be biased to lower prediction of uplift rate, for example.

Lines 374-375. “can have implications” is too vague. Perhaps phrase as “can change the force balances controlling flow at the site of an adjacent ..”

Line 381. The value of 1 x 1018 seems more appropriate to a region where there is active volcanism (Iceland, Patagonia, northern Antarctic Peninsula, Svalbard, Alaska), and that therefore a map-view of the prediction at 5 x 1018 might also be appropriate.

*Line 426. So far in the paper there has been no explanation of how the model is run in the period of altimetric mass balance (i.e. after 1992). In section (lines 559-567) that comes in the Methods Section, it is explained that a Farrell’s Green’s function method (elastic) is used. If this is what is used over 1992-present, then the predicted results are incorrect, and the models will have to be rerun with full viscoelasticity. This is because at such a low viscosity, the viscoelastic effects are dominated by the viscous element. For example, see the statement by Kieruff et al on page 1529, 1st column, second to last paragraph.

“If low viscosity values in the range of 1018 Pa s as found by Mémin et al. (2014) can be confirmed, then also our PDIM investigation should repeated with viscoelastic modelling instead of a purely elastic one. This is because such low viscosities lead to an early (after a few years only) viscous response of the Earth to PDIM Nield et al. (e.g. 2014).”

Having done this modeling many times (see Lange et al, Ivins et al. 2011), and noting that Spada et al (2011) point out the same thing, this is critical to the correct prediction. (Also please see the recent paper by Ivins et al. (2023) Figures 5-9, dotted lines are uplift rates). So, the question remains: is viscoelasticity included in the ice change driven solid Earth deformation during the altimetric period?

Line 563. Green is a man with a function.

Lines 570-573. For runs with SELEN, was the density also averaged, and was the core shrunk to near zero effective radius? Some explanation is needed.

Comment on the Supplementary Material. Work by Divine et al. tend to support the regional cooling that began at about 1400 CE, as in your model.

*Lines 419-426. “This finding has implications …”. Sorry, the fact is that many of these areas have been both measured with GNSS and modeled with both 2nd Millenium changes, and 20th Century deglaciation along with contemporary mass balance (from altimetry, GRACE and IOM methods). These include papers by Larsen for Alaska and Dietrich/Lange for Patagonia and Ivins et al 2011 for the northern Antarctic Peninsula.

*Lines 433 to 449, starting “Such large uplift rates can have implications … for local glacier dynamics …” All of this is speculation. And I say this because no one has studied the types of feedback mechanisms that are computed in various simulations of Thwaites, Pine Island (and by Whitehouse for the Weddell Sea) for Greenland. In fact, Jason Briner’s recent numerical model was run with coupling capability (Briner et al, Nature vol. 586, 2020), but they found that solid Earth deformation did not influence the glacier/ice sheet evolution. However, what can (and should) be stated is that given the very low viscosity values near 1018 Pa s, FUTURE numerical simulations should consider rapid vertical motions that may rapidly alter topography, beds slopes, and ultimately play a role in controlling the rates of ice mass discharge from the Greenland interior.

*Lines 457. The work of Paxman et al. made no attempt to predict uplift but used a sophisticated ‘rheological calculator’ developed by Chris Havlin that simultaneously models the seismic regime, and within that framework it is possible to predict viscosity appropriate to a 100-year time scale. Also, Adhikari pointed to a discrepancy in prediction using the standard viscosity models of GIA, thus indicating either transient viscosity, or the need to expand the RSL data set. Neither of these studies used transient rheological models, as are routinely used in models of post-seismic relaxation of large to great earthquakes. What I recommend as an improved discussion is that – for these four stations - it is possible to appeal to a Maxwell viscoelasticity that is logically posed with a laterally varying low viscosity channel that tracks the plume path. In other words, there really isn’t much of a straw man (transient) to beat on yet . However, the laboratory data sets, the necessity to satisfy tidal data, and the post-seismic relaxation studies, make transient viscosity quite compelling since Maxwell models can’t handle them.

Reference List:

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Otosaka, I. N. et al. (2023) Mass balance of the Greenland and Antarctic ice sheets from 1992 to 2020, Earth System Science Data,15, 4, 1597-1616, https://essd.copernicus.org/articles/15/1597/2023.

Spada, G., Colleoni, F. and Ruggieri, G., (2011) Shallow upper mantle rheology and secular ice sheet fluctuations, Tectonophysics, 511, 89–98.

Reviewer #3 (Remarks to the Author):

See comments in attached pdf.

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Decision Letter:

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Dear Dr Weerdesteijn,

Your revised manuscript titled "Recent ice melt above a mantle plume track is accelerating the uplift of southeast Greenland" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers 2 and 3. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I am satisfied with the authors' response and edits based on the reviewers' comments.

Reviewer #2 (Remarks to the Author):

I laud the authors for such an extensive res=vision and very thorough response to my review and to the other two reviews. I think the paper is ready to be accepted. I just have a couple changes that I ask for. From my own review and the author response:
1.
Item *25.

"… constrained by rock deformation studies …" should be changed to " … constrained by laboratory deformation studies …" since they used granular borneol, which is not a rock.

2.
The newly stated (in response to comments from Rev 3)
"Our results similarly employ a simple (linear) viscoelastic rheology law across timescales ranging from thousands of years to decades, but also emphasize the importance of lateral variations consistent with the Iceland plume track."

Should be changed to

“Our results similarly employ a classical linear Maxwell viscoelastic rheology law across timescales ranging from thousands of years to decades, but also emphasize the importance of lateral variations consistent with the Iceland plume track.”

The rationale for the above nomenclature change is as follows:

Maxwell rheology can assume a power-law dependency (see papers by Wouter van der Wal and Patrick Wu). And transient rheology, as formulated by Yamauchi and Takai (used by Paxman), is actually a linear viscoelastic solid and forms the input to Harriett Lau’s new Love number transient viscosity calculator. Also linear rheology is assumed in the compressible version of the Jackson and Faul formulation of transient viscosity now implemented by Lambert Caron in the ISSM Love number calculator, wherein the spherical self-gravitating Earth problem is solved similar to the Lau formulation. To us geodynamics modelers these seem ‘complicated’, but to the mineral physics community they are, nonetheless, “simple linear viscoelastic rheologies”.

This seems like a small technical point, but I think keeping the terminology correct, is the best practice.

With these small changes implemented, I strongly support publication of the manuscript in Communications Earth & Environment.

Reviewer #3 (Remarks to the Author):

The authors have done an impressive and thorough job of addressing my comments and those of the other reviewers. I’m very grateful for their careful consideration of my arguments and I think the extra analysis they’ve carried out has significantly strengthened their conclusions.

I have a few minor edits and additions that I think it would be useful for the authors to include (see below), but I am now very happy to see this paper published.

Remaining Comments

1) Low-viscosity Maxwell and transient rheology could both explain the sea-level and GPS data:

I suggested that the authors looked at Holocene relative sea-level (RSL) reconstructions within the study area because, if transient rheology were operating beneath Greenland, I was expecting that fitting rates of early Holocene RSL fall would require a higher optimal viscosity value than fitting the GNSS datasets. This expectation was based on a presumption that the Last Glacial Maximum–to–present deglaciation trends measured by the RSL data would have longer characteristic timescales than the recent ice mass loss trends observed by satellites. What I hadn’t realised is that the rate of ice mass loss between ~11 and 8 ka in ICE_6G is approximately equal to the measured present-day rate of deglaciation (Briner, 2022, Oceanography). As a result, even if transient rheology were active beneath Greenland, the effective viscosity required to fit the 8–11 ka average RSL fall/bedrock uplift rate and the GNSS data would be expected to be roughly equivalent. It therefore seems that this particular test (frustratingly!) cannot discriminate between Maxwell rheological models that include a low-viscosity (~1018–1019 Pa s) layer and transient models. Confirming or falsifying the presence of such a low-viscosity zone beneath most of SE Greenland will therefore require either more detailed post-8 ka RSL histories or independent steady-state viscosity estimates (e.g., from improved seismological, magnetotelluric, and/or petrological constraints).

Could this point about not being able to discriminate between Maxwell and transient rheologies based on early Holocene RSL rate and GNSS data alone be acknowledged somewhere in the “Complex Rheologies” section? I feel this is an important point to reiterate, especially in the context of the apparent disagreement between studies invoking transient rheology to fit Greenland observations (e.g., Paxman et al., 2023, AGU Advances and Adhikari et al. 2021, GRL) and others that do not (e.g., this study and the Pan et al., 2024, GJI).

2) Lines 484-486:

I suggest changing to: “… because the lateral extent of the low-viscosity region may be restricted (e.g., in the case of a plume track), significantly reducing modeled uplift rates.” This clarifies that small or reduced lateral extent will reduce rates, rather than any arbitrary lateral extent.

3) Lines 574-576:

A curve can’t be fast or slow. I would suggest rewording to “can reconcile generally slower rates of Holocene relative sea-level fall with faster GNSS-derived uplift rates”.

4) Line 819:

“during and following Greenland deglaciation”? Most of the rapid RSL fall seems to be contemporaneous with rapid ice mass loss (Figure S2A).

5) Figure 5:

What are the yellow stars (presumably low-viscosity layer of 1 x 1019 Pa s)? Caption and text only mention 1 x 1018 Pa s and 5 x 1018 Pa s low-viscosity layer runs (i.e., the red and blue stars). Also, shouldn't the stars be offset slightly from 30 and 60 km lithospheric thickness, since they represent 22.5 and 45 km lithospheric thickness, respectively? Or else, as in Figure 7, could a short explanation be added in the caption to highlight that these lithospheric values are the same as what is assumed within the 30 and 60 km plume track models within the track itself (I know this can probably be surmised from the text, but it would be good to make this crystal clear). This comments also applies to Figures S5, S8, and S19.

6) Supplementary Figure 21:

The legend on the figure doesn't appear to match the caption. As in the comment above, I would suggest adding a short explanation to highlight that these lithospheric values are the same as what is assumed within the 30 and 60 km plume track models within the track itself.

Fred Richards

Comments to Author

It is becoming increasingly clear from polar GNSS observations that, in certain regions of Greenland (and Antarctica), recent ice melting is inducing viscoelastic bedrock rebound at rates far faster than expected for commonly assumed upper mantle viscosities (~10^{20} Pa s). This rapid surface uplift has important ramifications for our understanding of near-future sea-level change, as it may help to slow or stabilise grounding line retreat on decadal to centennial timescales. Understanding the cause of this fast deformation, and how it might interact with future ice-sheet dynamics, is therefore of major societal importance. This study tackles this crucial question by investigating the degree to which GNSS-derived bedrock uplift rates in SE Greenland can be explained by potential Iceland plume-induced modification of mantle thermomechanical properties. Using a novel implementation of the ASPECT geodynamic modelling software to simulate bedrock adjustment to ice load changes, they show that if very low asthenospheric viscosity (~10^{18} Pa s) and lithospheric thickness (30–60 km) exists beneath the region of SE Greenland potentially impacted by the Iceland plume, rapid uplift can be produced in response to recent ice melting (i.e., since 1000 AD). This signal provides a good match to GNSS-derived bedrock uplift rates that previous glacial isostatic adjustment (GIA) models have struggled to reproduce. Importantly, the invoked rheological weakening implies that the GIA response to ice load changes during the last glacial cycle (122–0 ka) has mostly decayed away in this part of Greenland and is a minor contributor to present bedrock rebound rates. This result has important implications for current satellite-gravity-derived estimates of regional ice mass loss that use corrections which generally predict that the GIA response to the Last Deglaciation (~20–7ka) is still ongoing in this region.

Overall, I think this a pioneering and well-executed study that will be of interest to researchers in multiple Earth science disciplines. It provides evidence that previously identified discrepancies between present-day GNSS-derived uplift rates and those predicted by simple GIA models might be resolved by accounting for recent ice melting and potentially plausible 3D variations in Earth's thermomechanical structure. However, I believe more support for their current conclusions is needed. In particular, independent evidence for the spatial pattern and magnitude of the invoked viscosity variations, and a brief assessment of their compatibility with longer timescale constraints on GIA-induced solid Earth deformation such as Holocene sea-level markers. Although the authors show that they can obtain reasonable fits to GNSS uplift rates, as it stands, it is not clear that the low steady-state viscosities they require beneath large portions of SE Greenland are compatible with independent geophysical constraints on present-day mantle viscosity or longer timescale constraints on deglacial GIA-induced solid Earth deformation in SE Greenland. Their statement that their *"results confirm that complex rheologies are not necessary to explain rapid recent uplift, even for the most rapidly uplifting regions"* is therefore questionable at present, in my opinion.

If these issues can be resolved (see Main Comments below), along with my more minor concerns, I would be very happy to see this manuscript published in Communications Earth and Environment.

Main Comments

1) Lack of independent evidence for magnitude and geometry of inferred asthenospheric low-viscosity zone.

While there's substantial evidence that the Iceland plume has affected the thermomechanical properties of the mantle beneath Greenland in the past—and probably continues to do so along its SE margin—it is less clear that it presently sustains a 10^{18}–10^{19} Pa s low-viscosity zone that extends ~1000 km northeastwards along the Iceland plume track from its present-day position. Inferring viscosity from seismic velocity is non-trivial, but if there really was a ~150–200 km thick asthenospheric low viscosity region beneath the track, one would expect there to be a very strong negative seismic velocity signal associated with this feature. While the quoted seismic tomographic studies (e.g., Mordret, 2018, JGR and Celli et al., 2021, EPSL) do find low seismic velocities in the asthenosphere beneath parts of SE Greenland, they are confined to a margin-parallel patch between Kangerlussuaq and Scoresby Sound (Kangertittivaq). Moreover, the studies that have attempted to estimate viscosity within this low-velocity region have found relatively high viscosity (~1-5 x 10^{19} in Mordret at al., 2018 and Milne et al., 2019, GJI) compared to what appears to be required to fit many of the GNSS-derived uplift rates (<10^{19} Pa s for all but KUAQ). Finally, fitting KSNB and getting close to fitting PLPK (the two most southern GNSS stations) appears to require the imposition of a continuous low-viscosity layer, implying that the track-aligned (rather than margin-aligned) geometry of the low-viscosity zone imposed in this study may not be a good approximation of reality.

Given the above, I feel that the lack of independent evidence to support the magnitude and geometry of the inferred low-viscosity zone needs to be made clearer (although I acknowledge that the geometry issue is partially addressed with in the "Uplift rates at PLPK and KSNB" section). This discrepancy is quite interesting in my view, since it implies that a mechanism in addition to lateral steady-state viscosity variations is needed to explain the GNSS observations (see below).

2) Fit of predicted deformation to longer timescale constraints and implications for transient rheology.

I realise it is not currently possible to model transient rheologies in ASPECT; however, recent work (e.g., Adhikari et al., 2021, GRL; Lau et al., 2021, JGR; Paxman et al., 2023, AGU Advances; Richards & Hazzard, 2023, JGR) implies that the effective viscosities recorded by polar GNSS networks scale with ice loading/unloading timescale, pointing to the operation of transient creep in these regions. This behaviour could enable the reconciliation of the viscosities assumed in this study with those inferred from seismic tomography, since it would result in modern melting signals triggering deformation with ~10^1–10^2 times lower effective viscosities than those associated with the Last Deglaciation. Indeed, I suspect transient rheology may need to be invoked because, while the low steady-state viscosities inferred here can locally fit GNSS data, they appear to produce very rapid vertical displacement rates in the early Holocene. These rates reach ~80 mm/yr for the VFDG GNSS station at ~8 ka assuming a 600 km wide plume track and 10^{19} Pa s asthenospheric viscosity (Fig. 2); however, based on Fig. S16 it seems like present-day uplift at this station is only reproducible with a 10^{18} Pa s viscosity and 600 km wide plume track, indicating that even higher early Holocene rebound rates would likely be predicted for the best-fitting model at this site. At first glance, these very rapid rebound rates appear at odds with nearby relative sea-level constraints. For example, the Schuchert Dal sea-level markers compiled by Hall et al. (2010, QSR) document only a ~20 mm/yr early Holocene (11–8 ka) RSL fall, a signal that can be related to bedrock uplift rates relatively directly (local glacier recession is assumed to have largely ceased by ~12 ka so fall in sea-surface height due to reduced gravitational pull of the Greenland Ice Sheet on the surrounding ocean is likely a minor contribution). That said, the data-model agreement may be more reasonable if ASPECT predictions of total early Holocene bedrock elevation change, rather than rate of change, are compared with the observed RSL fall.

In any case, some direct comparison of the ASPECT model outputs with Holocene sea-level records in the study region (e.g., Schuchert Dal [Hall et al., 2010, QSR] and Ammassalik [Long et al., 2008, EPSL & QSR]) is needed to demonstrate that the wide plume tracks and low lithospheric thicknesses and viscosities obtained from the GNSS observations are compatible with longer timescale constraints. Whatever the result of this comparison, it will have important and interesting implications for whether—in addition to lateral viscosity variations—transient creep mechanisms need to be invoked to simultaneously explain sea-level records and GNSS-derived uplift rates in Southeast Greenland, a topic of considerable interest in the GIA community.

Minor Comments

1) **Lines 38–40:** I would suggest "…*a potentially thinner lithosphere and weakened upper mantle beneath parts of southeast Greenland"* or something similar to indicate that the seismic evidence is inconsistent with low mantle viscosity existing across the whole region.

2) **Lines 79–81:** *"…and a lithosphere that is thinned by…"*.

   I also think it's worth spelling out where the 25% lithospheric thinning comes from. I realise you cite Heyn & Conrad (2022, GRL) for this, but it would be helpful to say briefly what is done in this study, e.g., "*thinned by Δh (25% of T_{LT}) based on 3D numerical modelling of plume-lithosphere interaction [11] (Fig. 1C)."* Is the 25% based on the average across several model runs or the result of a specific model?

3) **Results Section; Regional and Global Modelling Subsection**: This subsection contains no results and instead describes the approach/methodology so the *"Results"* header seems out of place. Can this header be moved to above the *"Uplift rates in Southeast Greenland"* Section and an *"Approach"* header be added here instead? Otherwise, both can be excluded entirely; I don't think they're needed.

4) **Line 132:** When you say "*three largest mass-losing glaciers"* do you mean the three largest glaciers that are also net losing mass, or the three glaciers losing the most water mass to the ocean?

5) **Line 139:** "*A slow elastic response dominates…".* I don't wish to be too pedantic, but elastic responses are effectively instantaneous so "slow" seems to be the wrong adjective here. I would rephrase along the lines of "*If we apply the (layered) VM5i model, the solid Earth deformation is dominantly elastic, with the lack of viscous contribution leading to relatively modest uplift rates."*

6) **Lines 152–153:** Maybe worth justifying here why you can discount viscous deformation contributions from outside the loading area. I assume that you can do so by arguing that, where viscous responses to contemporary melting are occurring, these responses are localised to anomalously low viscosity regions outside the loading area. These responses will therefore have minimal impact compared to the elastic deformation, which may be longer wavelength.

7) **Lines 153–156:** Not clear how this signal is being modelled from the text. Can a brief description of the methodology be given or else a signpost added to the "*Greenland modeling of elastic response"* section of Materials and Methods?

8) **Figure 4:** I'm a little confused by this figure as I expected the middle/middle-right panels of the first three rows to be identical since they are labelled as representing parameters that would equate to those of the reference model (e.g., the 400 km plume track, 60 km lithospheric thickness, and 1 x 10^{19} Pa s track viscosity panels). Are any other parameters varying from row to row in addition to the main parameter being varied, or are the differences purely a visual artefact of the different colour scales being used in each row? If the former, can this be made explicit in the caption; if the latter, then I think this is misleading. In either case, I think the colour scales should be harmonised across the rows for ease of comparison.

9) **Paragraph beginning on Line 356:** I would mention the possibility that invoking transient rheology could better fit these two stations, since it would lower the effective viscosity

controlling responses to modern melting (decadal-to-centennial timescales) by a factor of 10–100 across the whole region (see Main Comment above and, e.g., Paxman et al., 2023). Assuming a transient rheology would likely give results very similar to the eventual proposed solution (i.e., a layer with low steady-state viscosity exists beneath the entire loading region).

10) **Figure 7 caption:** What lithospheric thickness is assumed when there is no track, just a continuous low-viscosity layer? Also 60 km thick? Or 0.75*60 km = 45 km?

11) **Lines 457–461:** To me, the Paxman et al., 2023 (P23) results look consistent with what is inferred here despite the relatively low (2°) resolution of the seismic constraints they used. For decadal timescales—which dominate the present-day uplift signal predicted by the lowest viscosity ASPECT models—P23 obtain ~1–4 x 10^{18} Pa s beneath the Kangerlussuaq region (see their Figure 5a, 5f, and S4a), which is compatible with what appears to be required here (1–5 x 10^{18} Pa s). The P23 results also indicate a more widespread low-viscosity zone, which is more compatible with what is needed to fit PLPK and KSNB uplift rates. I think these sentences need to be rephrased accordingly.

12) **Lines 463–465:** I would say "*<u>indicate</u>* that complex rheologies *<u>may</u> not be necessary*" since no evidence has yet been supplied that the low inferred steady-state viscosities are compatible with present-day geophysical observations or longer-timescale relative sea-level constraints. If this issue can be addressed (see Main Comments above) then the current, stronger wording could be justified; however, the evidence supplied is insufficient to warrant it at present.

13) **Line 478 and throughout:** Capitalize *"<u>E</u>arth"* in *"solid Earth"*. Check throughout document.

14) **Line 482:** *"…variety <u>of</u> viscoelastic-plastic rheologies"*.

15) **Lines 529–530:** "*Greenland experience<u>d</u> subsidence after that ice sheet deglaciated".*

16) **Lines 572–573:** For reproducibility, can you quote the value of the volume-averaged mantle density being used here?

17) **Lines 589–591:** The assigned timespans of the satellite altimetry era, second millenium, and glacial cycle loading changes appear to overlap. Is this actually the case? If so, is there not a risk that you're double/triple counting load changes during certain time periods?

18) **Lines 593–596:** Is the solid Earth deformation correction applied to the VMB dataset consistent with what is calculated here for the last glacial cycle loading signal? If it is, this should be made clear; if not, can some reassurance be provided that the discrepancy has a negligible impact on the results?

Fred Richards

Reviewer #1: Anonymous (Remarks to the Author)

This manuscript finds that incorporating a low viscosity upper mantle plume track into 3D GIA models for southeast Greenland can help to explain rapid uplift rates that have been observed in the region. The conclusions are novel as regional-scale GIA modeling that accounts for strong heterogeneity in solid Earth structure has not been completed for SE Greenland. At this point, very few studies have considered the impact of regional-scale variability in solid Earth structure on GIA; therefore, findings from this study should motivate other GIA modeling studies to consider higher-resolution solid Earth structure. Although it may not be explicitly stated in the manuscript, this study also points towards the importance of improving constraints on solid Earth structure in SE Greenland for advancing our understanding of GIA in the region. I have included general and line specific comments below.

Thank you, reviewer #1, for your positive view on this study and for your useful comments, which we have used to improve the manuscript. Please see below how we addressed your feedback. Changes to the manuscript are indicated in **bold**.

General comments

1) Is it possible to consider the effect of the plume track on horizontal crustal motions as well? Several GIA studies have shown that accounting lateral heterogeneity has a large impact on predicted horizontal crustal motions. It would strengthen the manuscript if the authors investigated the effect of the plume track on horizontal crustal motions. Of course, GIA model predictions could also be compared to observed horizontal crustal motions.

Due to model limitations in ASPECT, we currently cannot investigate horizontal crustal motions. This has 2 reasons:
1. The free surface (deforming mesh): There is zero shear stress at the surface boundary (shear stress from air on surface is negligible compared to between solids). With this boundary condition there will be flow across the boundary. For mass conservation, the surface boundary is advected in the direction of fluid flow, causing the mesh to deform upwards/downwards only. Therefore, the ASPECT code that we are using here is not yet able to usefully predict horizontal velocities. Instead, it has been benchmarked (successfully) for vertical motions (see Weerdesteijn et al., 2023). For more details on the free surface implementation please see Rose et al. (2017).
2. (In)compressibility: The effect of compressibility on the magnitude of horizontal displacement can be large, e.g., Tanaka et al. (2011) and Reusen et al. (2023), due to the neglect of material dilatation. The viscoelastic or new visco(elastic)-plastic rheology implementations in ASPECT assume the Boussinesq approximation (making the Stokes system to solve much less computationally expensive) and thus incompressibility is assumed when solving the continuity equation. Furthermore, the free surface with boundary traction has not yet been benchmarked for compressible convection. Thus, because compressibility is important for horizontal velocities (much more than for vertical velocities, see Tanaka et al. (2011) and Reusen et al. (2023)), it would be important to benchmark the ASPECT code for the compressible convection case, after items 1 and 2 above are solved. For more information about incompressibility in ASPECT, please see:
https://aspect-documentation.readthedocs.io/en/latest/user/methods/approximate-equations/ba.html#on-incompressibility.

Based on this comment, we feel that other readers might have a similar question and will wonder why we did not also use horizontal velocities from our models. To address this, we have added two sentences to the manuscript in the first paragraph of "Regional solid Earth deformation modeling in ASPECT". These two sentences briefly describe our points 1 and 2 above:
"**This setup allows us to accurately predict vertical [27] but not horizontal [26] motions of the free surface in response to imposed surface tractions.**" And "**Such benchmark tests have also not yet included compressibility, despite its potential importance for horizontal surface displacements [81, 82].**"

[26] Rose, I., B. Buffett, and T. Heister (2017). Stability and accuracy of free surface time integration in viscous flows. *Physics of the Earth and Planetary Interiors*, 262, p. 90-100. doi:10.1016/j.pepi.2016.11.007.

[27] M. F. M. Weerdesteijn, J. B. Naliboff, C. P. Conrad, J. M. Reusen, R. Steffen, T. Heister and J. Zhang (2023). Modeling viscoelastic solid earth deformation due to ice age and contemporary glacial mass changes in ASPECT. *Geochemistry, Geophysics, Geosystems*, 24(3) ,p. e2022GC010813.

[81] Reusen, J. M., R. Steffen, H. Steffen, B. C. Root, and W. van der Wal (2023), Simulating horizontal crustal motions of glacial isostatic adjustment using compressible Cartesian models, *Geophysical Journal International*, *235*(1), 542-553, doi:10.1093/gji/ggad232.

[82] Tanaka, Y, V. Klemann, Z. Martinec, and R. E. M. Riva (2011). Spectral-finite element approach to viscoelastic relaxation in a spherical compressible Earth: application to GIA modelling, Geophysical Journal International, 184(1), p. 220-234. doi: 10.1111/j.1365-246X.2010.04854.x.

2) While the authors justify their chosen solid Earth configurations based on findings from [13, 85, 9], the argument for adopting the given solid Earth configurations could be strengthened if the authors consider findings from seismic imaging studies as well, such as Pourpoint et al. (2018). Using various seismic methods, the Pourpoint et al. (2018) study places constraints on lithospheric structure across Greenland. The authors acknowledge that solid Earth structure is not well-constrained in this region, which makes it especially critical to pull constraints on solid Earth structure from all available geophysical studies.

Thank you for pointing out seismic studies that we did not consider, and we agree we need to make use of all constraints. We do refer to seismic studies of Celli et al. (2021) and Mordret (2018). Celli et al. (2021) use seismic waveform tomography to find "a large, low-velocity anomaly, indicative of high temperatures, at 400-660 kilometers depth beneath eastern Greenland". Mordret (2018) uses ambient seismic noise tomography to find a "200- to 300-m/s decrease of velocity in the upper mantle below the Kangerlussuaq Fjord area, compared to the rest of Greenland, (…). The viscosity inferred in this region is several orders of magnitude lower than the surrounding cratonic areas (…)." Pourpoint et al. (2018), also using ambient seismic noise tomography, find "(…) a deep high-velocity feature extending from southwestern to northwestern Greenland that may be the signature of a thick cratonic keel, a corridor of relatively low upper-mantle velocity across central Greenland that could be associated with lithospheric modification from the passage of the Iceland plume beneath Greenland or interpreted as a tectonic boundary between cratonic blocks, (…)". Additional to these studies, Antonijevic and Lees (2018) also use ambient seismic noise tomography to infer the lithospheric and upper mantle structure beneath Greenland. They find "slow velocities coinciding with the NW-SE trending Iceland plume track".

On other hand, we note that Darbyshire et al. (2018) provide constraints on lithospheric structure using Rayleigh wave group velocity tomography. These authors did not find notable seismic velocity anomalies associated with proposed plume tracks. However, their method only provides constraints for shallower depths (less than about 80-90 km), and ray path coverage becomes increasingly limited near the base of their model (as noted by Celli et al., 2021). This illustrates the disparities among the seismic constraints, as well as the uncertainty associated with them, which together emphasize the overall uncertainty regarding the size, amplitude, and path of the Iceland plume track.

Regarding lithospheric thickness, Pourpoint et al. (2018) find substantial variations in crustal thickness across Greenland, from roughly 25 to 45 km. However, crustal thickness is not equal to effective elastic thickness (required for GIA modeling), but Pourpoint et al. (2018) does find the following: "The results suggest thicker crust in the north, south central regions and thinner belt stretching east-west across central Greenland. The thinnest crust is along the central east margin. Still, we must consider potential trade-offs between uppermost mantle and lower crust and complexities not allowed in the inversion's model parameterization when interpreting our Moho depth model." And "In central eastern Greenland south of the 72°N lineament, our model suggests significant crustal thinning (…)" and "(…) thin crust in CE Greenland (…) could be the result of extensive stretching of continental crust during rifting and spreading of the North Atlantic Ocean and may also reflect interactions of the North Atlantic rifting with the Iceland plume ~56 to 35 Ma." This result is supported by Antonijevic and Lees (2018), who state that “In east Greenland the detected velocity reduction at longer periods (33-40s) reflects substantially thinned lithosphere, thermally ablated by the plume.” Examining Figures 3 and 4a of Antonijevic and Lees (2018) shows that this thinned region extends both southwards (along the coastline) and to the northwest (along the plume track) from the Kangerlussuaq area.

For the effective elastic thickness, we already refer to the studies of Steffen et al. (2018) and Audet (2014). E.g., Steffen et al. (2018) find reduced strength in the lower crust and lithospheric mantle beneath southern and central Greenland. They find an effective elastic thickness of the lithosphere over Greenland ranging between 5 and 85 km and an effective elastic thickness on the east coast “15 ± 25 km together with a deep Moho (∼50 km), suggesting mechanical decoupling and /or a weak lower crust (…).”

To improve our discussion of the geophysical constraints, we added Pourpoint et al. (2018) and Antonijevic and Lees (2018) to the Introduction:
“Magnetic, heat flow, gravity, radar, and seismic data [10, 9, 12, 13, 14, **15**, **16,** 17] point towards a potentially thinner lithosphere and weakened upper mantle beneath Southeast Greenland.”

We also added both of these references, as well as Darbyshire et al. (2018) to the section:
“**Extent of the low-viscosity region**” (Former “Uplift rates at PLPK and KSNB”): “Indeed, the lateral extent of the region weakened by the Iceland Plume is not well constrained [10, 9, 12, 13, 14, 50, 51, 52, 7, **15, 16, 53**], and potentially extends further south based on recent constraints from seismic tomography [14**, 16**].”

Regarding the width of the plume track, Pourpoint suggests a 200 km wide weakened track “This low-velocity corridor is especially interesting because it appears to connect the western and eastern Tertiary basalt provinces. The feature is about 200 km wide (…).” and “(…) the low-velocity corridor matches several of the proposed plume tracks”. Rogozhina et al. (2016) find “The lateral dimensions of the reconstructed geothermal anomaly are roughly 1,200 by 400 km, covering about a quarter of the Greenland land area. The GF values in the anomalous area are up to 2.5 times greater than the background GF values derived across the northern and western parts of Greenland.”

We added to the Material and Methods: Earth structure and variations:
“This lithospheric thinning is set to 25% of the surrounding lithospheric thickness (Fig. 1C), which is consistent with models of **thermal ablation by** plume-lithosphere interaction [11] **and indications from seismic tomography [e.g., 14, 15, 16, 12]**.” and “We employ plume track widths of 200, 400, and 600 km and a plume track trajectory following *Martos et al.* [9]. **The plume track width is not well constrained, and model choices are based on findings from seismics and magnetics (geothermal heat flow) [12, 10, 14, 15, 16].**”

Finally, we added text to the first paragraph of the Discussion section. This text compares the results of our study (where we vary the width of the plume track) to constraints from the seismic studies (which suggest different plume track widths, see above). In particular, we wrote:
“Narrower plume tracks of width ~200 km require weak viscosities of η_{PT} ~10^{18} Pa s, while wider tracks (400-600 km) can be stiffer (up to ~10^{19} Pa s) (Fig. 6). These width estimates are consistent with geophysical observations, which also range from ~200 km [15] to 400 km [10], or even wider [14]. Models that match uplift rates near the Kangerlussuaq glacier often do not also match observed uplift rates nearby, suggesting that the complex 3D nature of the plume track may be important. Particularly, our models indicate that rapid uplift observed at stations south of the Kangerlussuaq glacier is consistent with geophysical observations that suggest an influence of the Iceland plume along this portion of the Southeast Greenland coastline [9, 16, 14].”

[15] Pourpoint, M., S. Anandakrishnan, C. J. Ammon, and R. B. Alley (2018). Lithospheric Structure of Greenland From Ambient Noise and Earthquake Surface Wave Tomography. *Journal of Geophysical Research: Solid Earth*, 123, p. 7850–7876. doi: 10.1029/2018JB015490.

[16] Antonijevic, S. K., and J. M. Lees (2018). Effects of the Iceland plume on Greenland's lithosphere: New insights from ambient noise tomography. *Polar Science*, 17, 75-82. doi:10.1016/j.polar.2018.06.004.

[53] Darbyshire, F. A., T. Dahl-Jensen, T. B. Larsen, P. H. Voss, and G. Joyal (2018). Crust and uppermost-mantle structure of Greenland and the Northwest Atlantic from Rayleigh wave group velocity tomography. *Geophysical Journal International*, 212(3), p. 1546-1569, doi:10.1093/gji/ggx479.

3) There are several GNSS sites located just outside of the "area of interest" but within the "ice loading area" shown in Figure 1. Why were uplift rates from these sites not considered in the analysis? Might it be informative to compare predicted uplift rates to observed rates at these sites even if they are located just outside the "area of interest"?

Indeed, several GNSS sites are located outside the area of interest but within the ice loading area. We did not take these GNSS stations into account, because of their position with respect to the ice loading area boundary and regional model domain boundary. As stated in the first paragraph of the Results: "The different domains are chosen to minimize the influence of ice outside of the ice loading area and the model's lateral boundaries on solid Earth deformation inside of our area of interest." And in Materials and Methods: Regional models: Viscoelastic deformation driven by deglaciation of Southeast Greenland: "Our area of interest lies within the ice loading area, 250 km away from the boundaries of the ice loading area, which reduces the effect of ice loading changes outside the ice loading area on solid Earth deformation within the area of interest (Fig. 1A, blue box). A border of 500 km is added beyond the ice loading area in each direction to reduce the effect of the model's lateral boundaries (edge effects associated with the model) on solid Earth deformation within the area of interest (Fig. 1A, pink box)."

The Materials and Methods explanation is more detailed than the Results explanation and therefore we added the following reference in the Results explanation:
"The different domains are chosen to minimize the influence of ice outside of the ice loading area and the model's lateral boundaries on solid Earth deformation inside of our area of interest **(see Materials and Methods)**."

However, you raise a good point. Although the model results will be less accurate for the GNSS sites outside the area of interest than inside (but all inside the ice loading area), we looked into the GNSS sites that are just outside the area of interest, and close to our newly added sea level sites for comparing observation and modeling rates over the Holocene (see comments Reviewer #3). These GNSS sites are: Daugaard Jensen Gletscher (DGJG) and Scoresbysund (SCOR) to the north, and Helheim Glacier (HEL2) and Kulusuk (KULU) to the south. DGJG and HEL2 lay at the ice margin, whereas SCOR and KULU lay far from the ice margin at the coast.

Because these changes are so intertwined with the new sea level sites analyses, we refer to comment 2 of Reviewer 3, where we state all changes made related to the new GNSS and sea level sites. Thank you for your feedback of looking into these sites, as it gave more insight into the extend of the weakened Earth structure along the southeast coast of Greenland.

4) As I alluded to earlier, it would probably be fruitful to explicitly state that results from this study should motivate further efforts to place improved constraints on solid Earth structure in SE Greenland.

That is nice addition to mention indeed. We added in the Discussion ("**On the preferred Earth structure**"):
"**Our regional models represent an improvement, but emphasize the need for better geophysical constraints on the heterogeneous viscosity structure beneath Greenland**."

Line specific comments

1) Lines 77–78; 582-583: How were the plume track widths of 200, 400, and 600 km chosen? Were these plume track widths also based on findings from the Martos et al. [9] study?

We added to the Material and Methods: Earth structure and variations:
"We employ plume track widths of 200, 400, and 600 km and a plume track trajectory following *Martos et al.* [9]. **The plume track width is not well constrained, and model choices are based on findings from seismics and magnetics (geothermal heat flow) [12, 10, 14, 15, 16].**" Also note that we have added text to the first paragraph of the Discussion in which we compare our model results for different plume track widths to the plume track widths inferred from specific studies: **"These width estimates are consistent with geophysical observations, which also range from ~200 km [15] to 400 km [10], or even wider [14]."**

2) Line 79: A reference to justify the adopted lithospheric thicknesses would be helpful (i.e., 30, 60, 90 km) – perhaps look to the Pourpoint et al. (2018) study.

In the Materials and Methods, we refer to Steffen et al. (2021) and Audet (2018) regarding the lithospheric thicknesses: "The lithospheric thickness outside the track varies between 30, 60, and 90 km (thereby changing the thickness of upper mantle layer UM1), which is within the plausible range of elastic lithospheric thicknesses in Greenland [85, 13, **15**]."

We added Pourpoint et al. (2018) to the Introduction:
"Magnetic, heat flow, gravity**, radar, and** seismic data [10, 9, 12, 13, 14, **15,** 16, 17] point towards a potentially thinner lithosphere and weakened upper mantle beneath Southeast Greenland."

We also added Pourpoint et al. (2018) to the Reults: "Indeed, the lateral extent of the region weakened by the Iceland Plume is not well constrained [10, 9, 12, 13, 14, 50, 51, 52, 7, **15,** 16, 53], and potentially extends further south based on recent constraints from seismic tomography [14, 16]."

3) Line 80: "lithospheric" should be just "lithosphere".
Done.

4) Line 584: Please add units to 5×10^{20} here. Also please add a reference to justify the adopted upper mantle viscosity (for example reference Lecavalier et al., 2014).

We added: "The plume track lies within the upper mantle layer, which has a viscosity of $5 \cdot 10^{20}$ **Pa s** (Tab. S1), **consistent with different ice sheet deglaciation studies [28, 21, 49]**."

[49] Lecavalier, B. S., G. A. Milne, M. J. R. Simpson, L. Wake, P. Huybrechts, L. Tarasov, K. K. Kjeldsen, S. Funder, A. J. Long, S. Woodroffe, A. S. Dyke and N. K. Larsen (2014). A model of Greenland ice sheet deglaciation constrained by observations of relative sea level and ice extent, *Quaternary Science Reviews,* 102, p. 54-84, doi: 10.1016/j.quascirev.2014.07.018

5) Line 365: What is the viscosity adopted for this full low-viscosity layer? Is it 1×10^{18}? Please make it clearer what the viscosity of the low-viscosity layer is.

We added: "For simplicity, we implemented a full low-viscosity layer **($1 \cdot 10^{18}$ or $5 \cdot 10^{18}$)**, by making the plume track width W_{PT} as wide as the model domain (see Fig. 1C)."

6) Line 569: Earth structure and variations section: Can you please specify the lower mantle viscosity somewhere in this section.

We added:
"For the simulations in SELEN, we use the radially symmetric 11-layer VM5i rheology model (Tab. S1) [35], which is an adaption to the VM5a model [24, 18] without elastic compressibility**, with a lower mantle viscosity ranging between 1.5-$3.2 \cdot 10^{21}$ Pa s from 670 km depth to the core-mantle boundary**."

Reviewer #2: Anonymous (Remarks to the Author)

General comments

This manuscript details a new, state of the art computational model of ice change-related loading to better explain the rapid uplift rates at 4 sites in east Greenland that has been given in past research. These sites were identified in a Science Advances paper by Abbas Khan in 2016 and modeled via a locally applicable 1-D Earth structure. The work by Weerdesteijn and Conrad presented here is clearly a nice, and very rational, advancement of the modeling, and they appear to have taken sufficient care in constructing the model ice load changes, that I think the work deserves to be published.

Thank you for your enthusiastic response, and for your constructive comments below. We have used them to improve the manuscript. Changes to the manuscript are indicated in **bold**.

I do, however, have several concerns that need to be answered. Since my Detailed Comments flush these issues out with considerable granularity, I will only summarize the most important one here. The explanation of the 'elastic' loading is confusing. A Green's function approach is discussed as the technique for the altimetric period, yet on lines 456-457, it is stated that the same linear viscosity is used for all three modeled time periods. It is important, if not critical, to clarify. I assume the model uses Maxwell viscoelasticity, which is a simple linear viscoelasticity. Other linear viscoelastic constitutive relations would include the one proposed by Paxman and the one used in a recent paper by Ivins in J. Geodesy. These are 'higher order' linear viscoelastic rheologies, and are distinct from a non-linear rheology such as recently used by Blank et al.

These are very important points, and we appreciate that some of these rheological elements of our modelling was not entirely clear in the original manuscript. To be clear, we do use Maxwell viscoelasticity, and the Green's function approach is only used for far-field loads. We agree that it is critical to make this clear in the manuscript, and we have made significant changes to the Material and Methods section to do this. Please see our response to point 19 below. We have also followed the reviewer's suggestions for how to change our discussion of alternative implementations of rheology, such as the one discussed by Paxman et al. (2023). For more details, please see our response to point 25 below.

I place an indicator (*) on each detailed comment that I believe are essential to properly address in the revision.

We have done our best to respond to each point, and make changes as appropriate. We feel that the manuscript is improved as a result - thank you for these constructive comments.

Line specific comments

1) Line 23: Sentence beginning "This rapid viscous response … is not usually considered …" is incorrect. There is at least 25 years of literature discussing this. I suggest deleting this sentence and then picking up with the next, changing it to "When such regions occur beneath marine terminating outlet glaciers the vertical uplift may influence the future stability of the entire ice catchment basin upstream."

In reference to the Discussion, I have more to say about this.

We appreciate this comment and have removed this sentence. We followed the reviewer's advice to more specifically focus on the impact on the ice sheet stability. We have slightly adapted the reviewer's statement, writing:

“Regions of weakened mantle positioned beneath marine terminating glaciers, where rapid uplift resulting from modern deglaciation can affect the future stability of entire ice catchment areas upstream, will become increasingly important in the near future as deglaciation accelerates.”

2) Introduction: I am surprised that no mention is made of the novel paper Mark Fahnestock published in Science.

Thank you for pointing us to this paper. We have added it to the list of citations suggesting weakened lithosphere beneath SE and central Greenland. We also added “**radar**” to the list of data constraints.
“Magnetic, heat flow, gravity, **radar**, and seismic data [10, 9, 12, 13, 14, 15, 16, **17**] point towards a potentially thinner lithosphere and weakened upper mantle beneath parts of Southeast Greenland.”

[17] Fahnestock, M., W. Abdalati, I. Joughin, J. Brozena and P. Gogineni (2001). High Geothermal Heat Flow, Basal Melt, and the Origin of Rapid Ice Flow in Central Greenland. *Science,* 294(5550), p. 2338-2342, doi: 10.1126/science.1065370.

3) Line 29: Generally, the Earth is a planet, and as such it is capitalized, like Venus, Io, Europa, etc.

We now capitalized Earth globally within the document.

4) Line 34: A recent study by Kappelsberger reveals that ICE-6G is a somewhat poor choice for an ice history forward model. Huy3 might be a better choice (Lecavalier), according to these authors. Possibly consider adding the Kappelsberger to the reference list.

We appreciate that the ice model used in this study by Kappelsberger may represent a better choice. However, the Kappelsberger study is focused on northeast Greenland, and thus we have decided not to cite it here, in a sentence that discusses Southeast Greenland.

5) Lines 49-52: This statement about local as opposed to distant viscosity was shown (in detail) also by Klemann et al in modeling the 3-D viscosity around Patagonia. Please add this to the reference [18].

We have added this reference. “Distant loads drive long-wavelength deformation (thousands of km) that is sensitive to Earth’s overall stratified viscosity structure [20] while local loads drive regional deformation (tens of km) that is sensitive to the local viscosity structure nearby [18, **22**].”

[22] Klemann, V., E. R. Ivins, Z. Martinec and D. Wolf (2007). Models of active glacial isostasy roofing warm subduction: Case of the South Patagonian Ice Field. *Journal of Geophysical Research: Solid Earth*, 112(B9), 2007.

6) Lines 89-90: The study by [29] is a perfectly good one, but it is but one among a number. Ines Otosaka has presented an intercomparison of altimetry-based Greenland mass balance over 2002-2019 in her Figure 3b. Please give the error estimates that are reported therein. She also gives mass balance estimates from altimetry on an annual basis.

Our citation to [29] relates to a very specific purpose: We use this study to compute the mass loads associated with satellite altimetry for our “satellite altimetry era” mass loss calculations. Thus, we have not added a citation to Otosaka here, although we agree that this reference is very relevant for the all-Greenland mass balance. Since we are performing regional modelling, the all-Greenland mass balance is not exactly relevant for us here.

7) Line 99: “ … uplifts all of Greenland”. For a weak zone, one might expect that in some part of the solution space there will be subsidence that results from this more recent load changes (e.g., see Figure 3b of Ivins and James 2004, from Patagonia models). Might recommend saying “dominantly uplifts Greenland crust”.

This is a good point – maybe not all of Greenland is uplifted. We have changed the wording as suggested. "Second, recent deglaciation occurring elsewhere in Greenland (outside the ice loading area) induces a long-wavelength elastic response that **dominantly uplifts Greenland**."

8) Line 116: My recollection is that these are "formally adopted IGS station acronyms".

Only Scoresbysund (SCOR), newly introduced in the last part of the results, is an IGS station. Therefore, we stick with GNET because all stations we use are part of that network.

9) Lines 146-148: This was shown ("… greatest uplift rates occur near rapidly deglaciating regions …") by Lange et al. (2014) for Patagonia: a case wherein the location of the most rapidly changing ice is unambiguous. I think this should be noted.

This sentence is more specifically directed toward "idealized models", where the low-viscosity zone is specified and placed below a specified deglaciating region. Thus, we prefer to keep the citation to [18] as it is, rather than introducing a citation relevant to a specific area, of which there are potentially many.

10) Line 165: Might suggest replacing the reference to "second millennium" to "Post-1000 CE", and maybe even a more compact acronym of the authors choosing, since it is repeated throughout the manuscript.

We decided to stick with "second millennium" as the naming "Post-1000 CE" doesn't exclude the satellite altimetry era. We like to avoid introducing more acronyms (the GNSS stations already make it an acronym-heavy manuscript). We agree that it saves space, but it can also be confusing to a reader when these are not common acronyms used outside of this manuscript.

11) Line 172: I rather tend believe this statement, generally. Except that if one adopts the view taken by the ANU series of models and analysis, in which the lower mantle viscosity increases by over one order of magnitude. If this is a robust statement, then it would need quantification by actual modeling viscosity profiles that deviate substantially from VM5i, as used here. I leave it to the authors as to how to address this issue. It might be an important caveat, and maybe not. It's an open question.

This comment is related to the comment 19 below, and our response to that comment partly justifies the use of only regional models to model to compute the response to second millennium deglaciation. In particular, we point to the first paragraph of the Global models subsection of the Materials and Methods, which we added in response to point 19. Here we point to the recent study by Pan et al. (2024), which showed that most of the deformation from second millennium deglaciation has occurred within a low-viscosity asthenosphere, and this produces only local deformation near the region of deglaciation. This justifies using only regional models (not global models) to compute the response to second millennium deglaciation. We now added a pointer to the Materials and Methods section here, but we also added an explanation sentence in the main text, which reads:
"This is because the mantle response to second millennium deglaciation is mostly sensitive to the viscosity of the shallow asthenosphere [42], resulting in uplift patterns confined to short wavelengths (100s of km) corresponding to asthenospheric depths. This indicates that long-wavelength deformation in response to second millennium ice loading is minimal, and we do not compute Earth's response to second millennium loading outside of the loading area (as we do for the last glacial cycle and satellite altimetry loads, **see Materials and Methods**).**"**

12) Line 175: I suggest rephrasing the sentence, as the " …, followed by …" is confusing. Perhaps start: "The largest predicted uplift rates occur when imposing a thin lithosphere …".

We have made the change roughly following the suggestion:
"The largest predicted uplift rates occur for a thin elastic lithosphere (**rates up** to 11.2 mm/yr), **or for** a low viscosity or wide track (**rates** up to 6.3 mm/yr) (Fig. S7)"

13) Line 192-193: This seems contrary to lines 172-173. I think you mean for the last glacial cycle. Better call that out explicitly.

This is a good point, and we have made this change as suggested. Here is what we wrote:
"However, present-day uplift rates at the GNSS sites**, as driven by last glacial cycle loading,** are rather insensitive to variations in plume track characteristics and lithospheric thickness (Fig. S10)."

14) Lines 197-206: This material could be shortened. The main point is to deal with the four GNSS stations, and this is somewhat of a side note. Figure 2 might be deleted or set aside to the Supplemental material.

We agree that the originally submitted version of the paper predicted uplift at the GNSS sites occurring thousands of years ago. This was indeed a side note – our intent was to show that these locations may have experienced extremely rapid uplift also in the past. This was interesting, but we had no data to show this. However, now reviewer 3 has pointed us to sea level data that provides an important constraint on uplift patterns in the past. We have used this to revise Figure 2. Please see our response to Reviewer 3 at comment 2 for a detailed description of how we made these changes.

15) Figure captions: The caption titles in bold seem very long.

We agree, and we have gone through and shortened all of the bold titles in the main text, and most of them in the supplementary material. We did this mostly by removing information from the bold titles that is also included in the main text of the caption. We also shortened "vertical surface displacement rate" to "uplift rate" in many places, and generally tried to simplify the wording in the caption titles.

16) Lines 328 -335: Figure 6 is very well conceived. Can the authors explain how these results might change had compressibility and density stratification been imposed in the model? Incompressibility tends to be biased to lower prediction of uplift rate, for example.

We note that Tanaka et al. (2011) say in their abstract: "The results show that the effect of compressibility on the vertical displacement rate is small whereas the horizontal rates are markedly enhanced." Their calculation was for ICE5G/VM2, mostly for longer timescales and length scales than we address here. It is unclear how compressibility would affect our results – and thus, we are reluctant to speculate too much about it here. Furthermore, see our response to the first comment of Reviewer 1 regarding (in)compressibility.

17) Lines 374-375: "can have implications" is too vague. Perhaps phrase as "can change the force balances controlling flow at the site of an adjacent...".

We agree and have changed the text as suggested:
"Thus, ice mass changes at one glacier can **change the force balance controlling flow at the site of an adjacent glacier,** if both glaciers are underlain by a common low-viscosity region."

18) Line 381: The value of $1x10^{18}$ Pa s seems more appropriate to a region where there is active volcanism (Iceland, Patagonia, northern Antarctic Peninsula, Svalbard, Alaska), and that therefore a map-view of the prediction at $5x10^{18}$ might also be appropriate.

We appreciate that $1x10^{18}$ Pa s is a very low viscosity, but in Figure 7 we are looking at end-member cases to see what is needed to explain the observed uplift. However, we have also now added panel C showing the uplift rates at 8.5 ka bp for a model with a low viscosity layer of $5x10^{18}$ Pa s that was suggested by the reviewer. We also added panel D where we compare uplift rates for the past 25 thousand years at the locations Am and SD, for a variety of layered models. Importantly, this figure compares uplift rates to constraints from Holocene sea level, as suggested by reviewer 3.

***19) Line 426:** So far in the paper there has been no explanation of how the model is run in the period of altimetric mass balance (i.e. after 1992). In section (lines 559-567) that comes in the Methods Section, it is explained that a Farrell's Green's function method (elastic) is used. If this is what is used over 1992-present, then the predicted results are incorrect, and the models will have to be rerun with full viscoelasticity. This is because at such a low viscosity, the viscoelastic effects are dominated by the viscous element. For example, see the statement by Kieruff et al on page 1529, 1st column, second to last paragraph.

"If low viscosity values in the range of 1018 Pa s as found by Mémin et al. (2014) can be confirmed, then also our PDIM investigation should repeated with viscoelastic modelling instead of a purely elastic one. This is because such low viscosities lead to an early (after a few years only) viscous response of the Earth to PDIM Nield et al. (e.g. 2014)."

Having done this modeling many times (see Lange et al, Ivins et al. 2011), and noting that Spada et al (2011) point out the same thing, this is critical to the correct prediction. (Also please see the recent paper by Ivins et al. (2023) Figures 5-9, dotted lines are uplift rates). So, the question remains: is viscoelasticity included in the ice change driven solid Earth deformation during the altimetric period?

This is a very good point, and it cuts to the heart of our modelling effort – so it is important that our methods are clear. In short and to answer the final critical question: We DO include viscoelasticity for ice changes during the altimetric period. Viscoelasticity is included in our regional models with ASPECT, and we apply these models for all three timescales (last glacial cycle, second millennium, and satellite altimetry era). This modelling is described at the beginning of the Materials and Methods section. Instead, it is only the deformation due to far-field loads (those occurring OUTSIDE of our ice loading area) for the satellite altimetry era timescale that we treat with an elastic approach.

Based on this question, we realize that our initial description of the separation into regional modelling for local loading (within the ice loading area, teal box of Fig. 1) and global modelling for far-field loading (outside the ice loading area) was not completely clear. To clarify our modelling approach, we have made the following changes:

- In the Materials and Methods, we have now separated the regional modelling and global modelling efforts into only two sub-sections. This shows the distinction between then, and the sub-heading titles spell out which loads are handled by which type of modelling. These sub-heading are:
  "Regional models: Viscoelastic deformation driven by deglaciation of Southeast Greenland" and
  "Global models: Viscoelastic deformation driven by deglaciation outside of Southeast Greenland"
- We have added an introductory paragraph to the Global models subsection of the Materials and Methods. This paragraph spells out which ice loads are handled by the global models (those outside the ice loading area) and which types of global modelling we use for each timescale (viscoelastic for last glacial cycle and elastic for satellite altimetry era). Remember that these global models are used ONLY for ice loading occurring in the far field – that is, outside of the ice loading area. We also describe briefly why we make this separation, which is basically because only the longer timescale loading (last glacial cycle) occurring outside the ice loading area can produce sizeable vertical motion within SE Greenland. For this, we cite the recent work of *Pan et al.* (2024) (reference 61) who note that only longer timescale loading is sensitive to viscosities beneath the asthenosphere, and thus can drive deformation at longer wavelengths. For shorter timescales, deformation is mostly sensitive to the viscosity of the shallower asthenosphere, but then the deformation will be mostly local (and handled by our regional modelling). This new paragraph reads as follows:
  "The above-described regional models cannot compute solid Earth deformation due to loads positioned outside the ice loading area (Fig. 1A, teal box). Of these loads, only last glacial cycle loading has a sufficiently long timescale such that far-field loads can drive local ground motion via viscous deformation of the sub-asthenospheric mantle [42] We compute this deformation using global viscoelastic models, as described below. Because second millennium and satellite altimetry era loads

are more recent, they have only had time to drive significant deformation within the low-viscosity asthenosphere, and thus they can only produce ground motion locally [42]. Thus, we can ignore the viscous contribution from far-field loading at these timescales. We do, however, compute Earth's elastic response to satellite era deglaciation across Greenland, because this deformation occurs instantaneously and has a long-wavelength component that contributes to the GNSS observations."

- For the paragraph about the elastic deformation, we added a sentence to clarify that this modelling is only for far-field loads, while the response to local loading is handled viscoelastically (and is much larger):
  "Overall, this elastic response to distant deglaciation is much smaller in Southeast Greenland than the viscoelastic response to recent deglaciation within the ice loading area, which is captured by our regional models."
- At the beginning of the Regional models section, we changed the first sentence to specify that these models compute the viscoelastic response:
  "For regional-scale loads (those within the ice loading area), we use ASPECT (Advanced Solver for Problems in Earth's ConvecTion) v2.4.0 [23 ,24, 25, 30, 26] to model viscoelastic solid Earth deformation in Southeast Greenland."

20) Line 563: Green is a man with a function.

We added the apostrophe, as suggested: "following Farrell's [83] Green**'**s function approach"

21) Lines 570-573: For runs with SELEN, was the density also averaged, and was the core shrunk to near zero effective radius? Some explanation is needed.

For runs with SELEN the density was not averaged. We do not understand the comment about the core. Perhaps the confusion comes from Table S1 which states that the core lower radius is 0. In Table S1 we changed the 0 to N/A as the core does not have a lower radius like the Earth layers above the core, but only an outer radius. In ASPECT we model a box and not a sphere, as stated in the first and third paragraph of Materials and Methods. A spherical model with a free surface has not yet been benchmarked in ASPECT.

22) Comment on the Supplementary Material: Work by Divine et al. tend to support the regional cooling that began at about 1400 CE, as in your model.

This comment highlights the fact that we did not say much about the ice mass changes during the second millennium. Therefore, we have used this comment to add some context about these mass changes in the main text and the Supplementary Material (Figure S1). In both places we now mention that these ice mass changes are due to ice mass changes during the Little Ice Age. In the main text we cite Divine et al. (2008) and we write:
"For the second millennium we use a Bayesian estimate of ice mass change [6] (Fig. S1) **for the Little Ice Age, with maximum glacial extents during 1400-1900, consistent with ice core constraints on cooling during this period [31]."**

[31] Divine, D. V., E. Isaksson, H. Meijer, R. S. W. van de Wal, T. Martma, V. Pohjola, J. Moore, B. Sjögren, and F. Godtliebsen (2008). Deuterium excess record from a small Arctic ice cap, *Journal of Geophysical Research: Atmospheres*, 113(D19104), doi:10.1029/2008JD010076.

***23) Lines 419-426:** "This finding has implications …". Sorry, the fact is that many of these areas have been both measured with GNSS and modeled with both 2nd Millenium changes, and 20th Century deglaciation along with contemporary mass balance (from altimetry, GRACE and IOM methods). These include papers by Larsen for Alaska and Dietrich/Lange for Patagonia and Ivins et al 2011 for the northern Antarctic Peninsula.

This is a good point that rapid uplift has been attributed to deglaciation above low-viscosity regions for several other regions in the past. It is important to point this out, and we have done this by adding citations to the three references mentioned at the end of this sentence.
"This finding has implications for the interpretation of GNSS uplift rates near areas of past and current (de)glaciation, **as has been suggested for Alaska, Patagonia, and the Antarctic Peninsula [e.g., 64, 65, 66]."**

[64] Larsen, C. F., R. J. Motyka, J. T. Freymueller, K. A. Echelmeyer, and E. R. Ivins (2004). Rapid uplift of southern Alaska caused by recent ice loss, *Geophysical Journal International*, 158(3), 1118-1133, doi:10.1111/j.1365-246X.2004.02356.x.

[65] Dietrich, R., E. R. Ivins, G. Casassa, H. Lange, J. Wendt, and M. Fritsche (2010). Rapid crustal uplift in Patagonia due to enhanced ice loss, *Earth and Planetary Science Letters*, 289(1), 22-29, doi:https://doi.org/10.1016/j.epsl.2009.10.021.

[66] Ivins, E. R., M. M. Watkins, D.-N. Yuan, R. Dietrich, G. Casassa, and A. Rülke (2011), On-land ice loss and glacial isostatic adjustment at the Drake Passage: 2003–2009, *Journal of Geophysical Research: Solid Earth*, *116*(B2), doi:https://doi.org/10.1029/2010JB007607.

***24) Lines 433 to 449:** starting "Such large uplift rates can have implications … for local glacier dynamics …" All of this is speculation. And I say this because no one has studied the types of feedback mechanisms that are computed in various simulations of Thwaites, Pine Island (and by Whitehouse for the Weddell Sea) for Greenland. In fact, Jason Briner's recent numerical model was run with coupling capability (Briner et al, Nature vol. 586, 2020), but they found that solid Earth deformation did not influence the glacier/ice sheet evolution. However, what can (and should) be stated is that given the very low viscosity values near 1018 Pa s, FUTURE numerical simulations should consider rapid vertical motions that may rapidly alter topography, beds slopes, and ultimately play a role in controlling the rates of ice mass discharge from the Greenland interior.

We agree that this paragraph is largely speculation, and it would be good to emphasize the importance for future studies, as the reviewer suggests. Thus, we have rewritten this paragraph to reduce the speculation and highlight that our results are important for future studies. We now emphasize that ground uplift has so far not shown feedbacks with ice flow, citing Briner et al., (2020). However, our results now clearly predict rapid uplift, both past and in the present, above the low viscosity regions of the plume track, and that this rapid uplift could affect glacier dynamics in important ways. Below is the revised paragraph (new or moved text is highlighted):
"Such large uplift rates during the last deglaciation can have implications for local glacier dynamics on the periphery of the ice sheet, and thus may be important for ice sheet evolution and stability. For instance, we note that tidewater (i.e., marine-terminating) glaciers are the dominant type of outlet glacier in eastern Greenland. Among these, the Kangerlussuaq glacier is characterized by a reverse bed slope [67, 38] **that can facilitate runaway retreat. However, if the bedrock beneath a tidewater glaciers is uplifted quickly, the relative sea level falls and the grounding line can advance,** stabilizing the glacier [68, **69**]. **This is the case for the Thwaites glacier in West Antarctica, where the uplift from low-viscosity mantle can inhibit marine ice sheet instability, potentially reducing ice mass loss by over 20% after ~100 years [70]. Such feedback between glacier dynamics and solid Earth uplift has not yet been identified for Greenland [71]. However, given that our models predict** large uplift rates (up to ~60 mm/yr) during periods of rapid ice mass loss in the past **(e.g., Fig. 2), models of future deglaciation should consider the impact of rapid ground uplift immediately following deglaciation of Southeast Greenland. Recently,** the Kangerlussuaq glacier has experienced significant thinning and retreat (~200 m over 10 years) [38] and a 9 km retreat earlier in 20th century after the collapse of a large ice tongue (i.e., floating section) [72]. Such large rates of ice mass loss, **sitting above the weakened viscosities (possibly as low as ~10^{18} Pa s) of the Iceland plume track, are driving rapid ground uplift with potentially important** implications for both grounding line movement and glacier stability [1]."

[69] van Calcar, C. J., J. Bernales, C. Berends, W. van der Wal, R. van de Wal (preprint). Bedrock uplift reduces Antarctic sea-level contribution over next centuries. Doi: 10.21203/rs.3.rs-4863941/v1.

[71] Briner et al. (2020). Rate of mass loss from the Greenland Ice Sheet will exceed Holocene values this century, Nature, 586, p. 70-74, doi: 10.1038/s41586-020-2742-6

***25) Lines 457:** The work of Paxman et al. made no attempt to predict uplift but used a sophisticated 'rheological calculator' developed by Chris Havlin that simultaneously models the seismic regime, and within that framework it is possible to predict viscosity appropriate to a 100-year time scale. Also, Adhikari pointed to a discrepancy in prediction using the standard viscosity models of GIA, thus indicating either transient viscosity, or the need to expand the RSL data set. Neither of these studies used transient rheological models, as are routinely used in models of post-seismic relaxation of large to great earthquakes. What I recommend as an improved discussion is that – for these four stations - it is possible to appeal to a Maxwell viscoelasticity that is logically posed with a laterally varying low viscosity channel that tracks the plume path. In other words, there really isn't much of a straw man (transient) to beat on yet ϑ. However, the laboratory data sets, the necessity to satisfy tidal data, and the post-seismic relaxation studies, make transient viscosity quite compelling since Maxwell models can't handle them.

We agree, and have changed the discussion in the last paragraph accordingly. We have removed the implication that the Paxman et al (2023) study constrained viscosities geodetically and replaced this with a reference to a "sophisticated rheological calculator, constrained by laboratory studies". We also mentioned the effective viscosity that results on decadal timescales ($<10^{19}$ Pa s) and followed the reviewer's suggestion to state that the Southeast Greenland uplift rates can instead be the result of lateral variations consistent with the Iceland plume track. We wrote:
"For example, *Paxman et al.* [77] used a sophisticated rheological calculator, constrained by rock deformation studies, to estimate effective viscosities less than 10^{19} Pa s beneath most of Greenland for decadal timescales, and increasing viscosities for longer timescales. By contrast, *Pan et al.* [42] showed using 1D models with linear viscosity that a moderate low-viscosity zone beneath the lithosphere can reconcile **generally slower** Holocene relative sea-level curves with faster GNSS-derived **recent** uplift rates. **Our results similarly employ a simple (linear) viscoelastic rheology law across timescales ranging from thousands of years to decades, but also emphasize the importance of lateral variations consistent with the Iceland plume track."**

We also added a specification in several places that we are using Maxwell viscosity (beginning of the last paragraph of the Discussion and the end of the first paragraph of the Materials and Methods). We have also added a reference to Ivins et al. (J. Geodesy, 2023) in the last paragraph of the discussion, where we discuss transient rheologies.

[78] Ivins, E. R., L. Caron, and S. Adhikari (2023). Anthropocene isostatic adjustment on an anelastic mantle, Journal of Geodesy, 97(10), p. 92, doi:10.1007/s00190-023-01781-7.

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Spada, G., Colleoni, F. and Ruggieri, G., (2011) Shallow upper mantle rheology and secular ice sheet fluctuations, Tectonophysics, 511, 89–98.

Thank you for this list of important references. We have now cited many of them, as described in the points above.

Reviewer #3: Fred Richards (Remarks to the Author)

It is becoming increasingly clear from polar GNSS observations that, in certain regions of Greenland (and Antarctica), recent ice melting is inducing viscoelastic bedrock rebound at rates far faster than expected for commonly assumed upper mantle viscosities (~10^{20} Pa s). This rapid surface uplift has important ramifications for our understanding of near-future sea-level change, as it may help to slow or stabilise grounding line retreat on decadal to centennial timescales. Understanding the cause of this fast deformation, and how it might interact with future ice-sheet dynamics, is therefore of major societal importance. This study tackles this crucial question by investigating the degree to which GNSS derived bedrock uplift rates in SE Greenland can be explained by potential Iceland plume-induced modification of mantle thermomechanical properties. Using a novel implementation of the ASPECT geodynamic modelling software to simulate bedrock adjustment to ice load changes, they show that if very low asthenospheric viscosity (~10^{18} Pa s) and lithospheric thickness (30–60 km) exists beneath the region of SE Greenland potentially impacted by the Iceland plume, rapid uplift can be produced in response to recent ice melting (i.e., since 1000 AD). This signal provides a good match to GNSS-derived bedrock uplift rates that previous glacial isostatic adjustment (GIA) models have struggled to reproduce. Importantly, the invoked rheological weakening implies that the GIA response to ice load changes during the last glacial cycle (122–0 ka) has mostly decayed away in this part of Greenland and is a minor contributor to present bedrock rebound rates. This result has important implications for current satellite-gravity-derived estimates of regional ice mass loss that use corrections which generally predict that the GIA response to the Last Deglaciation (~20–7ka) is still ongoing in this region.

Thank you for this statement about the importance of our study.

Overall, I think this a pioneering and well-executed study that will be of interest to researchers in multiple Earth science disciplines. It provides evidence that previously identified discrepancies between present-day GNSS-derived uplift rates and those predicted by simple GIA models might be resolved by accounting for recent ice melting and potentially plausible 3D variations in Earth's thermomechanical structure. However, I believe more support for their current conclusions is needed. In particular, independent evidence for the spatial pattern and magnitude of the invoked viscosity variations, and a brief assessment of their compatibility with longer timescale constraints on GIA-induced solid Earth deformation such as Holocene sea-level markers. Although the authors show that they can obtain reasonable fits to GNSS uplift rates, as it stands, it is not clear that the low steady-state viscosities they require beneath large portions of SE Greenland are compatible with independent geophysical constraints on present-day mantle viscosity or longer timescale constraints on deglacial GIA-induced solid Earth deformation in SE Greenland. Their statement that their *"results confirm that complex rheologies are not necessary to explain rapid recent uplift, even for the most rapidly uplifting regions"* is therefore questionable at present, in my opinion.

This statement in the original text was also questioned by Reviewer 2. Therefore, we have revised the way we discuss complex rheologies. Please see our response to Reviewer 2, comment 25, and the changes we made in the last paragraph of the main text. Furthermore, based on your feedback we have added analyses comparing Holocene sea level rates from observations and our modeling. We believe this analysis strengthens the manuscript.

If these issues can be resolved (see General Comments below), along with my more minor concerns, I would be very happy to see this manuscript published in Communications Earth and Environment.

We have responded to all of the comments below, and we feel that the changes we made have improved the manuscript – thank you for these constructive comments. Changes to the manuscript are indicated in **bold**.

General comments

1) Lack of independent evidence for magnitude and geometry of inferred asthenospheric low-viscosity zone.

While there's substantial evidence that the Iceland plume has affected the thermomechanical properties of the mantle beneath Greenland in the past—and probably continues to do so along its SE margin—it is less clear that it presently sustains a 10^{18}–10^{19} Pa s low-viscosity zone that extends ~1000 km northeastwards along the Iceland plume track from its present-day position. Inferring viscosity from seismic velocity is non-trivial, but if there really was a ~150–200 km thick asthenospheric low viscosity region beneath the track, one would expect there to be a very strong negative seismic velocity signal associated with this feature. While the quoted seismic tomographic studies (e.g., Mordret, 2018, JGR and Celli et al., 2021, EPSL) do find low seismic velocities in the asthenosphere beneath parts of SE Greenland, they are confined to a margin-parallel patch between Kangerlussuaq and Scoresby Sound (Kangertittivaq). Moreover, the studies that have attempted to estimate viscosity within this low-velocity region have found relatively high viscosity (~1-5 x 10^{19} in Mordret at al., 2018 and Milne et al., 2019, GJI) compared to what appears to be required to fit many of the GNSS-derived uplift rates (<10^{19} Pa s for all but KUAQ). Finally, fitting KSNB and getting close to fitting PLPK (the two most southern GNSS stations) appears to require the imposition of a continuous low-viscosity layer, implying that the track-aligned (rather than margin-aligned) geometry of the low-viscosity zone imposed in this study may not be a good approximation of reality.

Given the above, I feel that the lack of independent evidence to support the magnitude and geometry of the inferred low-viscosity zone needs to be made clearer (although I acknowledge that the geometry issue is partially addressed with in the "Uplift rates at PLPK and KSNB" section). This discrepancy is quite interesting in my view, since it implies that a mechanism in addition to lateral steady-state viscosity variations is needed to explain the GNSS observations (see below).

With the newly added analysis on Holocene uplift rates, we feel like we answer these concerns. For these changes in the manuscript, we refer to our response on your next comment.

Additionally, we added a paragraph in the Discussion discussing our inferred extent of the weakened Earth structure, and constraints on a potentially weak Earth structure in the interior of Greenland:

"We explore the extent of the low-viscosity region by comparing modeled uplift rates with observations both to the north (toward Jameson Land) and south (near the Helheim glacier) of our modelled plume track. We find that the weakened region must extend to the southern sites, and we suggest a transition to a stiffer Earth structure near Scoresby Sound. These models are run with a low-viscosity layer, where we thus assign a weak Earth structure also to the interior of Greenland. While our estimations of the extent of the weakened region are based on observations of uplift along the Southeast Greenland coast, the lack of such constraints from Greenland's interior means that the westward (inland) extent of the weakened region remains uncertain. Geophysical observations provide some constraints: Seismic studies show slow velocity anomalies penetrating into Central Greenland [14, 16], but with velocity contrasts that are smaller than observed along the coast [12]. Geothermal heat flux observations [61, 9] and modeling [11] suggest that the Iceland plume may have weakened the lithosphere of interior Greenland, but substantially depend on a single controversial heat flow observation at NGRIP (North GReenland Ice core Project) [61]. The inclusion of other geophysical constraints from gravity and magnetics suggests only moderate lithospheric thinning in central Greenland between cratonic blocks, but confirm the presence of thin lithosphere along the southeast coast [62]. Our results are consistent with weakening instilled by the Iceland plume along the coast, and are not dependent on a similar weak Earth structure extending into Greenland's interior. This is because uplift rates are most sensitive to local asthenospheric viscosities [18], although nearby weak asthenosphere can moderately affect uplift rates (e.g., Fig. 7A and 7B). Thus, our modeling provides improved constraints on the Earth structure along the southeast coastal margin, but not the interior of Greenland. Our regional models represent an improvement, but

emphasize the need for better geophysical constraints on the heterogeneous viscosity structure beneath Greenland."

We also moved the already Discussion with Adhikari et al. and Khan et al. up and shortened it.

[61] Colgan. W., et al. (2022), Greenland geothermal heat flow database and map (version 1), Earth System Science Data, 14, p.2209-2238, doi: 10.5194/essd-14-2209-2022

[62] Wansing, A, J. Ebbing, and M. Moorkamp (2024). The lithospheric structure of Greenland from a stepwise forward and inverse modelling approach, Geophysical Journal International, 238(2), p. 719-741, doi: 10.1093/gji/ggae183.

2) Fit of predicted deformation to longer timescale constraints and implications for transient rheology.

I realise it is not currently possible to model transient rheologies in ASPECT; however, recent work (e.g., Adhikari et al., 2021, GRL; Lau et al., 2021, JGR; Paxman et al., 2023, AGU Advances; Richards & Hazzard, 2023, JGR) implies that the effective viscosities recorded by polar GNSS networks scale with ice loading/unloading timescale, pointing to the operation of transient creep in these regions. This behaviour could enable the reconciliation of the viscosities assumed in this study with those inferred from seismic tomography, since it would result in modern melting signals triggering deformation with ~10^1–10^2 times lower effective viscosities than those associated with the Last Deglaciation. Indeed, I suspect transient rheology may need to be invoked because, while the low steady-state viscosities inferred here can locally fit GNSS data, they appear to produce very rapid vertical displacement rates in the early Holocene. These rates reach ~80 mm/yr for the VFDG GNSS station at ~8 ka assuming a 600 km wide plume track and 10_{19} Pa s asthenospheric viscosity (Fig. 2); however, based on Fig. S17 it seems like present-day uplift at this station is only reproducible with a 10^{18} Pa s viscosity and 600 km wide plume track, indicating that even higher early Holocene rebound rates would likely be predicted for the best-fitting model at this site. At first glance, these very rapid rebound rates appear at odds with nearby relative sea-level constraints. For example, the Schuchert Dal sea-level markers compiled by Hall et al. (2010, QSR) document only a ~20 mm/yr early Holocene (11–8 ka) RSL fall, a signal that can be related to bedrock uplift rates relatively directly (local glacier recession is assumed to have largely ceased by ~12 ka so fall in sea-surface height due to reduced gravitational pull of the Greenland Ice Sheet on the surrounding ocean is likely a minor contribution). That said, the data-model agreement may be more reasonable if ASPECT predictions of total early Holocene bedrock elevation change, rather than rate of change, are compared with the observed RSL fall.

Below we discuss our analysis of the Holocene sea level observations in more detail. However, we point to two aspects of our analysis that relate to some of the points mentioned above. First, our estimates of sea level rise during the Holocene indeed do reasonably match the uplift rates observed at Schuchert Dal and Ammassalik, which we agree are in the range of 20-30 mm/yr. Uplift rates at these sites also peak near 9-8 ka bp like at VFDG (Fig. 7D), but we estimate average rates that agree with the observations at the sea level sites. Thus, our the uplift rates from our models with a low-viscosity region are not inconsistent with observed uplift rates. Second, we found it easier to compare uplift rates, rather than total uplift, in our analysis. This is because we measure uplift rates during a range for time for which observations are available, and these ranges of time can be extracted from the predicted uplift rates in our models. This time period is 11.0-8.5 ka bp for Schuchert Dal and 10.5 to 8.0 ka bp for Ammassalik. Furthermore, there is still a bit of eustatic sea level rise (including from Greenland) during this time period, and to thus to relate observations of Holocene relative sea level change to model predictions of uplift, we had to estimate rates of relative sea level change associated with ice melting outside of Greenland. Please see the last paragraph of the "Holocene uplift rates" (in the Materials and Methods) for this analysis.

In any case, some direct comparison of the ASPECT model outputs with Holocene sea-level records in the study region (e.g., Schuchert Dal [Hall et al., 2010, QSR] and Ammassalik [Long et al., 2008, EPSL & QSR]) is needed to demonstrate that the wide plume tracks and low lithospheric thicknesses and viscosities obtained from the GNSS observations are compatible with longer timescale constraints. Whatever the result of this comparison, it will have important and interesting implications for whether—in addition to lateral viscosity variations—transient creep mechanisms need to be invoked to simultaneously explain sea-level records and GNSS-derived uplift rates in Southeast Greenland, a topic of considerable interest in the GIA community.

From your feedback we appreciate and agree about the importance of looking at uplift/subsidence over the Holocene based on relative sea level markers, to see how these Earth models perform on different timescales. Therefore, we added an extensive comparison of our modeled rates and observed rates at Schuchert Dal (SD) and Ammassalik (Am), which lie within our ice loading area, for weakened Earth structures specifically. The sea level constraints indicate more than 20 mm/yr of uplift at both locations. What we found is that our "plume track" models actually underpredict uplift rates at these locations (see the new panel B of Figure 2 for a comparison). However, we also examined the "layer" case at these locations, which employs a weakened asthenosphere and thin lithosphere. We found that these models actually do a rather good job of fitting the Holocene sea level constraints, even without transient rheology. To see this, see the new panel D of Figure 7. This suggests that that the region of weakened Earth structure may extend further north (toward SD) and south (toward Am) along the coast. The southward extension also helps to explain the rapid uplift observed at PLPK and KSNB. The analysis of the Holocene sea level constraints is an important addition to our work, and we have made extensive changes to the manuscript to incorporate the new constraints – below is a summary of the changes that we made. We really appreciate this comment, as it expanded the scope of our work considerably.

In the abstract we added: "**Our models also indicate that rapid uplift similarly followed deglaciation at the end of the last ice age, as recorded by Holocene indicators of rapid relative sea level drop along Southeast Greenland's coastline.**"

In the Introduction we added: "We combined these global and regional deformations to model uplift rates in Southeast Greenland, **which we subsequently compared to observed rates of present-day uplift from GNSS and Holocene relative sea level (RSL) drop**."

In the Approach section we added: "We compare the combined uplift with GNSS observations from Southeast Greenland. **We also compare vertical rates during the Holocene at two locations in Southeast Greenland where geologic indicators of sea level change provide constraints on uplift**."

Figure 1B now also includes the locations of the two sea level sites: Schuchert Dal (SD) and Ammassalik (Am).

The first subsection in Results is now called "**Observations of present-day and Holocene uplift rates in Southeast Greenland**" in which we added: "**Constraints on past uplift along the southeast coast of Greenland are scarce, but we use geologic indicators of uplift rates based on sea level markers at two sites: Schuchert Dal (SD) [39] and Ammasssalik (Am) [40] (Fig. 1B). Schuchert Dal is located in the northern part of our ice loading area, within Jameson Land north of Scoresby Sound (Kangertittivaq). The Ammassalik site is located in the eastern part of Ammassalik Island, southeast of the Helheim glacier. Rapid sea level drop, indicating ground uplift, is observed in both locations. We estimate an uplift rate at SD of 28 mm/yr during 11.0 to 8.5 ka bp, and a rate of 24 mm/yr at Am during 10.5 and 8.0 ka bp (Fig. S20, and Material and Methods).**"

In the 'Regional modeling: Last glacial cycle ice mass changes' subsection we added: **"This rapid past uplift was recorded by Holocene relative sea level at SD and Am (Fig. 2B). Our models suggest uplift rates at these locations that are slower than observed (Fig. 2B), and only slightly affected by the plume track width (Fig. 2B), plume track viscosity (Fig. S11B), or lithospheric thickness (Fig. S12B). This is likely because both SD and Am lie outside of the plume track (Fig. 1B). We will revisit Holocene uplift at these sea level sites at the end of the Results section, where we investigate the horizontal extent of the low-viscosity region."**

We also added panel B to Figure 2 with the uplift rates at SD and Am over the Holocene. Furthermore, we split Figure S11 into Figure S11 and S12 and also added the sea level sites as B panels, as for Figure 2.

In the last Results section on 'Uplift rates at PLPK and KSNB' we changed the title '**Extend of the low-viscosity region**' to and we added a new section on analysis at four more GNSS sites and the two sea level sites. In Figure 7 we added panels C and D regarding the sea level sites, and in panel B we added the four new GNSS sites.

We added the following text: **"To test the hypothesis that a weakened Earth structure extends further south and / or north, we investigate uplift rates at four more GNSS sites (Fig. 7B), and we compare uplift rates over the last deglaciation at two sea level sites, Schuchert Dal (SD) [39] and Ammassalik (Am) [40] (Fig. 7C and D). These six new locations lie outside the region of interest, but inside the ice loading area (Fig. 1B). Because these locations are closer to the ice loading boundary, model uplift rates may be less accurate than for the five GNSS stations inside the area of interest (see Materials and Methods). The four new stations are DGJG [55] and SCOR [56] to the north, with slower uplift rates of 3.75±0.81 and 2.07±0.68 mm/yr, respectively, and HEL2 [57] and KULU [58] to the south with rapid uplift rates of 15.93±0.78 and 8.54±0.56 mm/yr, respectively (Fig. S18, and see Material and Methods). DGJG and HEL2 sit close to the present-day ice margin, whereas SCOR and KULU are currently far from the present-day ice margin, but were closer to it during the last deglaciation.**

We find that uplift rates to the north (DGJG and SCOR) are less sensitive to the chosen Earth model than the sites to the south (HEL2 and KULU) (Fig. S19). This is because there is less recent ice melt at the northern locations, while the Helheim glacier to the south is deglaciating rapidly. HEL2, closest to the deglaciating ice, shows the largest variation in uplift rates, with the weakest layer producing the fastest rates. This suggests that rapid uplift (upwards of about 10 mm/yr) requires both rapid deglaciation and weakened asthenosphere. The GNSS observations thus suggest that the weakened region along the Southeast Greenland coastline extends further south than initially modeled (beneath HEL2 and KULU). Such models still come close to matching uplift rates within the original plume track (Fig. 5). The lack of rapid deglaciation to the north (Fig. 1B) leads to slow uplift there even above weakened mantle (Fig. 7B). This prevents us from using GNSS observations to determine the northern extent of the weakened Earth region, as both layered and plume track models produce similar uplift rates at DGJG and SCOR (Fig. S19).

Holocene sea level indicators indicate rapid uplift at both SD to the north (28 mm/yr over 11.0 to 8.5 ka bp) and Am to the south (24 mm/yr over 10.5 and 8.0 ka bp) (Fig. S20). For an Earth model with layered asthenospheric viscosity of $5\cdot10^{18}$ Pa s beneath effectively 22.5 km lithospheric thickness, our models predict large uplift rates at both SD to the north and Am to the south at 8.5 ka bp (Fig. 7C, note the scale). Rapid deglaciation of Jameson Land at the end of the last ice age (but not recently, as Jameson Land is currently deglaciated) explains the rapid uplift observed at SD (Fig. 7D) and predicted at VFDG (Fig. S21), and indicates that the weakened Earth region extends northward toward SD (Fig. 7C). Earth models that are uniformly weak predict modeled uplift rates at Am that are generally slower than at SD (Fig. S22), and models that match observed rates at Am tend to overpredict rates at SD (Fig. 7D). This suggests a transition to a stiffer Earth structure (i.e., higher track viscosity and / or thicker lithosphere) to the north (beyond VFDG and somewhere near SD)."

We added Figure S18: GNSS uplift rates at 4 new sites, like Figure S3.
We added Figure S19: Modeled uplift rates at 4 new sites, like Figure 5.
We added Figure S20: Inferred uplift rates at SD and Am from RSL indicators.
We added Figure S21: Uplift rates at the 5 GNSS stations over the Holocene for the low-viscosity layer models.
We added Figure S22: Spatial plots of last deglaciation uplift rates.

Please also see our response to your first comment for the additional paragraph in the Discussion on the extent of the weak Earth structure from comparing our modeling to GNSS and Holocene RSL observations, and on interior constraints of a weakened Earth structure.

In the Materials and Methods we added info on the 4 new GNSS sites.
"We obtain processed (i.e. data product level 2) GNSS station height information from the Greenland GNSS Network (GNET) from the stations Kangerdlussuaq Gletscher (KUAQ) [32], Mikis Fjord (MIK2) [33], Pilappik (PLPK) [34], Steenstrup Nordre Bræ (KSNB) [35], and Vestfjord Gletscher (VFDG) [36] (Fig. 1B and S3), **and Daugaard Jensen Gletscher (DGJG) [55], Scoresbysund (SCOR) [56], Helheim Glacier (HEL2) [57], and Kulusuk (KULU) [58] (Fig. 7B and S18).**"
"We compute linear regression trends and the standard deviation of the detrended data at these **nine** stations for the period from the start of observations to the end of the VMB data at 31-12-2019 (Fig. S3). The start of observations is 7-8-2009 for KUAQ, 8-8-2009 for MIK2, 12-8-2007 for PLPK, 21-8-2007 for KSNB, 9-8-2009 for VFDG**, 12-08-2009 for DGJG, 02-02-2005 for SCOR, 25-08-2007 for HEL2, and 25-07-1996 for KULU. To determine trends over similar time periods, SCOR and DGJG are processed starting 12-08-2009, and KULU and HEL2 starting 25-08-2007."**

In the Materials and Methods we added info on the Holocene uplift rates:
"Holocene uplift rates

Relative sea level (RSL) estimates are scarce along the southeast coast of Greenland. We use geologic indicators of RSL to constrain uplift rates over the Holocene at two sites: Schuchert Dal (SD) [39] and Ammasssalik (Am) [40] in the northern and southern parts of our ice loading area, respectively (Figs. 1B and 7C). Rapid relative sea level drop, consistent with ground uplift, is observed in both locations during the Holocene following Greenland deglaciation (Fig. S20). For Ammassalik, *Long et al.* [40] compiled RSL estimates based on sediment cores from isolation and lake basins (4 RSL estimates from lakes below the marine limit, 2 upper RSL bounds from lakes above the marine limit) and determined a local marine limit of 69 m at around 11 ka bp from the lower limit of perched boulders above wave-washed bedrock. From these RSL estimates we estimate a land uplift rate of 24 mm/yr during 10.5 and 8.0 ka bp (Fig. S20A). For Schuchert Dal, *Hall et al.* [39] compiled RSL estimates based on field mapping of surficial deposits (shells) and examination of landforms (stratigraphic sections exposed in stream cuts). There are 62 samples of RSL estimates and another 34 with less confidence in water levels (e.g., from fjord samples) providing lower bounds on RSL. From these RSL estimates we estimate a land uplift rate of 28 mm/yr during 11.0 and 8.5 ka bp (Fig. S20B).

We relate our model predictions of uplift rate to RSL observations (Figs. 2, 7D, 11B, and 12B). In doing so, we assume that the observed RSL drop is dominated by bedrock uplift driven by nearby deglaciation. This assumption may be violated in several ways. First, loss of local ice mass loss in Greenland would lower RSL around Greenland due to reduced gravitational attraction of seawater to the ice sheet. Indeed, Greenland lost an ice volume equivalent to 2-3 meters of sea level during 11-8 ka bp [89], which, if distributed around Greenland's periphery, would have resulted in an approximately equivalent depression of the geoid around Greenland [41]. Although the geoid depression may have been larger near areas of more concentrated mass loss, the associated sea level drop (~1 mm/yr) only explains a small part of the 60-70 m that is observed (Fig. S20). Second, uplift or subsidence may be driven by ice loading changes outside of our ice loading area. In particular, the collapse of the peripheral bulge to the North America Ice Complex led to subsidence across much of Greenland [49]. However, our global modelling in SELEN (e.g., to compute Fig. 3D) suggests that this mechanism contribute only ~3.3 mm/yr of subsidence along the Southeast Greenland coast during the early Holocene, mostly driven by North American melt occurring prior to 11 ka bp. Third, eustatic sea level rose by ~25 meters during 11-8 ka bp [e.g., 21], mostly due to ice melting outside of Greenland. Based on normalized sea level fingerprints [90] and ice volume changes [91] we estimate that the Southeast Greenland coast only experienced RSL rise of 7.8 m during 11-8 ka bp, or ~2.6 mm/yr, due to melting of non-Greenlandic ice. In this estimate we exclude Greenlandic ice loss (it is included separately as described above), Laurentide and Antarctic ice contribute 6.0 m and 1.8 m of RSL, and the fingerprint of Fennoscandian ice melt shows a minimal RSL effect in Southeast Greenland [90]. The sum of the three processes described above, together indicating about 5 mm/yr of RSL rise, is uncertain but small compared to the observed sea level drop (Fig. S20) and of opposite sign. Thus, the Holocene land uplift that we estimate for SD and Am (Fig. S20) likely represents a lower bound.

Finally, we note that our explanation for the rapid uplift invokes an extension of the weakened zone to the north and south along the coast to fit the GNSS and Holocene sea level constraints. This explanation does not invoke transient rheology. This does not mean that transient rheology cannot be important, but instead it means that it is not strictly necessary to explain the observed uplift patterns in this area. We already made some changes to the last paragraph of the main text to address transient rheology in response to Reviewer 2 (comment 25). We feel that we have now addressed this topic more carefully.

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[56] International GNNS Service (IGS), *Scoresbysund,* 2009.

[57] The Danish Agency SDFE, UNAVCO Community and M. Bevis, *Greenland GNSS Network – HEL2-Helheim Glacier P.S.,* GPS/GNSS Observations Dataset: The GAGE Facility operated by EarthScope Consortium, 2007. doi: 10.7283/7PB0-2Z53.

[58] The Danish Agency SDFE, UNAVCO Community and M. Bevis, *Greenland GNSS Network - KULU-Kulusuk P.S.,* GPS/GNSS Observations Dataset: The GAGE Facility operated by EarthScope Consortium, 1998. doi: 10.7283/MF3E-KV11.

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[89] Vasskog, K., P. M. Langebroek, J. T. Andrews, J. E. Ø. Nilsen, and A. Nesje (2015). The Greenland Ice Sheet during the last glacial cycle: Current ice loss and contribution to sea-level rise from a palaeoclimatic perspective, *Earth-Science Reviews*, *150*, 45-67, doi:10.1016/j.earscirev.2015.07.006.

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[49] Lecavalier, B. S., G. A. Milne, M. J. R. Simpson, L. Wake, P. Huybrechts, L. Tarasov, K. K. Kjeldsen, S. Funder, A. J. Long, S. Woodroffe, A. S. Dyke and N. K. Larsen (2014). A model of Greenland ice sheet deglaciation constrained by observations of relative sea level and ice extent, *Quaternary Science Reviews,* 102, p. 54-84, doi: 10.1016/j.quascirev.2014.07.018

Line specific comments

1) Lines 38–40: I would suggest "*…a potentially thinner lithosphere and weakened upper mantle beneath parts of southeast Greenland"* or something similar to indicate that the seismic evidence is inconsistent with low mantle viscosity existing across the whole region.

We added "parts of" here to emphasize that the low viscosity mantle may not exist across all of Southeast Greenland:
"… a potentially thinner lithosphere and weakened upper mantle beneath **parts of** Southeast Greenland."

2) Lines 79–81: *"…and a lithosphere that is thinned by…"*. I also think it's worth spelling out where the 25% lithospheric thinning comes from. I realise you cite Heyn & Conrad (2022, GRL) for this, but it would be helpful to say briefly what is done in this study, e.g., "*thinned by Δh (25% of T_{LT}) based on 3D numerical modelling of plume-lithosphere interaction [11] (Fig. 1C)."* Is the 25% based on the average across several model runs or the result of a specific model?

The 25% value is based on a rough analysis of seismic constraints (e.g., Celli et al., 2021) as well as 3D models of thermal ablation of the lithosphere by the plume. We have already clarified our choice of 25% in response to Reviewer 1 (see our response to their comment 2). In particular, we wrote in the Materials and Methods:
"This lithospheric thinning is set to 25% of the surrounding lithospheric thickness (Fig. 1C), which is consistent with models of **thermal ablation by** plume-lithosphere interaction [11] **and indications from seismic tomography [e.g., 14, 15, 16, 12]**."

However, we realized that we did not point to the Materials and Methods here. Thus, we have clarified in the main text:
" … lithosphere that is thinned by Δh (25% of T_{LT}, **see Materials and Methods**) [11] (Fig. 1C)."

3) Results Section; Regional and Global Modelling Subsection: This subsection contains no results and instead describes the approach/methodology so the *"Results"* header seems out of place. Can this header be moved to above the *"Uplift rates in Southeast Greenland"* Section and an *"Approach"* header be added here instead? Otherwise, both can be excluded entirely; I don't think they're needed.

We agree and have introduced a section entitled "Approach" in place of the "*Regional and Global Modelling*" subsection. We have now moved the beginning of the "Results" section to before *"Uplift rates in Southeast Greenland*", which is the first of several subsections that describe the results.

4) Line 132: When you say "*three largest mass-losing glaciers"* do you mean the three largest glaciers that are also net losing mass, or the three glaciers losing the most water mass to the ocean?

This was indeed not clear in our formulation. We mean the first one, thank you for pointing out that this was unclear. We changed it to (on two occasions, also in the section "Extent of the low-viscosity region"):
"… one of Greenland's three largest glaciers, based on catchment size, and also deglaciating rapidly [37]."

5) Line 139: "*A slow elastic response dominates…".* I don't wish to be too pedantic, but elastic responses are effectively instantaneous so "slow" seems to be the wrong adjective here. I would rephrase along the lines of "*If we apply the (layered) VM5i model, the solid Earth deformation is dominantly elastic, with the lack of viscous contribution leading to relatively modest uplift rates."*

This is a good point that the elastic response cannot be "slow". We have used the wording suggested by the reviewer, but have changed it slightly to avoid another pedantic point: there is not a "lack of a viscous contribution" but instead the viscous contribution is so slow as to be unimportant (there is always some viscous contribution). We have written:

"**If** we apply the (layered) VM5i model (Fig. S4, bottom row), **the solid Earth deformation is dominantly elastic, and a minimal viscous contribution leads to relatively modest uplift rates."**

6) Lines 152–153: Maybe worth justifying here why you can discount viscous deformation contributions from outside the loading area. I assume that you can do so by arguing that, where viscous responses to contemporary melting are occurring, these responses are localised to anomalously low viscosity regions outside the loading area. These responses will therefore have minimal impact compared to the elastic deformation, which may be longer wavelength.

We agree that it is a good idea to justify this a bit more here, and the explanation given by the reviewer is correct. We have already addressed this topic in more detail in the Material and Methods, in response to reviewer 2. Please see our response to reviewer 2, point 19. However, we agree that it is useful to provide a short explanation here, as well as a pointer to the Materials and Methods, and we have written:

"Greenland deglaciation outside our ice loading area **can generate rapid local uplift away from Southeast Greenland, but also** elastic deformation that contributes to long-wavelength vertical motion [36] **across** Southeast Greenland. For **this elastic** component, we model global deflections of the solid Earth and sea surfaces **(see Materials and Methods)** … "

7) Lines 153–156: Not clear how this signal is being modelled from the text. Can a brief description of the methodology be given or else a signpost added to the "*Greenland modeling of elastic response"* section of Materials and Methods?

We have added a signpost to the Materials and Methods (see above comment).

8) Figure 4: I'm a little confused by this figure as I expected the middle/middle-right panels of the first three rows to be identical since they are labelled as representing parameters that would equate to those of the reference model (e.g., the 400 km plume track, 60 km lithospheric thickness, and 1 x 10^{19} Pa s track viscosity panels). Are any other parameters varying from row to row in addition to the main parameter being varied, or are the differences purely a visual artefact of the different colour scales being used in each row? If the former, can this be made explicit in the caption; if the latter, then I think this is misleading. In either case, I think the colour scales should be harmonised across the rows for ease of comparison.

This is correct that those panels are the same, and no other parameters are varied. This is indeed a purely visual artefact due to the color scale ranges, and can be confusing. Therefore, we added **(R)** to the relevant plot titles denoting the Reference model, and we added this in the Figure description text: **"(R) denotes the reference model.**"

9) Paragraph beginning on Line 356: I would mention the possibility that invoking transient rheology could better fit these two stations, since it would lower the effective viscosity controlling responses to modern melting (decadal-to-centennial timescales) by a factor of 10–100 across the whole region (see Main Comment above and, e.g., Paxman et al., 2023). Assuming a transient rheology would likely give results very similar to the eventual proposed solution (i.e., a layer with low steady-state viscosity exists beneath the entire loading region).

Based on our new analysis of Holocene sea level constraints at Ammassalik (Am) and Schurchert Dal (SD), we now argue that the weakened Earth region may extend further northward and southward along the SE coast of Greenland. This finding is consistent with the rapid uplift at PLPK and KSNB, which were outside of our original plume track, and 4 more GNSS stations (see your comment 2). Thus, we have significantly changed this section of the paper, the last part of the Results (including the title of the section: **Extent of the low-viscosity region**) to include the Holocene sea level constraints. We note that extending the low-viscosity region to the north and south along the coastline means that it is not necessary to invoke transient rheology to explain such fast uplift – and we discuss this in the last paragraph of the main text (please see our response to Reviewer 2, comment 25).

10) Figure 7 caption: What lithospheric thickness is assumed when there is no track, just a continuous low-viscosity layer? Also 60 km thick? Or 0.75*60 km = 45 km?

This wasn't clearly described indeed. In the text describing Figure 7 we added: "For simplicity, we implemented a full low-viscosity layer **($1{\cdot}10^{18}$ or $5{\cdot}10^{18}$ Pa s)**, by making the plume track width W_{PT} as wide as the model domain (see Fig. 1C). **We tested nominally 30 and 60 km thick lithospheres, which are thinned by 25% everywhere to form effectively 22.5 and 45 km elastic lithospheres**."
In the caption of Figure 7 we added: "These models use a plume track/layer viscosity of $1{\cdot}10^{18}$ Pa s and an **effective** lithospheric thickness of **45 km (60 km outside the plume track)**."

11) Lines 457–461: To me, the Paxman et al., 2023 (P23) results look consistent with what is inferred here despite the relatively low (2°) resolution of the seismic constraints they used. For decadal timescales—which dominate the present-day uplift signal predicted by the lowest viscosity ASPECT models—P23 obtain ~1–4 x 10^{18} Pa s beneath the Kangerlussuaq region (see their Figure 5a, 5f, and S4a), which is compatible with what appears to be required here (1–5 x 10^{18} Pa s). The P23 results also indicate a more widespread low-viscosity zone, which is more compatible with what is needed to fit PLPK and KSNB uplift rates. I think these sentences need to be rephrased accordingly.

We agree and have already adjusted this sentence in response to reviewer 2, point 25. Here is what we wrote:
"For example, *Paxman et al.* [60] used a sophisticated rheological calculator, constrained by rock deformation studies, to estimate effective viscosities less than 10^{19} Pa s beneath most of Greenland for decadal timescales, and increasing viscosities for longer timescales."
Note that we now start with "For example" instead of "By contrast", and we give an estimate of "less than 10^{19} Pa s" for the appropriate viscosity – this is consistent with our estimates of the low viscosity region here. However, note that in Fig. 4a and 4efg of *Paxman et al.* (2023), the reduced viscosities (less than 10^{19} Pa s) on decadal timescales actually cover most of Greenland – they are not noticeably more reduced in the southeast region. This to some extent distinguishes our results from those of Paxman, because our results indicate a reduced viscosity in Southeast Greenland relative to other areas of Greenland.

12) Lines 463–465: I would say "*indicate* that complex rheologies *may not be necessary*" since no evidence has yet been supplied that the low inferred steady-state viscosities are compatible with present-day geophysical observations or longer-timescale relative sea-level constraints. If this issue can be addressed (see Main Comments above) then the current, stronger wording could be justified; however, the evidence supplied is insufficient to warrant it at present.

We agree that we cannot discount the possibility complex rheologies are not relevant here. Therefore, we have adjusted the text here, and we already did so in response to reviewer 2, point 25. Building on the previous comment, we continue with:
"By contrast, *Pan et al.* [61] showed using 1D models with linear viscosity that a moderate low-viscosity zone beneath the lithosphere can reconcile slower Holocene relative sea-level curves with faster GNSS-derived recent uplift rates. **Our results similarly employ a simple (linear) viscoelastic rheology law across timescales ranging from thousands of years to decades, but also emphasize the importance of lateral variations consistent with the Iceland plume track."**
Thus, we have made the distinction between the complex rheologies of *Paxman et al.* (2023) and the simpler, but depth-varying, viscosities employed by *Pan et al.* (2024). Then we emphasize that our simple linear rheology law can also work, but we emphasize here the lateral viscosity variations of the Iceland plume track, which is necessary to explain the very fast uplift of KUAQ and nearby stations.

13) Line 478 and throughout: Capitalize *"Earth"* in *"solid Earth"*. Check throughout document.

We now capitalized Earth globally within the document.

14) Line 482: *"…variety of viscoelastic-plastic rheologies"*.

We added "of" – thanks for catching that.

15) Lines 529–530: "*Greenland experienced subsidence after that ice sheet deglaciated".*

Actually, we intended to use the present tense here, because we are talking about current uplift still happening in response to past deglaciation. However, we agree that this was not conveyed well. We now write:
"… Greenland **is still experiencing** subsidence **following the deglaciation of that ice sheet**."

16) Lines 572–573: For reproducibility, can you quote the value of the volume-averaged mantle density being used here?

We added: "In ASPECT we also use the VM5i rheological model, excluding the core and with constant volume-averaged mantle density **of 4423.61 kg/m^3** [27]."

17) Lines 589–591: The assigned timespans of the satellite altimetry era, second millennium, and glacial cycle loading changes appear to overlap. Is this actually the case? If so, is there not a risk that you're double/triple counting load changes during certain time periods?

All models are run until 2020 (end of our VMB altimetry dataset) to see the deformation at that time. For the glacial cycle loading there are no more ice loading changes in the past 2000 years (see Figure S2). For the second millennium dataset the mass anomaly is zero from 1995 onwards (see Figure S1), and indeed there is an overlap of 3 years with the VMB dataset that starts in 1992. However, over our ice loading area the rate of ice mass loss from the VMB dataset for 1995-2020 is ~4x higher than for 1992-1995, and thus mass loss was much slower during the overlap period than it has been recently. Secondly, the ice mass loss in the overlap period is less than 5% of the total over the satellite altimetry era. We added text to recognize that there is an overlap from the different time period estimates, but also to show how small this overlap is.

- In Figure S1B we added a red line showing the ice mass loss over the satellite altimetry era from the VMB dataset. Comparing this line to the (blue) line for the second millennium shows how small the overlap is between the two datasets.
- In Materials and Methods we added, regarding the second millennium: "We then let the ASPECT model run for another 25 years **with zero loading**, until 2020, to find the solid Earth deformation due to relaxation after second millennium ice loading." Then in the caption to Figure S1, we added: "**There is a 3-year overlap between the second millennium and satellite altimetry datasets (1992-1995). However, the ice mass loss during this short overlap period is less than 5% of the total over the satellite altimetry era (red line) and less than 3.5% of the total second millenium period (blue line).** Time step sizes for the input data and the deformation model are 5 yr."
- And regarding the last glacial cycle: "**The ice loading changes are zero from 2 ka bp onwards (Fig. S2).**"

18) Lines 593–596: Is the solid Earth deformation correction applied to the VMB dataset consistent with what is calculated here for the last glacial cycle loading signal? If it is, this should be made clear; if not, can some reassurance be provided that the discrepancy has a negligible impact on the results?

We use a VMB (altimetric/volume-derived mass balance) product from multiple satellite altimetry missions from *Simonsen et al. (2021)*. *Simonsen et al. (2021)* does not state the correction they applied other than that they corrected for vertical bedrock movement caused by both viscous deformation and elastic rebound of the Earth's crust. We can safely assume that this correction is based on a radially symmetric Earth model, and that the viscous deformation is due to last glacial cycle ice mass changes and the elastic deformation due to contemporary ice mass changes. Thus, there is no correction applied for viscous deformation caused by recent ice mass changes, which we show to be significant in areas of a low-viscosity upper mantle.

Valencic et al. (2024) study the GIA correction for satellite altimetry measurements and show for Antarctica (1) “an uncertainty in total present-day ice volume change equivalent to approximately 10 % of Antarctic ice mass loss inferred for the period 2010–2020” due to (not) incorporating a laterally heterogenous Earth structure, and (2) “a systematic error of $\sim$ 3 % in the projected net ice volume change, with most of the difference arising in areas of West Antarctica above mantle zones of low viscosity” due to (not) incorporating spatial variability in the scaling to convert GIA-corrected ice surface height changes obtained from satellite altimetry to ice volume estimates. In the latter, they incorporate the viscous deformation from recent ice mass changes (that can be on decadal scales for low mantle viscosities).

We already stated that satellite altimetry is less affected by vertical bedrock movement than satellite gravimetry: “Also, estimates based on satellite altimetry are less affected by solid Earth deformation [86]. Solid Earth uplift rates in Greenland are on the order of millimeters [3] whilst the surface elevation change from altimetry is much larger, on the order of meters [87, 47].” And we add to this: “**Note that the GIA correction on the ice surface height changes from satellite altimetry does not include a viscous deformation on short timescales (which we show can be significant above areas of low-viscosity mantle), which can cause an error in ice volume changes estimates of a few percent [88], thereby affecting predicted uplift rates.**”

[88] Valencic, N., L. Pan, K. Latychev, N. Gomez, E. Powell, and J. X. Mitrovica (2024). Mapping geodetically inferred Antarctic ice surface height changes into thickness changes: a sensitivity study, *The Cryosphere*, 18, p. 2969-2978, doi: 10.5194/tc-18-2969-2024.

General comment

Due to restructuring of the paper to accommodate the journal style requirements, the responses to the reviewers got lost in this tracked changes document due to the amount of tracked changes from the restructuring. We have highlighted the changes we made as response to the reviewers comments in yellow. The order of Supplemental Figures also changed due to the restructuring.

Reviewer #1 (Remarks to the Author):

I am satisfied with the authors' response and edits based on the reviewers' comments.
Thank you!

Reviewer #2 (Remarks to the Author):

I laud the authors for such an extensive revision and very thorough response to my review and to the other two reviews. I think the paper is ready to be accepted. I just have a couple changes that I ask for. From my own review and the author response:

1. Item *25.

"… constrained by rock deformation studies …" should be changed to " … constrained by laboratory deformation studies …" since they used granular borneol, which is not a rock.
Done.

2. The newly stated (in response to comments from Rev 3)
"Our results similarly employ a simple (linear) viscoelastic rheology law across timescales ranging from thousands of years to decades, but also emphasize the importance of lateral variations consistent with the Iceland plume track."

Should be changed to

"Our results similarly employ a classical linear Maxwell viscoelastic rheology law across timescales ranging from thousands of years to decades, but also emphasize the importance of lateral variations consistent with the Iceland plume track."
Done.

The rationale for the above nomenclature change is as follows:

Maxwell rheology can assume a power-law dependency (see papers by Wouter van der Wal and Patrick Wu). And transient rheology, as formulated by Yamauchi and Takai (used by Paxman), is actually a linear viscoelastic solid and forms the input to Harriett Lau's new Love number transient viscosity calculator. Also linear rheology is assumed in the compressible version of the Jackson and Faul formulation of transient viscosity now implemented by Lambert Caron in the ISSM Love number calculator, wherein the spherical self-gravitating Earth problem is solved similar to the Lau formulation. To us geodynamics modelers these seem 'complicated', but to the mineral physics community they are, nonetheless, "simple linear viscoelastic rheologies".

This seems like a small technical point, but I think keeping the terminology correct, is the best practice.

With these small changes implemented, I strongly support publication of the manuscript in Communications Earth & Environment.
Thank you for pointing out these two small inconsistencies in our wording, and thank you for your positive view on the revisions!

Reviewer #3 Fred Richards (Remarks to the Author):

The authors have done an impressive and thorough job of addressing my comments and those of the other reviewers. I'm very grateful for their careful consideration of my arguments and I think the extra analysis they've carried out has significantly strengthened their conclusions.
Thank you for your positive view on the revisions!

I have a few minor edits and additions that I think it would be useful for the authors to include (see below), but I am now very happy to see this paper published.

Remaining Comments

1) Low-viscosity Maxwell and transient rheology could both explain the sea-level and GPS data:

I suggested that the authors looked at Holocene relative sea-level (RSL) reconstructions within the study area because, if transient rheology were operating beneath Greenland, I was expecting that fitting rates of early Holocene RSL fall would require a higher optimal viscosity value than fitting the GNSS datasets. This expectation was based on a presumption that the Last Glacial Maximum–to–present deglaciation trends measured by the RSL data would have longer characteristic timescales than the recent ice mass loss trends observed by satellites. What I hadn't realised is that the rate of ice mass loss between ~11 and 8 ka in ICE_6G is approximately equal to the measured present-day rate of deglaciation (Briner, 2022, Oceanography). As a result, even if transient rheology were active beneath Greenland, the effective viscosity required to fit the 8–11 ka average RSL fall/bedrock uplift rate and the GNSS data would be expected to be roughly equivalent. It therefore seems that this particular test (frustratingly!) cannot discriminate between Maxwell rheological models that include a low-viscosity (~1018–1019 Pa s) layer and transient models. Confirming or falsifying the presence of such a low-viscosity zone beneath most of SE Greenland will therefore require either more detailed post-8 ka RSL histories or independent steady-state viscosity estimates (e.g., from improved seismological, magnetotelluric, and/or petrological constraints).

Could this point about not being able to discriminate between Maxwell and transient rheologies based on early Holocene RSL rate and GNSS data alone be acknowledged somewhere in the "Complex Rheologies" section? I feel this is an important point to reiterate, especially in the context of the apparent disagreement between studies invoking transient rheology to fit Greenland observations (e.g., Paxman et al., 2023, AGU Advances and Adhikari et al. 2021, GRL) and others that do not (e.g., this study and the Pan et al., 2024, GJI).
This is a good point. We elaborated on this in the Discussion under Complex Rheologies, split the text into two paragraphs, and make some additional changes (**bold**):

"… . Our results similarly employ a **classical linear Maxwell** viscoelastic rheology **law to successfully predict observed rates of early Holocene and modern uplift rates in Southeast Greenland. However,**

rates of Greenlandic mass loss across millennia in the early Holocene were similar to recent melting rates occurring over the past two decades [64]. Thus, it may be difficult to use a Holocene-to-modern comparison to constrain the potential impact of deglaciation rate on asthenospheric rheology.

Our regional GIA modeling across timescales ranging from thousands of years to decades emphasizes the importance of lateral variations **in rheology** consistent with **those expected for** the Iceland plume track. In particular, we **demonstrate that** localized regions of unusually rapid uplift occur where rapid deglaciation is positioned above pockets of mantle with diminished linear viscosity and thin lithosphere. We have identified one such region **along the coastline of** Southeast Greenland, where uplift in excess of 17 mm/yr is occurring near the rapidly retreating Kangerlussuaq glacier and above mantle that has been weakened by interaction with the Iceland plume."

2) Lines 484-486:

I suggest changing to: "… because the lateral extent of the low-viscosity region may be restricted (e.g., in the case of a plume track), significantly reducing modeled uplift rates." This clarifies that small or reduced lateral extent will reduce rates, rather than any arbitrary lateral extent.
Done.

3) Lines 574-576:

A curve can't be fast or slow. I would suggest rewording to "can reconcile generally slower rates of Holocene relative sea-level fall with faster GNSS-derived uplift rates".
We have changed this sentence from a comparison of uplift occurring across these timescales to a comparison of the rates themselves, because at SD and Am specifically the rates were not slower than GNSS rates: "By contrast, *Pan et al.* [42] showed using 1D models with linear viscosity that a moderate low-viscosity **layer beneath Greenland's lithosphere can reconcile uplift rates inferred for the millennial timescales of Holocene relative sea-level fall with those occurring on the more recent decadal timescales of GNSS observations**."

4) Line 819:

"during and following Greenland deglaciation"? Most of the rapid RSL fall seems to be contemporaneous with rapid ice mass loss (Figure S2A).
Done.

5) Figure 5:

What are the yellow stars (presumably low-viscosity layer of 1 x 1019 Pa s)? Caption and text only mention 1 x 1018 Pa s and 5 x 1018 Pa s low-viscosity layer runs (i.e., the red and blue stars). Also, shouldn't the stars be offset slightly from 30 and 60 km lithospheric thickness, since they represent 22.5 and 45 km lithospheric thickness, respectively? Or else, as in Figure 7, could a short explanation be added in the caption to highlight that these lithospheric values are the same as what is assumed within the 30 and 60 km plume track models within the track itself (I know this can probably be surmised from the text, but it would be good to make this crystal clear). This comments also applies to Figures S5, S8, and S19.
Indeed, we did not do a good job describing the yellow stars. In the caption of Figure 5 we added (bold): "… and a full low-viscosity layer (stars, only for models with effective lithosphere thickness of 22.5 or 45

km**, corresponding to 30 and 60 km outside the plume track**, and asthenosphere viscosity of $1 \cdot 10^{18}$, $5 \cdot 10^{18}$, **or $1 \cdot 10^{19}$** Pa s**, as indicated by star color**)".

Figures S5, S8, S19 (now Figures S7, S9, S14 after restructuring): We refer to the caption of Figure 5 that is now updated.

6) Supplementary Figure 21:

The legend on the figure doesn't appear to match the caption. As in the comment above, I would suggest adding a short explanation to highlight that these lithospheric values are the same as what is assumed within the 30 and 60 km plume track models within the track itself.
Note that with the restructuring, this figure is now Figure S16. We changed/added to the caption (bold): "Vertical surface displacement rates since the last glacial maximum for low-viscosity layers of $1 \cdot 10^{18}$ and $5 \cdot 10^{18}$ Pa s, and **nominal** lithospheric thicknesses of **LT = 30 and 60 km (see legend)** for five GNSS sites. Shown for comparison are results for the layered VM5i rheological model (black), ranges of present-day rates induced by ice loading changes over the satellite altimetry era (blue bar) and second millennium (pink bar). **Note that these low-viscosity layers models are constructed using a plume track that is wider than the model itself. Because the lithospheric thickness is reduced by 25% within the plume track (Figure 1C), the LT = 30 and 60 km models use effective lithospheric thicknesses of 22.5 and 45 km, respectively.**"

Also, we also added this information to the caption of Figure S17: "Shown for Earth models with low-viscosity layers of $1 \cdot 10^{18}$ and $5 \cdot 10^{18}$ Pa s and **nominal** lithospheric thicknesses **of LT = 30 and 60 km (see column headers). Note that these models are constructed using a plume track that is wider than the model itself. Because the lithospheric thickness is reduced by 25% within the plume track (Figure 1C), the LT = 30 and 60 km models use effective lithospheric thicknesses of 22.5 and 45 km, respectively.**"