nature portfolio

Peer Review File

Projections of 21st Century Sea-Level Fall Along Coastal Greenland

Corresponding Author: Ms Lauren Lewright

This file contains all reviewer reports 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:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

[Editorial Note: See attachment at end of file]

Reviewer #2

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #3

(Remarks to the Author)
Review of "Projections of 21st Century Sea-Level Fall Along Coastal Greenland"

This manuscript aims at assessing the relative sea level along the coast of Greenland in the 21st century in response glacial isostatic adjustment and past and future ice-mass changes. My area of work is statistical methodology, and I am no expert in glaciology, so I will narrow my comments on the technical aspects of the methods.
The methods section is mostly a description of very basic approaches such as weighted sum of squares, decomposition of the uplift in each station into a sum of different terms and linear regression, so there is not much to comment on, I assume the choice of models was thorough and that the choice of these methods is based on well-established literature.
The interesting part is the Bayesian framework. This part is written unusually well for an applied work, so I commend the authors for their effort. They propose a hierarchical Bayesian model for the GNSS observations, where the true VLM rates are a combination of 14,040 GIA model solutions.
Some comments
- No validation. The model makes many assumptions, but none are validated. For example, is x at least approximately Gaussian? If one were to fit, say, a skew-Normal or some other members of the exponential family, how would the results change? What about the exponential covariance function in (7), would it change if we had a Matérn covariance? Such assessment should have been done and their predictive skills measures with some cross-validation.
- Lack of a continuous spatial process. While the model proposed is able to explain through the vector c which model better explains the data, it does not assume any continuous spatial process. I am assuming the models have been interpolated on the same grid as the data to avoid this issue. This, however, begs the question: to what extent is the interpolation impacting the results? A much better solution would have been to provide a spatial map of a Multinomial distribution.
- Lack of details on software and hardware. There is a wide variety of software able to perform Gibbs sampling and MCMC (e.g., STAN, JAGS, INLA, etc.). Which one has been used here and why? What computational resources have bene used? What was the computational time?
- Lack of reproducibility. As of now, this work is not reproducible. No code has been provided, so I am not in the position of commenting on it. Given the high standing of the journal, I would have expected, at the very least, a detailed commented

code on a GitHub repository for all methods used. This is in my opinion a considerable shortcoming for this submission.

Reviewer #4

(Remarks to the Author)
In this work, the authors present updated projections of 21st century sea-level change for Greenland, including updated VLM estimates generated by combining GIA models and VLM observations from permanent Global Navigation Satellite Systems (GNSS). My expertise is in sea level projections more broadly, and not in the specifics of GIA modeling/observations. Therefore, I have focused most of my comments below on the authors' generation of complete sea-level change projections, rather than the new VLM estimates.

The manuscript is generally well-written, and most figures are clear. However, while I think this manuscript has the potential to be a meaningful contribution to the sea level and GIA communities, I also have some concerns about the methods the authors are currently using to generate complete sea-level change projections. If the authors can update their approach and the explanation of their methods, I would look forward to revisiting the revised version of the study. My detailed comments are outlined below.

~Andra J. Garner

Major Comments
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Reading through the manuscript, I had quite a few questions about the methodology for the total RSL projections. The <10 line explanation of these methods (Lines 523-531) generated more questions for me than it answered.

Lines 523-526 say, "In addition to sea-level changes caused by GIA, other processes will contribute to the RSL experienced by coastal Greenland over this century. These processes include changes in global glaciers, sterodynamic changes, and changes in terrestrial water storage, excluding glaciers and ice sheets. Note that the contribution from Antarctica is already included in the previous section."

If Antarctic ice loss is already included in the LIA to 2100 Ice Mass change, the first sentence of this section (and much of the results) should be amended to reflect this. Because, in this case, this section is not "Non-GIA related contribution to future sea-level change", but "Contributions to future sea-level change excluding GIA and Ice Sheet Mass Loss". This fact was not clear to me until I reached line 526 of the manuscript.

Lines 526-528 state, "Estimates for the contributions of each of these processes to RSL in Greenland to 2100 CE for RCP 2.6 and RCP 8.5 were obtained from the IPCC AR6."

This is very confusing, since the IPCC AR6 did not make any explicit sea-level change projections for RCP scenarios, but instead focused on SSP scenarios. Furthermore, to my knowledge, the IPCC AR6 did not provide RSL projections for Greenland in the full report--though the datasets made available for the IPCC AR6 projections (which should include additional citations beyond AR6 Chapter 9) did. So, this description leaves me with a lot of questions about where the contributions for each of these "non-GIA" processes actually came from.

Lines 528-529 say that, "The mean of these estimates was made relative to 2017 CE and linearly interpolated at 5 year intervals between 2020 and 2100 CE." What does this mean for AR6 projections that are provided in terms of percentiles (median and likely range)? How did the authors construct a "mean" from these projections that are provided as a distribution?

In addition, the IPCC AR6 projections are provided for a 1995-2014 baseline--so how were the authors' estimates made relative to 2017? Furthermore, when the authors say that these estimates were linearly interpolated at 5 year intervals between 2020 and 2100, does that mean that the first interpolation was actually 3 years (going from 2017-2020), or should the text say that the estimates were linearly interpolated at 5-year intervals from 2022-2097?

Lines 529-531 say that, "For our total estimate of RSL around Greenland for the 21st century, we sum GIA-related contributions associated with the deglaciation and the LIA to 2100 CE, and non-GIA related contributions." How does this work with the uncertainty the authors provide? Are the authors also linearly summing uncertainty across components? This needs explanation, since, again, the AR6 projections are provided in terms of percentiles, and unless a median estimate is being used, the uncertainty across percentiles cannot just be added together.

It seems to me that the correct approach for the analysis the authors present would be to use the "IPCC AR6 Relative Sea Level Projections without Background Component" dataset, which is available here: https://zenodo.org/records/5967269. This dataset was specifically designed to allow scientists to use their own estimates of VLM to generate new RSL projections in a manner consistent with IPCC methodology. Using this dataset, my suggestion would be to simply use the AR6 ice sheet projections (also from ISMIP, which the authors use for their LIA-2100 ice-mass change, and which are already incorporated into the workflows as part of this dataset). The authors could still include the VLM component calculated from their LIA-2100 ice-mass change for their GIA/VLM "background" component of RSL. But, I think from a standpoint of correctly combining components within the AR6 framework, it's best to use the ice sheet estimates that have already been included within the framework in this dataset.

Note that when using this dataset, there are a number of citations/acknowledgements that should be included, as noted on the Zenodo page:

"In order to document the impact of these sea-level rise projections, users of the projections are obligated to cite chapter 9 of Working Group 1 contribution to the the IPCC Sixth Assessment Report, the Framework for Assessment of Changes To Sea-level (FACTS) model description paper, and the version of the data set used:

• Fox-Kemper, B., H.T. Hewitt, C. Xiao, G. Aðalgeirsdóttir, S.S. Drijfhout, T.L. Edwards, N.R. Golledge, M. Hemer, R.E. Kopp, G. Krinner, A. Mix, D. Notz, S. Nowicki, I.S. Nurhati, L. Ruiz, J.-B. Sallée, A.B.A. Slangen, and Y. Yu, 2021: Ocean, Cryosphere and Sea Level Change. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 1211–1362, doi:10.1017/9781009157896.011.
• Kopp, R. E., Garner, G. G., Hermans, T. H. J., Jha, S., Kumar, P., Reedy, A., Slangen, A. B. A., Turilli, M., Edwards, T. L., Gregory, J. M., Koubbe, G., Levermann, A., Merzky, A., Nowicki, S., Palmer, M. D., & Smith, C. (2023). The Framework for Assessing Changes To Sea-Level (FACTS) v1.0: A platform for characterizing parametric and structural uncertainty in future global, relative, and extreme sea-level change. Geoscientific Model Development, 16, 7461–7489. https://doi.org/10.5194/gmd-16-7461-2023
• Garner, G. G., T. Hermans, R. E. Kopp, A. B. A. Slangen, T. L. Edwards, A. Levermann, S. Nowikci, M. D. Palmer, C. Smith, B. Fox-Kemper, H. T. Hewitt, C. Xiao, G. Aðalgeirsdóttir, S. S. Drijfhout, T. L. Edwards, N. R. Golledge, M. Hemer, G. Krinner, A. Mix, D. Notz, S. Nowicki, I. S. Nurhati, L. Ruiz, J-B. Sallée, Y. Yu, L. Hua, T. Palmer, B. Pearson, 2021. IPCC AR6 Sea Level Projections. Version 20210809. Dataset accessed [YYYY-MM-DD] at https://doi.org/10.5281/zenodo.5914709.

Please also include in the acknowledgements of works citing these projections:

We thank the projection authors for developing and making the sea-level rise projections available, multiple funding agencies for supporting the development of the projections, and the NASA Sea-Level Change Team for developing and hosting the IPCC AR6 Sea-Level Projection Tool."

Taking this approach for the total sea level projections (and making it clear that this was the approach) will greatly strengthen the authors' discussion and comparison of their results to the AR6 regional results for Greenland. This approach will ensure that all components (and scenarios) considered are the same as in the AR6 regional projections, allowing the authors to more vividly emphasize the importance of their work on Greenland VLM when it comes to accurately assessing future SLR change for the region.

Finally, the abstract says that the authors predict RSL fall of 2.8 +/- 0.8 m for RCP 8.5. Similarly, Lines 292-293 say, "The fall in RSL by up to almost 3 m that we project to occur by the end of this century along much of the coast . . ." However, looking at Figures 5-7, and considering the accompanying text, it's unclear how the authors conclude that their findings suggest "almost 3 m" of RSL fall "along much of the coast". It looks like up to ~2 m in many places, not 3. Please provide clarification of how the 3 m value is determined--right now I don't see data/results to support this conclusion.

Minor Comments
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Lines 259-261: "For all locations considered here, it is noteworthy that the projections for the two future emissions scenarios (RCP2.6 vs. 8.5) only start to diverge around 2050 CE." This may be noteworthy, but it's probably also worth mentioning that this is consistent with what many other studies have already shown (e.g., Garner et al., 2018, Horton et al., 2018, IPCC AR6 Chapter 9):

• Garner, A. J., J. L. Weiss, R. M. Horton, A. Parris, R. E. Kopp, J. T. Overpeck, B. P. Horton (2018), Evolution of 21st Century Sea-level Rise Projections. Earth's Future. doi: 10.1029/2018EF000991.

• Horton, B. P., R. E. Kopp, A. J. Garner, C. C. Hay, N. S. Khan, K. Roy, and T. A. Shaw (2018), Mapping sea-level change in time, space, and probability. Annual Review of Environment and Resources. 43:1. doi:10.1146/annurev-environ-102017-025826.

• Fox-Kemper, B., H.T. Hewitt, C. Xiao, G. Aðalgeirsdóttir, S.S. Drijfhout, T.L. Edwards, N.R. Golledge, M. Hemer, R.E. Kopp, G. Krinner, A. Mix, D. Notz, S. Nowicki, I.S. Nurhati, L. Ruiz, J.-B. Sallée, A.B.A. Slangen, and Y. Yu, 2021: Ocean, Cryosphere and Sea Level Change. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 1211–1362, doi: 10.1017/9781009157896.011.

Please ensure that the baseline for total RSL projections is stated clearly at least once in the results section for Future

Projections of RSL, and in the captions of all figures that show total RSL projections, to provide clarity to readers.

Comments on Figures
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The maps generally look very good, however the colorbar on some of them becomes quite dark (e.g., Fig. 3b, Fig. 4b,d,f, and h), making portions of the map somewhat difficult to see. Would it be possible to adjust the colorbar to keep it lighter?

The station numbers on the inset map of Greenland in Figure 2 are not legible. I think this could be solved by making the map of Greenland larger--perhaps make it a third panel to the right of panels a) and b)?

The white stars/black points on Figure 5d don't work well. The black points are particularly difficult to see. I'd suggest putting the black points on a different panel than the white stars . . . And perhaps make them a different color and/or shape, so that they don't blend into the map outlines.

In Figure 8, it seems odd to discuss panel c) before a) and b) in the caption. Perhaps consider reordering the figures, or revisiting the caption.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
We appreciate the improvements made by the authors and respect their choices, nevertheless we add an annotated manuscript with what we consider minor suggestions for further improvement. We don't need to review them any further.

Reviewer #2

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #3

(Remarks to the Author)
I am overall ok with the review and I don't have further comments

Reviewer #4

(Remarks to the Author)
I have reviewed the authors' updates to the manuscript, and commend them for considerable improvements, especially to the readability of some figures that were previously difficult to distinguish, and for the inclusion of important clarifications in the text.

However, I do still have one major concern, which again relates to the way that sea-level projections from AR6 are being incorporated.

In response to my original comment detailing concerns with the authors' approach, the authors said that "We have plotted the percentiles provided by the AR6 projections and those associated with our approximated mean and standard deviation for all processes at select times and locations, and find that they fit well." They also provided a plot of both AR6 data for the ocean dynamics component of sea-level rise and their own analysis, assuming a normal distribution.

However, for the ocean dynamics component for the AR6 projections, a t-distribution with the mean/co-variance of a multi-model ensemble constructed from CMIP6 models was used. A normal distribution fit to the ocean dynamic component of the projections would therefore be expected to align very closely, with the greatest discrepancies being in the tail, owing to the relatively few CMIP6 models available (~20), for which the t-distribution accounts for with a degree-of-freedom term. (see Fox-Kemper et al., [Ref. 1 in the manuscript], Kopp et al., 2023, [Ref. 50 in the manuscript] Kopp et al., 2014 [https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2014EF000239]).

This does not mean that it is appropriate to assume a normal distribution for all components or for the full SLR projections, which are well-known to have long (and important) upper tails. The AR6 datasets that the authors use are provided as full distributions for a reason. The appropriate methodology would require working with the full distribution and framing the authors' results in terms of that distribution. The current assumption of a normal distribution for sea-level rise projections is inappropriate--particularly given that sea-level rise projections are well-known not to conform to a normal distribution.

Building upon my concerns about this methodology, I continue to be concerned about the validity of the uncertainty that the authors provide for their total estimate of RSL around Greenland for the 21st century. As noted above, the correct approach for using the AR6 projections is not to treat them as normal distributions, but to work with the full distribution. If an approach that uses the full distribution is adopted (as it should be), the analysis of uncertainty will also need to be adjusted, since, as noted in my original response, uncertainty across percentiles cannot just be added together (unless it is the median).

Version 2:

Reviewer comments:

Reviewer #4

(Remarks to the Author)
I appreciate and commend the effort the authors have put into revising their methodology for the sea-level rise projections. I have no further comments at this time, and would recommend that it move forward for publication in the present form.

~Andra Garner

We thank the reviewers for their detailed and positive feedback. We have now revised the manuscript to address the comments that have been raised. We describe these changes below by interspersing the original feedback in black with our response in blue. We note that all our line numbers refer to the revised manuscript. We also include a version of the manuscript that indicates all changes to easily track changes from the original manuscript.

Reviewer #1 and #2 Evaluations:

The manuscript presents projections of 21st-century sea level fall along the coast of Greenland. The authors use available ice mass change estimates from the Last Glacial Maximum to 2100, derived from various ice models and reconstructions, to compute relative sea level change using the GIA model SEAKON for different Earth structures. They analyze separately the contributions of GIA-related processes associated with past deglaciation, the Little Ice Age, and future ice mass loss. In addition they use to non-GIA-related processes from the most recent IPCC WG1 report. The ensemble mean results indicate that sea level change in 2100 is primarily driven by future ice mass loss and non-GIA-related contributions, with projected maximum sea level fall of 1 ± 0.3 m for RCP 2.6 and 2.8 ± 0.8 m for RCP 8.5.

General Remarks:
The study is conducted thoroughly, and the manuscript is generally well written.

However, the broader impact of the study is superficially addressed and remains somewhat unclear. While it is acknowledged that reliable sea level projections have significant implications for Greenlandic coastal communities, this aspect is only briefly mentioned and not elaborated upon in the manuscript.

The authors highlight the limitations of traditional approaches that consider only elastic deformation in projections. However, it is unclear whether they are referring to sea level projections based on GIA models or dynamical ice sheet models. Ice sheet models incorporate elastic and viscous deformation in different ways, often assuming a constant present-day rate. It is not clear in the manuscript how that compares to the results of this paper.

The results section assumed prior knowledge on linear, nonlinear and transient behavior of the Earth's interior. Furthermore, different rheologies are used on different timescales, which is not explained in the manuscript. The introduction should include an explanation of these different types of deformation and its relevance on different timescales. Too much GIA expertise is presumed for the audience of Nat. Comm.

The spatial figures present ensemble means, but the standard deviation is relatively large due to variations among ice sheet models. Given this variability, displaying only the ensemble mean may not be the most informative approach. It would be beneficial to also present the mean per ice sheet reconstruction, as this likely determines the main pattern of deformation. In figures such as Figure 3, the relative error appears spatially consistent. It would be more informative to include a range of relative errors in the text and provide additional figures as suggested above.

We thank the reviewer for their positive assessment and for their constructive feedback and summary of the main concerns here. Most of the points above are also reflected in the Line-by-Line Comments below and we therefore address them as they appear in the comments below. The only comment that is solely raised here relates to the variation in sea-level change that is driven by differences among ice-sheet models used in our projections (last paragraph). We have included an additional figure in the supplement that shows spatial projections of sea-level change at 2100 CE for each ice sheet modelling group used in this study (Supplementary Fig. 4).

<u>Line-by-Line Comments</u>
L12: Replace However by As a consequence

We agree that both "However" and "As a consequence" could be used here. However, we do believe that our use of "However" highlights the unexpectedness of projected sea-level fall in Greenland (considering the ice sheet is causing global sea level to rise) and therefore better fits the narrative that we are attempting to tell through this paper. We therefore decided to keep the original wording.

L14: Predictions is used throughout the manuscript where projections are meant, please change

We thank the reviewer for noticing this and have changed the wording from predictions to projections where relevant.

L20: It is completely unclear what is meant with traditional approaches. Some applications have GIA as a constant offset. Does traditional approaches refer to GIA to ice sheet models or to IPCC style sea level projections

We are referring to approaches in which sea-level projections driven by projected ice-mass change use an elastic Earth structure to calculate deformation (as is used, for example, in refs. 1, 62, 63). This statement has been removed from the abstract to adhere to the word limit, but we have added these references to our discussion on this point (lines 305-316).

L22: can these later two statements be more detailed. For a tenth of the change the sentence can still be made but the impact is negligible.

We agree that this sentence is brief and have added some details that allow us to still adhere to the word limit of the abstract (lines 14-16):

> "A fall in relative sea level has implications for Greenlandic communities whose economy, near-shore infrastructure, and food security are vulnerable to coastal changes, and for the dynamics of marine-terminating glaciers"

For more details on these two statements, we refer to our results and discussion section, where we have added text to better describe the implications of our study (lines 317-346).

L66-69: Provide the full range of upper mantle viscosity and lithospheric thickness values from references 5, 26, and 27 to give the reader a better overview of the uncertainty rather than highlighting a single result.

> We agree that including the preferred values of these parameters from relevant studies would enhance the reader's understanding of their uncertainty. We have added to the text (lines 65-72):
>
> > "Lecavalier et al. [5] find that GIA models forced by a GrIS deglaciation history (termed Huy3) derived from a dynamical ice sheet model best fit RSL data in Greenland when using a 120 km thick elastic lithosphere and upper and lower mantle viscosities of 5×10^{20} Pa s and 2×10^{21} Pa s, respectively. However, these values are not consistent across all studies that tune their model parameters to fit Greenland RSL data [23, 24]. Tarasov & Peltier [23] find that their GrIS history performs well when coupled with a lithosphere of 90 km. Fleming & Lambeck [24] also shows preference for a thinner lithosphere (80 km), in addition to higher values for the viscosities of the upper mantle (4×10^{21} Pa s) and lower mantle (10×10^{21} Pa s).

L72: The reference to an AGU abstract (ref 29) appears redundant, given the availability of peer-reviewed sources supporting this statement.

> We appreciate the reviewer pointing this out. This was an oversight on our part and the reference has been removed from the revised manuscript.

L71-74: Reference 33 states that lateral structure does not explain misfits to RSL data. Could you clarify how this compares to refs 5, 8, and 23? It is mentioned that the fit improves but it is not clear whether misfits remain after including lateral variations? If ref 33 also includes lateral variations, consider citing it together with refs 5, 8, and 23.

> We realized that this portion of the text was unclear and have added further explanation. References 5 and 8 found that using different 1D Earth parameters for the East and West coasts of Greenland improved their ability to fit RSL data. Ref 33 (now 32) followed on these studies by running a GIA model with a 3D varying Earth structure and found that it was unable to explain all of the misfits to the RSL data that were pointed out in ref 5. We removed former ref 23 from this portion of the text as it noted how a laterally varying Earth structure could improve model fit to GPS data, and in this section we are talking about RSL data. The revised text now reads (lines 72-78):
>
> > "A difference in the optimal 1D Earth parameters inferred for the East and West coasts of Greenland suggest that improvements in the misfit between modeled predictions and RSL data could be achieved by considering a more complex, laterally-varying (i.e., 3D) Earth structure [5, 8]. The influence of a 3D Earth structure on GIA predictions is shown to be significant in numerous studies focusing on various regions around the world [e.g. 28–32]. In Greenland, Holocene RSL predicted by 3D GIA models can differ by tens of meters when

compared to the 1D case, however it does not necessarily explain all previously-indicated misfits to the RSL data [32]."

L74-75: The phrase "observations beyond paleo RSL data" is somewhat vague. Could you specify what is meant here?

We have changed the sentence to more explicitly describe what we intended (lines 78-79). We use this sentence as a segway into the following paragraph, where we describe contemporary observations of GIA in more detail:

"Incorporating observations of present-day deformation can be useful to further constrain values for the properties of Earth's interior."

L79: Define the time frame for "since the last deglaciation" and "contemporarily." The period from the Last Glacial Maximum to the start of the Little Ice Age (~1000 CE) is defined in L114-115 but should be introduced earlier to improve readability. Could the authors clarify the exact time period they are referring to?

We agree that being specific about what is meant by these two time frames would be helpful for the reader and have reworded it for clarity (lines 81-84):

"Present-day VLM recorded at the 57 permanent GNSS stations that span coastal Greenland, called GNET, is caused by deformation driven by ice changes that have occurred in the past (most importantly over the last deglaciation, i.e. last 26 kyr) and that are occurring today (i.e. during the recorded period)[33]."

L92: Do you mean during or since the LIA?

We mean during and since the LIA but agree that this a little confusing. We have reworded this to "since the beginning of the LIA"(line 97).

L94: A non-GIA person will not understand why mantle viscosity would be smaller since the LIA, as it is suggested to be a material property. Please rephrase to make this clear for non-GIA specialists.

We agree that our original explanation of transient rheology was lacking and could be further elaborated. We have added to the original paragraph to make the explanation of transient rheology understandable for the broader audience (lines 100-109):

"They suggest that this lowering in mantle viscosity for ice-sheet changes since the beginning of the LIA hints at the presence of transient deformation beneath Greenland. Transient deformation is an extension of the classical Maxwell rheology. While the latter consists only of an instantaneous elastic response and a steady-state viscous creep, transient deformation, as is, for example, captured by a Burgers rheology, allows for additional dissipative mechanisms which act to lower the apparent viscosity over short periods. This deformation style is supported by high-temperature high-pressure laboratory experiments of rock deformation [36, 37] and has been explored in other GIA studies [e.g. 38-42]. In the case of loading events that occur over centennial timescales (like the LIA), the viscosity of the mantle can effectively be lower than that inferred for longer-timescale events (like the LGM or mantle convection), allowing the mantle to deform more quickly and with a greater magnitude."

L97: Is ref 39 the only reference discussing higher-order linear viscoelasticity and are refs 40 and 41 the only references to related to non-linear rheology? Please clarify the distinction.

We decided to rephrase this sentence in response to the comment above and now simply refer to past studies that have explored transient rheology in GIA modeling (line 106).

L102-103: The term "spatially extensive asthenosphere" is subjective. In Antarctica, an order of magnitude difference in mantle viscosity significantly affects the response, and similar variations can be expected between east and west Greenland. Please provide further clarification.

We agree with the reviewer that our wording made the point we were trying to make ambiguous. In Pan et al. (2024), they insert a low-viscosity layer beneath a 75 km thick lithosphere and on top of an upper mantle of viscosity 5×10^{20} Pa s. They show that including this low-viscosity channel allows them to fit both present-day VLM and paleo RSL data in Greenland. This study shows that transient rheology is not *required* to reconcile these datasets. However, seismic tomography studies (cited in line 115) have shown high velocity anomalies under portions of the lithosphere in Greenland, highlighting old cratonic regions, and contradicting the existence of a homogenous sub-lithosphere low-viscosity layer at one depth. The authors have changed our terminology to make our point more clear (lines 112-115):

"However, Pan et al. [44] caution that a sub-lithospheric low-viscosity layer with a regular Maxwell rheology would also be able to explain both RSL data and present-day VLM if the LIA ice history is included in the GIA model. Further work is needed to understand the possible extent of a sub-lithospheric low-viscosity layer around and below the cratonic keel of Greenland [45, 46].

L120: Between 1000 CE and 2100, VLM observations are corrected for coeval (and deglacial) ice mass change. However, coeval ice mass change is included in the GPS signal. Why is it corrected for separately?

Here we consider the GPS signal to be a result of viscoelastic deformation in response to ice mass change over three distinct time periods: the deglacial (30-1ka), the LIA (1ka to the beginning of the GPS record), and the duration of the GPS record. We predict the second of these contributions and want to compare it to the observation. We therefore need to remove the first and third contribution (deglacial and coeval) from the observation. In response to the reviewers comment on L154-164 we have expanded on this here.

L131-136: Ref 44 is not mentioned in the introduction but should be placed in context in the introduction to improve readability. Refs 8, 33, and 44 are not discussed further, but ref 5 is—why? Additionally, why are refs 15 and 45 not mentioned alongside refs 5, 8, 33, and 44 in L132?

> We agree that Ref 44 (now ref 33) is an important study that should be included in the introduction and we have added it to line 84. We realized we cited the wrong study from Simpson et al. (ref 8 in original manuscript) by accident. We have now cited the correct study in the revised manuscript (ref 26). Reference 5 is included in the subsequent interpretation of our present-day VLM caused by the deglaciation to support the statements made about the ice history, as this reference provides a detailed chronology of ice extent for Greenland from the LGM. Ref 15 isn't mentioned in Line 155 because it uses deglacial uplift rates produced by ref 32 (former reference 33), so including it would be redundant. Ref 45 (now reference 53) is not included in Line 155 as it does not produce predictions of Greenland-wide uplift rates for us to compare our results to. We generalized text in line 157 by changing "6-ka" to "mid-Holocene" so that our statement would be consistent with all the deglacial ice histories that we cite.

L143: Add a brief explanation of what "normalized weights" mean at the start of the paragraph.

> We have changed the sentence to clarify this (lines 166-168):
>
> > "The weights for the deglacial GIA models (normalized so that they sum to one) fall within the range of 0.0081 to 0.0227, and do not deviate substantially from the value that would be assigned if all models were weighted equally (1/65 = 0.0154)."

L143-152: Since ice history is a key factor, please consider adding a supplementary figure showing average ice mass change from ICE6G, Huy3, and other reconstructions/models. Include a brief description of these reconstructions.

> We thank the reviewer for this suggestion and do agree that the ice history is a key factor when modelling GIA. However, we consider a more detailed description of the deglacial ice histories to be beyond the scope of this (already long) paper since it is easily findable in the cited literature.

L154-164: The method for removing elastic and GIA signals from observations is not entirely clear. Please include a brief explanation in the introduction of this section. Additionally, a short explanation of the Bayesian approach in L120 would help in understanding Figure 2.

> We have added details to the introduction of this section in order to make these methods more clear (lines 130-140):
>
> > "For (2), we use a new suite of 14,040 1D Maxwell GIA simulations. We first predict present-day VLM in Greenland driven by the LIA and compare these to observations of uplift measured at 55 GNET stations. In order to make a direct

comparison, we correct the observed VLM for uplift associated with deglacial and coeval mass changes, the former of which is calculated in step (1) and the latter is taken from Berg et al. [19]. We further account for potential differences between the reference frame used in the 1D GIA model and that used for the VLM observations. We perform a Bayesian inversion to identify the distribution of most likely ice and Earth parameters varied in the 1D GIA model and create a posterior prediction of present-day VLM driven by the LIA in Greenland. We then use these optimal parameters to create a suite of 1D Maxwell GIA runs forced by LIA-to-2100 CE ice-mass change, using an ensemble of projections of Greenland and Antarctic ice change for both Representative Concentration Pathway (RCP) 2.6 and RCP 8.5 [47, 48]."

Ad Figure 2: very difficult to see a difference between prior and posterior. Can you plot a panel a the prior and as panel b the posterior-prior or maybe in one plot observed-prior and observed-posterior. Alternatively, We suggest to show all three components (observations-ELA-GIA, Prior LIA and Posterior LIA) in one figure using overlapping boxplots in three different colors instead of shaded areas.

We agree that it is hard to see the difference between the prior and posterior solutions in this figure and thank the reviewer for their suggestions on ways in which this figure could be improved. We have replotted this figure so that the observations and predictions for both the posterior and prior are all in one figure (Figure 2). We have also added to the text to describe why the posterior and prior means look similar (lines 184-186) :

"There is only a slight difference in mean between our prior and posterior solutions, as the model parameters used in this study (Table 1) were sampled from the posterior distribution of parameters from Adhikari et al. [15]."

Ad Figure 3: How large is the relative standard deviation? 15%? seems an insightful quantitative value. It also looks like there is barely a spatial pattern in the standard deviation it is more or less a relative error. So what is the added value of panel b, maybe you can leave it out and just mention the relative error.

The average relative error taken across GNET stations is 27%. We have chosen to leave the original figure as is, but have included this number in the text (line 191-192):

"The standard deviation of the posterior solution across GNET stations is 20-30% of the posterior mean."

L171: It is not clear whether the weighted average LIA vertical land motion of all simulations is similar to the average of Adhikari. Could you clarify this?

We now clarify in line 196 that we refer to the posterior (weighted) LIA vertical land motion.

L180-184: The finding of low upper mantle viscosity does not imply that the entire upper mantle has this viscosity, as a 1D model is used. Rather, it suggests that the regions where the mantle is sensitive exhibit this viscosity, while other areas (insensitive to the ice load) may have different viscosities. Could you clarify how this leads to an indication of transient viscosity approaching steady state? Why would the mantle viscosity be smaller since the LIA?

> The reviewer is correct that our findings do not indicate that the entire upper mantle has this viscosity and we changed the sentence to reflect that we are only referring to the mantle below Greenland. Inferring a lower viscosity for ice changes since the LIA compared to the estimates of viscosity inferred in Greenland over the deglaciation (e.g. 5×10^{20} Pa s, ref 5) supports the idea of transient rheology, as it suggests that the frequency of loading (i.e. how quickly ice grows/melts) can impact the apparent viscosity of the mantle's response to that loading. We say that the viscosity would approach that of the steady-state viscosity on deglacial timescales because a higher viscosity is expected on longer timescales within the transient regime of deformation and a higher viscosity for the deglaciation has been inferred from previous studies (see lines 65-72). However, since our study does not explicitly infer deglacial-timescale viscosity, we removed this part of the sentence. We also added an explanation to the introduction (lines 100-109) that explains transient rheology better (and why the mantle viscosity would be smaller over the LIA). Lastly we changed the wording in the sentence that is being referred to here that our viscosity inference **may** be indicative of transient deformation (rather than **is**) given the results by Pan et al., (2023).

L194 Do you intend to say effect the results only marginally?

> The authors were intending to say that both the data and the inference methods that we use lead to the minor differences in results between this study and Adhikari et al. (2021) (ref 15). We have changed "affect" to "leads to" in line 220 to make our meaning clearer.

L198 Repeat that this is the forebulge effect of North America, if that is what explains it.

> We have revised this sentence for clarify (lines 225-228):
>
> > "The deglacial contribution is the smallest component but causes a slight rise (< 0.21 m $\pm$ 0.05 m) in RSL in west and southeast Greenland due mainly to Laurentide forebulge collapse and a fall in RSL (~ -0.1 m $\pm$ 0.09 m) in the northeast (Fig. 4a) caused by ongoing uplift in response to ice loss since the LGM."

L204 Unclear from where you took the Antarctic values.

> We now summarize in the beginning of the results and discussion section where the Antarctic contribution comes from (lines 137-140). It is also further described in the methods section (lines 537-541).

L207: Could you specify which gravitational effects are included—self-gravitation of ice on relative sea level, self-gravitation of the Earth on deformation, or both?

> It's both. We now describe this explicitely lines 235-237):
>
> > "Gravitational effects include both the ice and the solid Earth's pull on the ocean's surface and a uniform gravitational shift caused by the input of meltwater into the ocean."

L208: You refer often to Table S2, would it be possible to absorb it in the main text?

> Table S2 has been absorbed in the main text as Table 1.

L211: Please include the spatial distribution of gravitational effects on sea level in the supplementary materials. The range of ~20-30%—what does this depend on? Was only one simulation conducted? How much does this effect amount to in meters? Please also show the spatial distribution in the supplementary material.

> The range depends on location and RCP scenario. We only used one set of LIA ice history and Earth structure parameters in our GIA model, but ran the model to 2100 for RCP 2.6 and RCP 8.5 using the ensemble of projections of ice change from the 8 ice sheet modeling groups used in this study. We have clarified this in lines 237-243:
>
> > "We calculate this by running the GIA code from the LIA to 2100 CE using the ensemble of ice sheet projections for RCP 2.6 and RCP 8.5 with one parameter combination (Table 1) that performs well in fitting observations, and then separating the two components. We find that RSL at 2100 CE caused by ice-mass changes from the LIA to 2100 CE is dominantly driven by solid Earth deformation, with sea-level changes caused by gravitational effects constituting ~ 0-20% of the total for RCP 2.6 and ~ 10-30% of the total for RCP 8.5, depending on location around the coast (Supplementary Figure 3)."
>
> We also included a spatial plot of the percentage of RSL change at 2100 driven by LIA-to-2100 CE ice-sheet mass change that is made up from gravitational effects in the supplementary material (Supplementary Figure 3). This effect amounts to a maximum contribution of approximately -0.2 m for RCP 2.6 and approximately -0.7 m for RCP 8.5, which is represented by the percentage range described in the text.

L212-218: Are these results derived from literature rather than modelled? If so, please add references.

> We thank the reviewers for pointing this out. The relevant references have been added to line 245.

L240: So separating does not add much. I think you can describe one and argue that qualitatively the other locations are yielding comparable results.

We thank the reviewer for this suggestion. We have described projected sea-level change for Tasilaaq and Nuuk and have moved the analyses for the other towns/settlements to the supplement.

Figure 4: unclear what the white areas in panel i to l are (same in Figure 5)

Projections for the thermal expansion, ocean dynamics, global glacier change, and land water storage contributions to RSL do not exist within these areas, so we have excluded the regions from our projections at 2100 CE. An explanation of this has been added to the figure captions for figures 4 and 5.

L260: I am not sure whether that is noteworthy as this holds for most points along the world coastline.

This is a great point that was also brought to our attention by another reviewer. We have decided to keep this sentence in the text, but have added references for other studies that have made a similar finding (lines 279-281):

> "For all locations considered here, it is noteworthy that the projections for the two future emissions scenarios (RCP 2.6 vs. 8.5) only start to diverge around 2050 CE, which is consistent with prior work on sea-level projections globally [e.g. 1, 59, 60] (Supplementary Figure 5)."

Ad Figure 6: try to homogenize the lay-out of the figure. Unclear what the figure adds. I believe it can be left out.

We have moved this figure to the supplement and have attempted to homogenize it better.

Ad Figure 7: What is the added value of a second location I think only Nuuk is sufficient? Explain the red curve in the caption as well.

The break-down of the different factors that contribute to local sea-level projections will differ from place to place. Especially the contribution from ice change since the beginning of the LIA can vary significantly. We believe that showing the break-down for two locations highlights this.

L294: Not sure whether this is true. It assumes the marine terminating glaciers dynamics are controlled by water depth. I think that is questionable, at least make clear this is what you assume and provide reference for this assumption.

Though not a primary control of marine-terminating glacier dynamics, we do assume that the impact of GIA on bed topography and water depth at the grounding line modulates glacier behavior, as illustrated in several papers [refs 65, 66]. We have changed the text to explicitly state how a RSL fall of the projected magnitude could impact the glacier (by changing the position of the grounding line) and have added additional text and references to support this (line 317-326):

> "The fall in RSL by up to 2.8 m for RCP 8.5 that we project to occur along Greenland's coasts by the end of this century could act to slow down the pace of grounding-line retreat for Greenland's marine-terminating glaciers, as the position of the grounding line depends on sea level at the grounding line [64-66]. Ice discharge from marine-terminating glaciers was responsible for approximately two-thirds of Greenland's total contribution to global sea level over the period 1972–2018 CE [67, 68]. The response of the solid Earth and gravity field to this ice-mass loss can raise the bed topography and decrease RSL at the grounding line, which can lead to its stabilization and potential re-advance [65, 66]. This effect may be particularly impactful for large marine-terminating glaciers that rest on bedrock that deepens inland (like Kangerlussuaq and Sermeq Kujalleq [69]) and are vulnerable to marine ice sheet instability feedbacks, where the flux of ice across the grounding line increases as it retreats [64, 70-74]."

L305: Adhikari et al. studied Antarctica and the response of the Antarctic Ice Sheet and its ice shelves cannot be directly compared to that of the Greenland Ice Sheet and its outlet glaciers due to fundamental differences in their physical processes and environmental conditions. Please include this nuance in the manuscript.

We thank the reviewer for pointing this out and agree. We have included this nuance in our manuscript and use it to motivate the need for a Greenland specific coupled GIA-ice sheet modeling study (lines 328-333):

> "This amount of uplift is of similar magnitude to that projected to occur along Greenland's coasts over a similar time frame, however a direct comparison between the dynamics of Greenland and Antarctica's marine-terminating glaciers cannot be made. This limitation, along with the potential for the GIA-induced stabilization of these marine-terminating glaciers to reduce Greenland's contribution to global sea-level rise, motivates the need for a detailed analysis that couples GIA and ice-sheet models on centennial timescales in Greenland."

L307 can you explain this in more detail. Why has a ~ meter an impact? in view of the hazards and threats communities are exposed to?

The authors agree with the reviewer that the discussion of the impact of projected sea-level fall on Greenlandic communities can be expanded. We have added to the paragraph to reflect this (lines 335-344):

> "RSL fall on the order of multiple meters has the potential to increase navigational hazards and limit the accessibility of harbors to deep draft boats by reducing the water depth beneath their keels. Marine channels that are already shallow today are vulnerable to becoming exposed at low tide, disconnecting

navigational routes and waterflow, which may impact marine habitats. Port infrastructure, such as docks and mooring sites, could become less useful as the falling sea surface would make access to them more difficult or even impossible. A reliance on global sea-level projections has led to the perspective among many residents that sea level will rise in Greenland due to climate change [75], which would create a very different set of hazards than those presented above. The regional RSL projections provided in this study can be used by communities to assess their site-specific risks to sea-level fall and prepare adaptation strategies accordingly."

L311: In the context of publishing the manuscript in Nature Communications, it would be valuable to elaborate on the interdisciplinary aspects of the study. Could you clarify what interdisciplinary studies are meant?

We now specify what we mean by interdisciplinary studies in this case (lines 344-346):

"Subsequent interdisciplinary studies that consider these projections with regard to the national economy, coastal structure, and marine ecology are needed to better understand and communicate the impact that RSL fall could have on these communities."

Ad Figure 8: If understood correctly I think the text discusses the relative contribution for the viscous part why not show that or show both?

We have changed Figure 8 panel d to show the viscous percentage of RSL at 2100 CE.

L312: clarify that you prescribe ice sheet history and what that implies. a. Not a consistent glaciological model, and b not a coupled solution with a glaciological model.

We added 2 sentences (lines 367-370) to clarify this:

"While the Greenland and North American portions of Huy3 were created using glaciological models that account for ice dynamics, ICE6G, ANU, and the other global ice masses in Huy3 were not. Further, none of the reconstructions used here were created using a coupled GIA-ice sheet model."

L334: Confusing phrasing. it is the same model but 65 different members with different physical component or parameter settings.

We agree that this phrasing is confusing. We have revised it to (line 373-374):

"We use output from the suite of 65 GIA model runs described above to calculate the contribution of the deglaciation to present-day VLM and future sea-level change."

L335: What is meant by the latter I presume the present-day VLM; unclear what that precisely is. The present-day rate or the VLM caused by the future melt?

We have revised this (line 374) to explicitly say ‘Present-day VLM’ rather than ‘The latter’.

L349: observed RSL at location y add the last bit.

The authors thank the reviewer for catching this small detail. We have decided to remove the superscript y, as it was added in to suggest we were calculating the residual between the observed elevation of RSL and the predicted. However, we have realized that since we aren’t calculating any other residual, it is unnecessary to specify this within the equation, as it is clarified in the text.

Line 354: can you explain in words what the second line in equation 1 secures. I guess it has to do with the limiting aspect but unclear how that works.

The reviewer is correct that the second line in the equation treats the limiting data points. For each RSL data point, d_i, there exists a model prediction of RSL at that location and time, m_i. We wanted to define a misfit function, $\rho_i\left(d_i, m_i\right)$, so that the total error, E , for one model run is equal to the sum of the misfits between that model run’s predictions of RSL and the data at every location/time. Following Menke, 2018 (Geophysical Data Analysis, Eq. 9.6), this can be achieved by relating the misfit to the conditional probability of the data given the model prediction:

$$\rho_i = -2 ln p(d_i|m_i)$$

So that:

$$E = -2\sum_i ln p(d_i|m_i)$$

If we take the terrestrial limiting case as an example, we would want $p\left(m_i\right) = 0$ when $d_i \ll m_i$ (the model predicts that RSL is higher than that inferred from the limiting data point , which is inconsistent) and $p\left(m_i\right) = 1$ when $d_i \gg m_i$ (the model predicts RSL lower than that of the limiting data point, which is consistent). This is consistent with a Heaveside function, with the step situated at the model RSL prediction, m_i. If we assume that the data points are represented by normal distributions, when $d_i \approx m_i$, we want $p\left(m_i\right)$ to be proportional to the amount of the data’s normal probability distribution that is situated to the right of this step. This leads us to the following equation for $\rho_i\left(d_i, m_i\right)$:

$$\rho_i\left(d_i, m_i\right) = \frac{1}{2} + \frac{1}{2} erf(q_n \frac{\left[d_i - m_i\right]}{\sigma_i})$$

For the terrestrial limiting case, $q_n = 1$:

- If $d_i \gg m_i$, $d_i - m_i$ is large and positive, so $\left(q_n \frac{[d_i - m_i]}{\sigma_i}\right) = 1$ and $\rho_i(d_i, m_i) = 1$
- If $d_i \ll m_i$, $d_i - m_i$ is large and negative, so $\left(q_n \frac{[d_i - m_i]}{\sigma_i}\right) = -1$ and $\rho_i(d_i, m_i) = 0$
- If $d_i \approx m_i$, $d_i - m_i$ is small and $\left(q_n \frac{[d_i - m_i]}{\sigma_i}\right)$ would lie somewhere between 0 and 1 (the closer together that d_i and m_i are to each other, the smaller the value of the error function). The probability then becomes dependent on the proportion of the data's normal distribution lies to the left or right of m_i. For example, if the mean of the normal distribution is equal to m_i, there is an equal proportion of the normal distribution to the left and right of m_i and the probability would be equal to ½.

We believe that this is too much detail to include in the text but now point the interested reader to reference 83 (and references within) for a detailed explanation of this equation (line 390-392).

Line 378: but that those not exclude that lateral variation in the structure are important. You have to comment on this despite you use a 1D model to provide context on the limitations of your 1D set-up

We thank the reviewer for pointing out this missing detail. We have added text and a reference to describe our reasoning for excluding both lateral and radial variations in viscosity in our LIA GIA models (lines 424-427):

> "We assume a laterally and radially homogeneous viscosity structure because the inferred mantle viscosity over centennial timescales varies minimally for the upper mantle below Greenland [43] and it is unlikely for small-scale load changes, like those experienced in Greenland since the beginning of the LIA, to invoke deformation within the lower mantle [44]."

In addition to justifying our model choice we have also added the caveat that 3D variations may be relevant (lines 427-429):

> "However, we also acknowledge that omitting 3D variations in Earth structure, especially those driven by lateral variations in lithospheric thickness, may have some impact on our results."

L395: why is that not affecting the entire south(western) part?

We believe that the reviewer meant to ask why the plume isn't affecting the entire southeastern portion of Greenland. Published estimates of the hotspot track through Greenland have the northwest crossing over the plume at approximately 80 Ma and then central east Greenland passing over it at around 40 Ma (see Fig 1 in ref 25 and Fig 2 in ref 86). The two stations in central East Greenland that we removed have anomalously high uplift rates, suggesting that the mantle below them is still weak from passing over the hotspot, a process that we do not account for in our GIA models (which is why we remove them from our analysis). The weakening of mantle structure driven by the hotspot is thought to be relatively narrow (ref. 87), so we do not believe the entire southeastern part of Greenland to be affected.

L422: I would expect V_RF to be independent of time, but you suggest you use each of the 14,040 predictions?

It is correct that the correction is independent of time. V_RF is the contribution to VLM driven by differences between the reference frame of the GIA model (center of mass of the solid Earth reference frame) and that of the observations. Because each of the 14,040 GIA simulations that we use distributes mass within the solid Earth slightly differently (depending on Earth structure and ice history parameters), the center of mass of the solid Earth that the prediction is in reference to will be slightly different between each of the model runs, and thus we must make this correction for each.

We thank the reviewers again for this very detailed and constructive review. Our appreciation is reflected in the acknowledgements.

References:
Menke, W. (2018). *Geophysical data analysis: Discrete inverse theory*. Academic press.

Reviewer #3 Evaluations:

This manuscript aims at assessing the relative sea level along the coast of Greenland in the 21st century in response glacial isostatic adjustment and past and future ice-mass changes. My area of work is statistical methodology, and I am no expert in glaciology, so I will narrow my comments on the technical aspects of the methods.

The methods section is mostly a description of very basic approaches such as weighted sum of squares, decomposition of the uplift in each station into a sum of different terms and linear regression, so there is not much to comment on, I assume the choice of models was thorough and that the choice of these methods is based on well-established literature.
The interesting part is the Bayesian framework. This part is written unusually well for an applied work, so I commend the authors for their effort. They propose a hierarchical Bayesian model for the GNSS observations, where the true VLM rates are a combination of 14,040 GIA model solutions.

> We appreciate the favorable assessment and constructive criticism. The reviewer will find responses to all their comments below.

Comments:

No validation. The model makes many assumptions, but none are validated. For example, is x at least approximately Gaussian? If one were to fit, say, a skew-Normal or some other members of the exponential family, how would the results change? What about the exponential covariance function in (7), would it change if we had a Matérn covariance? Such assessment should have been done and their predictive skills measures with some cross-validation.

> We alluded to validation in lines 496-499 of the original manuscript where we said:
>
> > "We also computed standard model diagnostics [80] to interrogate the solutions, for example, scrutinizing model-data residuals to demonstrate the appropriateness of the model given the observations and demonstrating that solutions have converged."

We acknowledge that this superficial level of detail was inadequate. Therefore, to address the reviewer's comment, we've added more detail in Supplementary Section 3. In particular, we've added—

(a.) A histogram making the case that the data x are approximately Gaussian (Supplementary Fig. 6)
(b.) A residual analysis that supports our argument that the model is justifiable given the data (Supplementary Fig. 7)
(c.) A convergence analysis demonstrating that solutions have converged (Supplementary Fig. 8)
(d.) A leave-one-out cross-validation exercise that quantifies prediction errors and coverage rates (Supplementary Fig. 9)

Please see Supplementary Section 3 for an extended discussion and Supplementary Figs 6-9.

It's a good question whether choosing a different distributional shape in the data-level equation or form of covariance function in the process-level equation would substantially alter the results. We've argued that our model is appropriate given the data (see above), but it would require substantial modifications to the Bayesian model structure and code to definitively determine whether an alternative model structure, such as suggested by the reviewer, would materially change the results. Since Bayesian modeling isn't the central focus of the paper, performing such substantial modifications goes beyond our scope. However, to acknowledge the reviewer's comments, we've added to Supplementary Section 3 suggesting that a more in-depth study be undertaken in the future.

Lack of a continuous spatial process. While the model proposed is able to explain through the vector c which model better explains the data, it does not assume any continuous spatial process. I am assuming the models have been interpolated on the same grid as the data to avoid this issue. This, however, begs the question: to what extent is the interpolation impacting the results? A much better solution would have been to provide a spatial map of a Multinomial distribution.

The GIA process features complex, nonstationary, and anisotropic spatial structure. Thus, rather than use a continuous spatial process based on a simplified probability distribution, we chose to model GIA as a linear combination of geophysical model predictions spanning the uncertainty space of model parameters (mantle viscosity, lithospheric thickness, etc.) to more faithfully capture the scales and magnitudes of the process. While we spatially interpolated from the geophysical model predictions to the locations of the GNSS data, this isn't a major source of uncertainty, since the GIA process varies over typical spatial scales of hundreds of km, which is much larger than the model's spatial resolution (spherical harmonic degree and order 512 which equates to a spatial resolution of ~40 km). Therefore, no changes to the manuscript were needed on this point.

Lack of details on software and hardware. There is a wide variety of software able to perform Gibbs sampling and MCMC (e.g., STAN, JAGS, INLA, etc.). Which one has been used here and why? What computational resources have been used? What was the computational time?

We derived the conditional posterior distributions and wrote our own code for the Gibbs sampler. We used Metropolis sampling for any parameters with non-standard conditional posterior distributions. The code was written in MATLAB, uploaded to GitHub (see below), and takes ~20 minutes to run on a 2023 MacBook Pro (Apple M2 Pro Chip; 32 GB Memory). We have included this in the revised text (line 531-533):

> "The Bayesian model code was written and executed using the MATLAB numeric computing platform and takes ~20 minutes to run on a 2023 MacBook Pro (Apple M2 Pro Chip; 32 GB Memory)."

Lack of reproducibility. As of now, this work is not reproducible. No code has been provided, so I am not in the position of commenting on it. Given the high standing of the journal, I would have expected, at the very least, a detailed commented code on a GitHub repository for all methods used. This is in my opinion a considerable shortcoming for this submission.

> We've uploaded the code used to run the model and make the results onto our GitHub site (https://github.com/christopherpiecuch/bayesLIA) and onto Zenodo (https://zenodo.org/records/16780315)

Reviewer #4 Evaluations:

In this work, the authors present updated projections of 21st century sea-level change for Greenland, including updated VLM estimates generated by combining GIA models and VLM observations from permanent Global Navigation Satellite Systems (GNSS). My expertise is in sea level projections more broadly, and not in the specifics of GIA modeling/observations. Therefore, I have focused most of my comments below on the authors' generation of complete sea-level change projections, rather than the new VLM estimates. The manuscript is generally well-written, and most figures are clear. However, while I think this manuscript has the potential to be a meaningful contribution to the sea level and GIA communities, I also have some concerns about the methods the authors are currently using to generate complete sea-level change projections. If the authors can update their approach and the explanation of their methods, I would look forward to revisiting the revised version of the study. My detailed comments are outlined below.

> We thank the reviewer for this assessment and have addressed the comments and concerns about our methods below.

Major Comments:

Lines 523-526 say, "In addition to sea-level changes caused by GIA, other processes will contribute to the RSL experienced by coastal Greenland over this century. These processes include changes in global glaciers, sterodynamic changes, and changes in terrestrial water storage, excluding glaciers and ice sheets. Note that the contribution from Antarctica is already included in the previous section."

> If Antarctic ice loss is already included in the LIA to 2100 Ice Mass change, the first sentence of this section (and much of the results) should be amended to reflect this. Because, in this case, this section is not "Non-GIA related contribution to future sea-level change", but "Contributions to future sea-level change excluding GIA and Ice Sheet Mass Loss". This fact was not clear to me until I reached line 526 of the manuscript.
>
> > We agree with the reviewer that using "non-GIA related processes" is ambiguous and misleading. We have taken the reviewer's suggestion and changed our wording to "contributions to future sea-level change excluding ice-sheet mass change and associated GIA". This change has been propagated throughout the manuscript.

Lines 526-528 state, "Estimates for the contributions of each of these processes to RSL in Greenland to 2100 CE for RCP 2.6 and RCP 8.5 were obtained from the IPCC AR6."

> This is very confusing, since the IPCC AR6 did not make any explicit sea-level change projections for RCP scenarios, but instead focused on SSP scenarios.
>
> > We thank the reviewer for correcting this mistake on our part. We have clarified in the text that we obtain projections for these processes for SSP1-2.6 and SSP5-8.5.

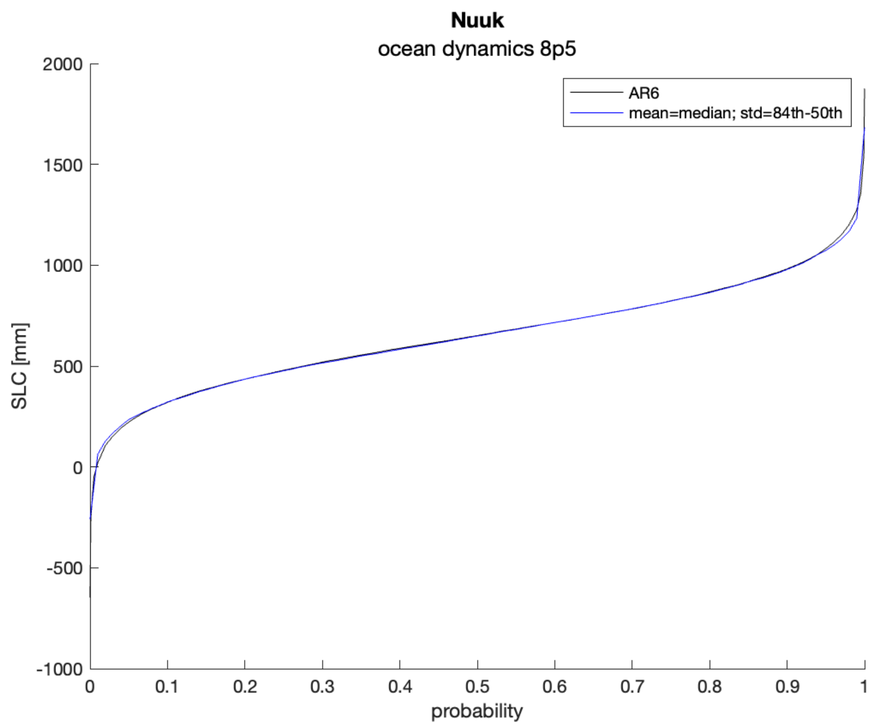
Furthermore, to my knowledge, the IPCC AR6 did not provide RSL projections for Greenland in the full report—though the datasets made available for the IPCC AR6 projections (which should include additional citations beyond AR6 Chapter 9) did. So, this description leaves me with a lot of questions about where the contributions for each of these "non-GIA" processes actually came from.

> We obtained the projections for these processes from the "IPCC AR6 Relative Sea-Level Projections without Background Component" dataset. We profusely apologize for our oversight in including the proper references for this dataset and that mistake has been fixed in the revised version. We also clarified in the text that we obtained the projections from this dataset (lines 560-564):
>
> > "Estimates for the RSL contribution of each of these processes in Greenland to 2100 CE for SSP1-2.6 (Shared Socioeconomic Pathway 1 paired with an approximate radiative forcing of 2.6 W/m^2 by 2100) and SSP5-8.5 (Shared Socioeconomic Pathway 5 paired with an approximate radiative forcing of 8.5 W/m^2 by 2100) were obtained from the dataset of RSL projections associated with the IPCC AR6 that excludes the signal driven by long-term vertical land movement [1, 50-52]."

Lines 528-529 say that, "The mean of these estimates was made relative to 2017 CE and linearly interpolated at 5 year intervals between 2020 and 2100 CE."

What does this mean for AR6 projections that are provided in terms of percentiles (median and likely range)? How did the authors construct a "mean" from these projections that are provided as a distribution?

> The reviewer is correct that the projections for these processes were provided in terms of percentiles. We approximate the mean and standard deviation from this by assuming that the data is normally distributed and taking the mean as the median and the standard deviation as the difference between the 84th and 50th percentiles. We have plotted the percentiles provided by the AR6 projections and those associated with our approximated mean and standard deviation for all processes at select times and locations, and find that they fit well. We have included a figure below that illustrates this for the projection of the ocean dynamic contribution to RSL at 2100 CE in Nuuk. A description of this method has been included in the text (lines 566-567):
>
> > "We make the assumption that this data is normally distributed and equate the mean and the median and define the standard deviation as the difference between the 84th and 50th percentiles."



In addition, the IPCC AR6 projections are provided for a 1995-2014 baseline--so how were the authors' estimates made relative to 2017?

> We linearly extrapolated the mean and standard deviations of projected RSL for each driver and SSP scenario to 2017. We then subtracted this extrapolated mean value from the mean of RSL at timesteps from 2020-2100 and combine the uncertainty by taking the square root of the summed variances.

Furthermore, when the authors say that these estimates were linearly interpolated at 5 year intervals between 2020 and 2100, does that mean that the first interpolation was actually 3 years (going from 2017-2020), or should the text say that the estimates were linearly interpolated at 5-year intervals from 2022-2097?

> We agree that this wasn't clearly stated. The projections originally were at 10 year time steps from 2020-2100. We linearly interpolated them at 5 year time steps beginning in 2020 and extending out to 2100, making that first time step 3 years (from 2017-2020). We have adjusted the text to make this more clear (lines 569-570):
>
> > "We then linearly interpolated the projections at 5-year intervals beginning in 2020 CE and extending to 2100 CE."

Lines 529-531 say that, "For our total estimate of RSL around Greenland for the 21st century, we sum GIA-related contributions associated with the deglaciation and the LIA to 2100 CE, and non-GIA related contributions."

How does this work with the uncertainty the authors provide? Are the authors also linearly summing uncertainty across components? This needs explanation, since, again, the AR6 projections are provided in terms of percentiles, and unless a median estimate is being used, the uncertainty across percentiles cannot just be added together.

Because we used the percentiles provided for RSL driven by processes excluding ice-sheet mass loss and associated GIA to derive means and standard deviations for those contributions, we calculated the total estimate of RSL around Greenland by summing the means and derived the standard deviation of this estimate by square rooting the sum of the variances of the contributions. We have added a sentence to the text to state that this is how we combine the uncertainties across contributions (lines 572-573):

"The standard deviations of the contributions are combined by taking the square root of the sum of their variances."

It seems to me that the correct approach for the analysis the authors present would be to use the "IPCC AR6 Relative Sea Level Projections without Background Component" dataset, which is available here: https://zenodo.org/records/5967269. This dataset was specifically designed to allow scientists to use their own estimates of VLM to generate new RSL projections in a manner consistent with IPCC methodology. Using this dataset, my suggestion would be to simply use the AR6 ice sheet projections (also from ISMIP, which the authors use for their LIA-2100 ice-mass change, and which are already incorporated into the workflows as part of this dataset). The authors could still include the VLM component calculated from their LIA-2100 ice-mass change for their GIA/VLM "background" component of RSL. But, I think from a standpoint of correctly combining components within the AR6 framework, it's best to use the ice sheet estimates that have already been included within the framework in this dataset.

We thank the reviewer for this considerate suggestion. In our projections, we did use this dataset for the contributions to RSL from thermal expansion and ocean dynamics, land water storage, and global glacier change. In response to an earlier comment we now highlight this more clearly in lines lines 560-564. However, the projections of RSL driven by future ice sheet change provided by this dataset include gravitational, rotational, and deformational effects calculated using a sea level equation solver that assumes an elastic earth structure. An important finding in our study is the inference of a low mantle viscosity below Greenland over centennial timescales (like the timescale over which we are projecting RSL). Because of this, we find that 20-40% of RSL in Greenland at 2100 driven by future ice sheet loss is made up of viscous deformation. If we were to use the projections of RSL driven by ice-sheet change provided by this dataset and only include our projection of RSL driven by GIA from historical ice changes, we would not be accounting for this viscous deformation, which would lead to an underestimation of future RSL change along Greenland's coasts. We have revised the text to provide more detail on the comparison between our results and those shown in AR6. (lines 282-301):

"The most recent Assessment Report (AR6) of the IPCC includes regional projections of sea-level change at 2100 CE relative to a 1995-2014 baseline [1]. These differ from our Greenlandic projections in both magnitude and sign. For RCP SSP1-2.6, IPCC AR6 projects a RSL rise on the order of half a meter or less along most of Greenland's coasts, except the northern and northeastern portions, where a RSL fall is projected. For SSP5-8.5, projected RSL change is similar to that for SSP1-2.6 in most areas, but RSL fall is now projected to occur in the northwest near Kullorsuaq and is stronger along the northern coast. Our projections differ from those in AR6 in three ways: (1) how GIA in response to past ice melt is calculated, (2) how GIA in response to future ice melt is calculated, and (3) the ice-sheet models that are used for future projections. Regarding the first point, the authors of AR6 estimate RSL caused by VLM from GIA driven by historical ice changes and other processes with a Gaussian process regression method [61] that utilizes tide-gauge data. This dataset does not include tide-gauges in Greenland, making their inference for the long-term VLM component of RSL in Greenland uncertain. The authors of AR6 recognize the limitations in this method and assign their projections of long-term VLM and GIA low-to-medium confidence, stating that for many regions, a regional analysis would result in projections of greater confidence [1]. Regarding point (2), the authors of AR6 use a sea-level equation solver [62] with an elastic Earth structure. Viscous deformation associated with future ice change, which we estimate to be significant (see next section), is not included. Both factors likely cause AR6 projections to underpredict the amount of RSL fall that will occur over the next century in Greenland. Lastly regarding point (3), we highlight that our RSL projections were made using future ice-sheet mass changes from ISMIP6 simulations that were forced by CMIP5 (and not CMIP6) climate output. Therefore, they represent only a subset of the ice-sheet projections considered in AR6."

Note that when using this dataset, there are a number of citations/acknowledgements that should be included, as noted on the Zenodo page.

We again apologize for not including these citations and acknowledgements in the original submission and thank the reviewer for pointing out this mistake. They have been included in the revised version.

Taking this approach for the total sea level projections (and making it clear that this was the approach) will greatly strengthen the authors' discussion and comparison of their results to the AR6 regional results for Greenland. This approach will ensure that all components (and scenarios) considered are the same as in the AR6 regional projections, allowing the authors to more vividly emphasize the importance of their work on Greenland VLM when it comes to accurately assessing future SLR change for the region.

We thank the reviewer again for this feedback and hope that our revisions now more clearly describe our work.

Finally, the abstract says that the authors predict RSL fall of 2.8 +/- 0.8 m for RCP 8.5. Similarly, Lines 292-293 say, "The fall in RSL by up to almost 3 m that we project to occur by the end of this century along much of the coast . . ." However, looking at Figures 5-7, and considering the accompanying text, it's unclear how the authors conclude that their findings suggest "almost 3 m" of RSL fall "along much of the coast". It looks like up to ~2 m in many places, not 3. Please provide clarification of how the 3 m value is determined--right now I don't see data/results to support this conclusion.

> We agree that this is misleading and have re-written this portion of the sentence (line 317) as "The fall in RSL by up to 2.8 m for RCP 8.5 that we project to occur along Greenland's coasts".

<u>Minor Comments:</u>

Lines 259-261: "For all locations considered here, it is noteworthy that the projections for the two future emissions scenarios (RCP2.6 vs. 8.5) only start to diverge around 2050 CE." This may be noteworthy, but it's probably also worth mentioning that this is consistent with what many other studies have already shown (e.g., Garner et al., 2018, Horton et al., 2018, IPCC AR6 Chapter 9):

> The authors thank the reviewer for pointing us to other studies that have made a similar finding. We have mentioned this in the text and included the relevant citations (lines 279-281):
>
> > "For all locations considered here, it is noteworthy that the projections for the two future emissions scenarios (RCP 2.6 vs. 8.5) only start to diverge around 2050 CE, which is consistent with prior work on sea-level projections globally [e.g. 1, 59, 60] (Supplementary Fig. 5)."

Please ensure that the baseline for total RSL projections is stated clearly at least once in the results section for Future Projections of RSL, and in the captions of all figures that show total RSL projections, to provide clarity to reader

> The authors have added a sentence in the text to state the baseline clearly (lines 224-225) and have added the baseline in the relevant figure captions:
>
> > "All projections reported in this study are relative to 2017 CE."

<u>Comments on Figures:</u>

The maps generally look very good, however the colorbar on some of them becomes quite dark (e.g., Fig. 3b, Fig. 4b,d,f, and h), making portions of the map somewhat difficult to see. Would it be possible to adjust the colorbar to keep it lighter?

> We thank the reviewer for their positive comment and have adjusted the colorbar of the standard deviation plots to be lighter (Figs. 1, 3-5).

The station numbers on the inset map of Greenland in Figure 2 are not legible. I think this could be solved by making the map of Greenland larger--perhaps make it a third panel to the right of panels a) and b)?

> We agree with the reviewer and have made the inset map of Greenland larger so that the station numbers are legible (Fig. 2).

The white stars/black points on Figure 5d don't work well. The black points are particularly difficult to see. I'd suggest putting the black points on a different panel than the white stars. And perhaps make them a different color and/or shape, so that they don't blend into the map outlines.

> We have separated the black points and white stars onto two separate panels (5c and d) and believe this now makes it easy to distinguish the black dots from the map outline.

In Figure 8, it seems odd to discuss panel c) before a) and b) in the caption. Perhaps consider reordering the figures, or revisiting the caption.

> We have reworded the caption so that the panels are introduced in order.

We again thank the reviewers for their careful consideration and positive feedback regarding our responses to the first round of reviews. We have revised the manuscript based on the comments received through the second round. We describe these changes below by interspersing the original feedback in black with our response in blue. We note that all of the line numbers in our responses refer to the revised manuscript. We also include a version of the manuscript that indicates all changes to easily track changes from the original manuscript.

Reviewer #1 and #2 Evaluations:

We appreciate the improvements made by the authors and respect their choices, nevertheless we add an annotated manuscript with what we consider minor suggestions for further improvement. We don't need to review them any further.

We thank the reviewers for their positive assessment. We have addressed all minor suggestions below.

Line-by-Line Comments

L21: why not immediately mention SSPs here?

The contributions to RSL around Greenland from thermal expansion and ocean dynamics, land water storage, and global glacier change are based on SSP-RCP scenarios (i.e SSP1-2.6), however, the ice sheet projections we use are based solely on the emissions scenarios RCP 2.6 and RCP 8.5. We refer to the RCP emission scenario when we talk about total projected RSL, as only some of the contributions are based on SSP scenarios. We specify this in the revised text (lines 265-267):

> "Note that here and for the remainder of the text, we refer to the RSL projections as representing emissions scenarios RCP 2.6 and 8.5, which include the SSP1-2.6 and SSP5-8.5 projection components for the other processes."

L22: not clear from the rebuttal where the change from 30-40 to 20-40 comes from?

We misread the result of the relative contribution of viscous deformation to RSL at 2100 CE (Fig 8d) in the original submission, which led to this correction in the revised manuscript.

L66-87: This paragraph assumes that misfits can only arise from GIA, but uncertainties in ice dynamics and climate forcing maybe equally well be considered to be responsible for misfits.

We thank the reviewers for pointing out this nuance. We have included a sentence that says data-model residuals can come from uncertainties in both the ice reconstruction and the Earth structure (lines 71-73):

> “Each of these studies finds that their predictions of RSL are unable to match all of the observed RSL data, a consequence of uncertainties in both the ice reconstruction and prescribed Earth parameters.

L79: The previous sentences contain a very nice overview of the different viscosities that other studies found, but it doesn’t state anything about the misfits of those studies. Please add information on the misfits of other studies before stating in the highlighted part that improvement of the misfit is needed.

We have added a sentence that mentions how each of the studies we described that tunes their model simulations to fit paleo RSL data has remaining misfits to that data (line 71-73, see previous comment). We believe a more in-depth description of the residuals that exist for each study would dilute the main point of the paper. We highlight that we use published model runs for this component and it only contributes minorly to future sea level change around Greenland. While important to include, this component is not the main focus of the paper.

L85: Not clear where in the text these were indicated before.

As described in the response to the review for L66-87, we have now added a sentence where these misfits are explicitly mentioned (lines 71-73).

L85: This document does not contain a reference list, but according to the reference list in the new manuscript without tracked changes, this refers to Bevis et al., PNAS, 2012. I cannot find a comparison of 1D and 3D GIA models in this paper, is this reference correct?

We thank the authors for pointing out this reference error. It has been corrected in the revised manuscript.

L86: We have commented about this sentence before and the sentence is improved by replacing "beyond paleo RSL data", but it is still not connected to the rest of the paragraph. Please discuss before this sentence what the previous studies did incorporate to constrain the mantle viscosity and whether or not they incorporated present-day observations. Only after this information you could state that incorporating present-day observations could further constrain viscosity.

In lines 66 and 68-69, we specify that the study from Lecavelier et al. (2014), Tarasov & Peltier (2002), and Fleming et al. (2004) tune their GIA models to paleo RSL data to constrain solid

Earth parameters. However, we do agree that the last sentence did seem disconnected from the rest of the paragraph and have removed it from the revised manuscript.

L117: Reference error.

This has been corrected in the revised manuscript.

L146: explain better why you use 1D and 3D. Why is 1D sufficient for changes since LIA, why not use 3D as well?

We already provided a justification for using a 1D GIA model for the LIA instead of a 3D one in our last version, which is given in lines 143-146 of the revised manuscript.

L158: Why RCP and not SSP?

The ice-sheet projections considered in this study were taken from ISMIP6 simulations forced by CMIP5 climate models, which only represent the RCP scenarios and not the combined SSP-RCP scenarios. Our reasoning for using the CMIP5 forced ISMIP6 simulations is detailed in the response to the comment made on L382 below.

L185: Unclear what the basis is for the weighing presumably misfit with the observational record but you don't mention this.

In lines 128-130, where we give a brief description of how we calculated the deglacial contribution to both present-day VLM and future sea-level change, we write that we weight the models based on their fit to Holocene RSL data. This is then further detailed in the methods (lines 383-398).

L196: whereas ICE6G is .....

We have changed the sentence to provide a clear comparison between ICE-6G and Huy3 (lines 173-175):

> “This is expected, as additional and improved data constraints were used in the creation of of Huy3 compared to the GrIS portion of ICE-6G.”

Figure 2: Include in the caption over which time period the mean in taken.

We believe that including this in the caption would over-complicate the figure caption as the time period over which the mean is calculated differs for each site and also slightly for each contribution (i.e., the elastic and observed, the deglacial GIA, and the LIA contributions).

L352: A reference to new supplemental Fig. 4 is missing. The caption of that figure also misses a reference to where the figure was taken from I think.

Supplementary Figure 4 is referenced in lines 270-271. We have now included the reference where we obtained the individual ice sheet projections from in the caption.

L382: is there CMIP6 forced ISMIP, if so why did you not include that?

In order to account for the uncertainty in LIA ice history and earth structure, we could only run a subset of the future ice projections considered in the IPCC AR6. Most of the ensemble of CMIP6 models have Effective Climate Sensitivities (ECS; how much global mean temperature changes when carbon dioxide concentration in the atmosphere is doubled) that range from 2.3°C-3.2°C or 4.6°C-5.6°C. The CMIP5 models used in Goelzer et al. (2020), from which we obtained our ice-sheet projections, have ECS that are either close to the median (2.7°C-2.9°C) or are from the upper end (4.1°C-4.7°C) of ECS from the ensemble of CMIP5 models. So the ice-sheet projections used in our study were forced by CMIP5 models with similar ECS as that in many of the CMIP6 models. However, we don't represent the higher end climate sensitivity from CMIP6 models (ECS > 4.7°C), so our estimates of RSL at 2100 CE might be more conservative, as it is shown that those CMIP6 models do produce a greater GMSL contribution from Greenland (Payne et al., 2021). We have added a sentence to the manuscript to describe this limitation (lines 306-310):

> "The range of climate sensitivities represented in the ensemble of CMIP6 models has an upper tail that exceeds the range of climate sensitivities from the CMIP5 models used to create the ice-sheet projections considered in this study. Therefore, the ice-sheet projections used here represent a more conservative-to-moderate subset of those considered in AR6."

L404: layout mistake

We thank the reviewer for pointing this out and have corrected it in the revised manuscript.

<u>References</u>

Payne, Antony J., et al. "Future sea level change under coupled model intercomparison project phase and phase 6 scenarios from the Greenland and Antarctic ice sheets." *Geophysical Research Letters* 48.16 (2021): e2020GL091741.

Reviewer #4 Evaluations:

I have reviewed the authors' updates to the manuscript, and commend them for considerable improvements, especially to the readability of some figures that were previously difficult to distinguish, and for the inclusion of important clarifications in the text.

We thank the reviewer for this positive assessment.

However, I do still have one major concern, which again relates to the way that sea-level projections from AR6 are being incorporated.

In response to my original comment detailing concerns with the authors' approach, the authors said that "We have plotted the percentiles provided by the AR6 projections and those associated with our approximated mean and standard deviation for all processes at select times and locations, and find that they fit well." They also provided a plot of both AR6 data for the ocean dynamics component of sea-level rise and their own analysis, assuming a normal distribution.

However, for the ocean dynamics component for the AR6 projections, a t-distribution with the mean/co-variance of a multi-model ensemble constructed from CMIP6 models was used. A normal distribution fit to the ocean dynamic component of the projections would therefore be expected to align very closely, with the greatest discrepancies being in the tail, owing to the relatively few CMIP6 models available (~20), for which the t-distribution accounts for with a degree-of-freedom term. (see Fox-Kemper et al., [Ref. 1 in the manuscript], Kopp et al., 2023, [Ref. 50 in the manuscript] Kopp et al., 2014 [https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2014EF000239]).

This does not mean that it is appropriate to assume a normal distribution for all components or for the full SLR projections, which are well-known to have long (and important) upper tails. The AR6 datasets that the authors use are provided as full distributions for a reason. The appropriate methodology would require working with the full distribution and framing the authors' results in terms of that distribution. The current assumption of a normal distribution for sea-level rise projections is inappropriate--particularly given that sea-level rise projections are well-known not to conform to a normal distribution.

Building upon my concerns about this methodology, I continue to be concerned about the validity of the uncertainty that the authors provide for their total estimate of RSL around Greenland for the 21st century. As noted above, the correct approach for using the AR6 projections is not to treat them as normal distributions, but to work with the full distribution. If an approach that uses the full distribution is adopted (as it should be), the analysis of uncertainty will also need to be adjusted, since, as noted in my original response, uncertainty across percentiles cannot just be added together (unless it is the median).

We thank the author for this detailed comment and agree that using the full distributions instead of approximate normal distributions would improve the accuracy of our results. We have now changed our approach to capture the full distribution of each contribution. We calculate the total sea level projection by sampling the distribution of each contribution (at each location and timestep) and summing it together. This results in a distribution for the total future sea level change of which we report the median and likely range (which we define as the difference between the 17th-83rd percentiles, following the IPCC AR6). We also plot the 5th-95th ranges of the RSL distributions for each contribution and the total at 2100 CE for Tasiilaq and Nuuk to show the spread between the tails of our distribution (Fig. 6e-f).

These changes are propagated throughout the revised manuscript (e.g. see Figs. 4-6) and the changes in our methods are described in lines 398-399 and 571-582. We believe that our projections now incorporate the IPCC projections sufficiently well and analyze uncertainty as suggested, and again, thank the reviewer for their guidance on this methodology.

.

The manuscript presents projections of 21st-century sea level fall along the coast of Greenland. The authors use available ice mass change estimates from the Last Glacial Maximum to 2100, derived from various ice models and reconstructions, to compute relative sea level change using the GIA model SEAKON for different Earth structures. They analyze separately the contributions of GIA-related processes associated with past deglaciation, the Little Ice Age, and future ice mass loss. In addition they use to non-GIA-related processes from the most recent IPCC WG1 report. The ensemble mean results indicate that sea level change in 2100 is primarily driven by future ice mass loss and non-GIA-related contributions, with projected maximum sea level fall of 1 ± 0.3 m for RCP 2.6 and 2.8 ± 0.8 m for RCP 8.5.

General Remarks

The study is conducted thoroughly, and the manuscript is generally well written.

However, the broader impact of the study is superficially addressed and remains somewhat unclear. While it is acknowledged that reliable sea level projections have significant implications for Greenlandic coastal communities, this aspect is only briefly mentioned and not elaborated upon in the manuscript.

The authors highlight the limitations of traditional approaches that consider only elastic deformation in projections. However, it is unclear whether they are referring to sea level projections based on GIA models or dynamical ice sheet models. Ice sheet models incorporate elastic and viscous deformation in different ways, often assuming a constant present-day rate. It is not clear in the manuscript how that compares to the results of this paper.

The results section assumed prior knowledge on linear, nonlinear and transient behavior of the Earth's interior. Furthermore different rheologies are used on different timescales, which is not explained in the manuscript. The introduction should include an explanation of these different types of deformation and it's relevance on different timescales. Too much GIA expertise is presumed for the audience of Nat. Comm.

The spatial figures present ensemble means, but the standard deviation is relatively large due to variations among ice sheet models. Given this variability, displaying only the ensemble mean may not be the most informative approach. It would be beneficial to also present the mean per ice sheet reconstruction, as this likely determines the main pattern of deformation. In figures such as Figure 3, the relative error appears spatially consistent. It would be more informative to include a range of relative errors in the text and provide additional figures as suggested above.

Line-by-Line Comments

L12: Replace However by As a consequence

L14: Predictions is used throughout the manuscript where projections are meant, please change

L20: It is completely unclear what is meant with traditional approaches. Some applications have GIA as a constant offset. Does traditional approaches refer to GIA to ice sheet models or to IPCC style sea level projections

L22**:** can these later two statement be more detailed. For a tenth of the change the sentence can still be made but the impact is negligible.

L66-69: Provide the full range of upper mantle viscosity and lithospheric thickness values from references 5, 26, and 27 to give the reader a better overview of the uncertainty rather than highlighting a single result.

L72: The reference to an AGU abstract (ref 29) appears redundant, given the availability of peer-reviewed sources supporting this statement.

L71-74: Reference 33 states that lateral structure does not explain misfits to RSL data. Could you clarify how this compares to refs 5, 8, and 23? It is mentioned that the fit improves but it is not clear whether misfits remain after including lateral variations? If ref 33 also includes lateral variations, consider citing it together with refs 5, 8, and 23.

L74-75: The phrase "observations beyond paleo RSL data" is somewhat vague. Could you specify what is meant here?

L79: Define the time frame for "since the last deglaciation" and "contemporarily." The period from the Last Glacial Maximum to the start of the Little Ice Age (~1000 CE) is defined in L114-115 but should be introduced earlier to improve readability. Could the authors clarify the exact time period they are referring to?

L92: Do you mean during or since the LIA?

L94: A non-GIA person will not understand why mantle viscosity would be smaller since the LIA, as it is suggested to be a material property. Please rephrase to make this clear for non-GIA specialists

L97: Is ref 39 the only reference discussing higher-order linear viscoelasticity and are refs 40 and 41 the only references to related to non-linear rheology? Please clarify the distinction.

L102-103: The term "spatially extensive asthenosphere" is subjective. In Antarctica, an order of magnitude difference in mantle viscosity significantly affects the response, and similar variations can be expected between east and west Greenland. Please provide further clarification.

L120: Between 1000 CE and 2100, VLM observations are corrected for coeval (and deglacial) ice mass change. However, coeval ice mass change is included in the GPS signal. Why is it corrected for separately?

L131-136: Ref 44 is not mentioned in the introduction but should be placed in context in the introduction to improve readability. Refs 8, 33, and 44 are not discussed further, but ref 5 is—why? Additionally, why are refs 15 and 45 not mentioned alongside refs 5, 8, 33, and 44 in L132?

L143: Add a brief explanation of what "normalized weights" mean at the start of the paragraph.

L143-152: Since ice history is a key factor, please consider adding a supplementary figure showing average ice mass change from ICE6G, Huy3, and other reconstructions/models. Include a brief description of these reconstructions.

L154-164: The method for removing elastic and GIA signals from observations is not entirely clear. Please include a brief explanation in the introduction of this section. Additionally, a short explanation of the Bayesian approach in L120 would help in understanding Figure 2.

Ad Figure 2: very difficult to see a difference between prior and posterior. Can you plot a panel a the prior and as panel b the posterior-prior or maybe in one plot observed-prior and observed-posterior. Alternatively We suggest to show all three components (observations-ELA-GIA, Prior LIA and Posterior LIA) in one figure using overlapping boxplots in three different colors instead of shaded areas.

Ad Figure 3: How large is the relative standard deviation? 15%? seems an insightful quantitative value. It also looks like there is barely a spatial pattern in the standard deviation it is more or less a relative error. So what is the added value of panel b, maybe you can leave it out and just mention the relative error.

L171: It is not clear whether the weighted average LIA vertical land motion of all simulations is similar to the average of Adhikari. Could you clarify this?

L180-184: The finding of low upper mantle viscosity does not imply that the entire upper mantle has this viscosity, as a 1D model is used. Rather, it suggests that the regions where the mantle is sensitive exhibit this viscosity, while other areas (insensitive to the ice load) may have different viscosities. Could you clarify how this leads to an indication of transient viscosity approaching steady state? Why would the mantle viscosity be smaller since the LIA?

L194 Do you intend to say effect the results only marginally?

L198 Repeat that this is the forebulge effect of North America, if that is what explains it.

L204 Unclear from where you took the Antarctic values.

L207: Could you specify which gravitational effects are included—self-gravitation of ice on relative sea level, self-gravitation of the Earth on deformation, or both?

L208: You refer often to Table S2, would it be possible to absorb it in the main text?

L211: Please include the spatial distribution of gravitational effects on sea level in the supplementary materials. The range of ~20-30%—what does this depend on? Was only one simulation conducted? How much does this effect amount to in meters? Please also show the spatial distribution in the supplementary material.

L212-218: Are these results derived from literature rather than modelled? If so, please add references.

L240: So separating does not add much. I think you can describe one and argue that qualitatively the other locations are yielding comparable results.

Figure 4: unclear what the with areas in panel I to l are (same in Figure 5)

L260: I am not sure whether that is noteworthy as this holds for most points along the world coastline.

Ad Figure 6: try to homogenize the lay-out of the figure. Unclear what the figure adds. I believe it can be left out.

Ad Figure 7: What is the added value of a second location I think only Nuuk is sufficient? Explain the red curve in the caption as well.

L294: Not sure whether this is true. It assumes the marine terminating glaciers dynamics are controlled by water depth. I think that is questionable, at least make clear this is what you assume and provide reference for this assumption.

L305: Adhikari et al. studied Antarctica and the response of the Antarctic Ice Sheet and its ice shelves cannot be directly compared to that of the Greenland Ice Sheet and its outlet glaciers due to fundamental differences in their physical processes and environmental conditions. Please include this nuance in the manuscript.

L307 can you explain this in more detail. Why has a ~ meter an impact? in view of the hazards and threats communities are exposed to?

L311: In the context of publishing the manuscript in Nature Communications, it would be valuable to elaborate on the interdisciplinary aspects of the study. Could you clarify what interdisciplinary studies are meant?

Ad Figure 8: If understood correctly I think the text discusses the relative contribution for the viscous part why not show that or show both?

L312: clarify that you prescribe ice sheet history and what that implies. a. Not a consistent glaciological model, and b not a coupled solution with a glaciological model.

L334: Confusing phrasing. it is the same model but 65 different members with different physical component or parameter settings

L335: What is meant by the latter I presume the present-day VLM; unclear what that precisely is. The present-day rate or the VLM caused by the future melt?

L349: observed RSL at location y add the last bit.

Line 354: can you explain in words what the second line in equation 1 secures. I guess it has to do with the limiting aspect but unclear how that works.

Line 378: but that those not exclude that lateral variation in the structure are important. You have to comment on this despite you use a 1D model to provide context on the limitations of your 1D set-up

L395: why is that not affecting the entire south(western) part?

L422: I would expect V_RF to be independent of time, but you suggest you use each of the 14,040 predictions?