Reply to comments by anonymous Referee (R2)

We would like to thank the reviewer for their thorough and valuable comments on our manuscript. We appreciate the opportunity to clarify and improve our work based on the feedback. Below, we provide responses to the comments.

General comments:

Comment: (1) "Piece of story". From the current research I don't see the novelty of the results. Methodologically you do large effort for the historical simulations but then the concluded results are very general or not new. As an example: The lines 16-21 (abstract) somehow repeat results that are not new: Sensitivity of glacier (low/mid/high) retreat forcing was shown by Rückamp et al. (2020), ISMIP6 sea level projections (both CMIP5 and 6) were shown by Goelzer et al. (2020) and Payne et al. (2020) (except SSP2-4.5, you refer to it in Line 409), relevance of dynamic vs SMB mass loss by Choi et al. (2021) and Rückamp et al. (2020). I think, the different initialization approaches and their influence are the novelty part of this work, and you should focus on that (as you did partly). You should more highlight, how your work is connected to initMIP (improve initializations) and/or to ISMIP6 (improve projections). Well, both topics are closely connected to each other, but I think it makes a difference when describing the scope.

Response: (1) We appreciate this feedback and have decided to restructure the manuscript to create a more coherent narrative and to focus on the novelty aspect of our study (intialization, anomaly vs absolute climate forcing). We will provide more in-depth discussions on these parts of the study, and – to further streamline the manuscript – have decided to omit the work on the partitioning of SMB vs ice loss by outletglacier retreat (Sect. 3.3.1). Furthermore, we will adjust our introduction and conclusion accordingly and highlight how our results complement existing studies like InitMIP and ISMIP6.

Comment: (2) I found the paper a bit overloaded with experiments not needed. As presented so far, I don't understand how the different ocean forcings (low, medium, high) and the experiments with and without elevation feedback help do understand the effect initialization approaches on the projections? So please skip these simulations and focus a bit more on the initialization (as in Fig. 7); if that is the overall scope. The grid dependency was not shown although mentioned in the discussion and conclusion. The

promised comparison of absolute forcing vs. anomaly forcing (line 76) is very weak and not supported by any figure and not shown in the results section. To my understanding it is rather a technical detail than a comparison (because ERA5 is not available beyond ~2020). So far, I have not understood why you use one CMIP5 model and all other CMIP6 models? Why SSP1-2.6. SSP2-4.5 and SSP5-8.5 scenarios? I case you want to investigate the influence of the initialization on the projections, a few (well selected) projections runs would be enough.

Response: (2)
1. Focus on initialization: Concerning the advise to condense and focus our results, please see the response to the previous comment.
2. Grid dependency: We will include a figure and analysis of the grid dependency.
3. Comparison of absolute SMB forcing vs anomolous SMB forcing. We underdstand the need to clarify the difference of absolute SMB forcing vs. anomalous SMB forcing and we will iclude a schematic to make this more accesible.
4. Climate forcing: We based our selection on climate forcing on availability. Our aim is to sample a wide range of climate models and emission scenarios in order to understand how a range of potential future climate conditions would impact the Greenland ice sheet. Furthermore, putting the (small) impact of the initialization into the context of climate pathway uncertainty, we are able to show that scenario uncertainty is larger than uncerainty due to initialization or absolute vs anomaly forcing.

Responses to specific comments: We thank the referee for their specific feedback and suggestions for improvement of the manuscript. We will clarify the parts suggested by the referee. Furthermore, we will include a more in-depth description of the model setup, explain the necessity of the SMB-height feedback parameterization and expand on the treatment of climate forcing in the historical period. We will also provide a more in-depth analysis of our results for the historical period. Moreover, we agree that climate forcing and simulation results need to be made available. Below, we offer responses to select comments where we believe clarification is needed.

Comment: Line 205: The simulation ESM-SMB init is not introduced. Do you mean you use the SMB of NorESM from the ESM-init ensemble? Anyway, I found the paragraph "To further explore ..." hard to follow, because it is not demonstrated with figures/numbers.

Reply: The results of this paragraph were presented in Fig.3, but we acknowledge that this could be better explained.We will further clarify the experiments described in this paragraph.

Comment: Line 222: I am bit confused about the negative and positive signs of SLC (i.e. negative signs == loose mass). Maybe it would be more intuitive to show SLC relative to 1960 or as change to initial ice mass.

Reply: We decided to present all results in terms of sea-level contribution with respect to 2015 to make our results directly comparable to other studies such as ISMIP6.

Comment: Paragraph 3.2 (1) First of all, I found it very interesting that all simulations start with a large SLC spread in ~1960 and then converge towards 2015. Any explanation for that?

Reply: We show the mass loss in terms of sea-level contribution with respect to the year 2015. Therefore, all simulations converge to 0 mm SLC in 2015 by definition. We realize that our historical simulations could benefit from further analysis and we will provide this in the revised manuscript.

Comment: Around Line 395: Maybe I missed that comparison in the methods/results section. What do you mean with baseline SMB? Directly comparable: I guess, they are identical. Some equations would help.In my understanding, you have in the anomaly approach:

SMB(t)=SMB_ESM_1960-1989 + SMB_ESM(t) - SMB_ESM_1960-1989

which is identical to the absolute approach: SMB(t)=SMB_ESM(t).

Reply: We acknowledge the need to clarify the difference and will do so in our revision. In terms of applied SMB the anomaly approach and the absolute differ as follows:

Anomaly approach: SMB_applied(t) = SMB_ref_ERA5_annual_mean_1960-1989 + SMB_ESM(t) – SMB_ESM_annual_mean_1960-1989.

Absolute approach: SMB_applied(t) = SMB_ESM(t).

We believe these revisions address the reviewer's concerns and greatly improve the clarity and focus. We thank the reviewer again for the helpful feedback.