Author’s reply to referee comments

We would like to thank the reviewers 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 a point by point response to all comments. Lines in parentheses in the response refer to the revised version of the manuscript.

Reply to comments by anonymous Referee (R1)

General comments:

Comment: (1) I find some conclusions not evidently supported by the simulations. For example, 'While discharge from outlet-glaciers remains a substantial factor, the future evolution of the ice sheet is governed by mass loss due to changes in SMB.' First, this conclusion might be forcing dependent, i.e. if you use another ESM output as SMB forcing, does this conclusion stand? Second, the authors prescribed ice front retreat in the simulations to compare, although ice discharge is not the same as mass loss due to frontal retreat (calving), and frontal retreat is only one of many factors that can cause changes in ice discharge (others are like basal sliding, dynamical thinning).

Response: We agree that the results regarding the relative importance of SMB vs discharge is forcing dependent. Indeed, in our manuscript we have analyzed this partitioning for a range of different forcings as simulated by the ESM-scenario ensemble used in our study (see for example Fig 10). However, following both reviewers’ suggestions, we have streamlined the storyline and placed greater focus on the novelties of this study, e.g. initialization and historical consistency of our projections. We therefore removed section 3.3.1 of the study. Consequently, we have removed respective parts from the abstract, introduction and conclusion.

Comment: (2) Methods description is quite general. For example, what does medium sensitivity mean and how is it defined? For the mechanisms that the authors are trying to explore, such as SMB-elevation feedback, it would be helpful to present the equations.

Response: We have expanded on our description of the setup and applied parameterizations (see Sect. 2.1 and relevant specific comments).

Comment: (3) The writing is easy to follow, but the logic is not always clear. For example, after reading the introduction, I don’t see how these pieces of work connect with each other to form one piece of story. There are other mechanics not explored in ISMIP6 (e.g., sliding laws, initialization methods) if the target is to compensate ISMIP6. Here, I by no means suggest more experiments, but organizing and streamlining the structure.

Response: We have restructured the manuscript to create a more coherent narrative. Following the suggestions of both referees, we removed section 3.3.1 and instead focused on the novel initialization aspect and the impacts of the initialization on the projections, as well as to expand on the analysis of the historical period. Furthermore, we have adjusted our introduction and conclusion accordingly highlighting how our results complement existing studies like ISMIP6.

Comment: (4) Some conclusions are presented without evidence. For example, the mesh convergence study was conducted and suggested 16km resolution is enough, but no figure to present this. I’m happy that the authors have done mesh convergence study to ensure a proper numerical setup, but I’d like to see these results presented (maybe in supplementary).

Response: We have included a figure showing the differences due to grid resolution (Fig. 11) and presented results from the grid sensitivity study more in-depth (line 327 – line 334).

Response to specific comments:

Comment: Topic: remove the period dot

Response: Removed period dot.

Comment: Abstract: Quite lengthy with too much details?

Response: We revised the abstract, cutting out parts and shifting the focus to historical consistency of the projections.

Comment: Line27: Please check the citations. I don’t think Shepherd et al., 2012 suggested the contributions from SMB and ice dynamics separately. This recent article is more appropriate to be cited here: The IMBIE Team. Mass balance of the Greenland Ice Sheet from 1992 to 2018. Nature 579, 233–239 (2020). https://doi.org/10.1038/s41586-019-1855-2

Response: We have revised the introduction so that this part no longer applies.

Comment: Line49-66: The description of different initialisation methods is a bit confusing. The authors used ’consistency’ and ’self-consistent’ to describe the advantage of paleo spin-up approach. In my opinion, as long as the model solve the equations correctly, the output variables should be consistent. Does ’consistency’ means ’an equilibrium state’ ? Or ’consistent’ with the paleo climate? It would be helpful to explain the issues (and reasons causing them) explicitly.

Response: The aim here was to express that our initialization approach results in an ice sheet that is in balance with its forcing history. We realize now that the word 'self-consistent' is not needed and removed it. We furthermore explained consistency as consistent with its forcing.

Comment: Line74-79: ’sample uncertainties’→’quantify uncertainties’ ?

Response: We decided to keep ’sample uncertainties’, meaning we are looking at different uncertainties in the ensemble.

Comment: Line74-79: This paragraph summarized the study, although not streamined. Initialisation methods, SMB, ice dynamics of outlet glaciers are mentioned. However, the uncertainty study is then on climate forcing and ’modeling choices’. The connections between the pieces marked by ’.... Additionally, . We.... Moreover...’ are not obvious. This might be improved by a better narrative.

Response: We revised the respective paragraph, improving the narrative by omitting ‘Moreover, we perform a suite of single forcing experiments to separate the respective contribution of SMB and outlet-glacier retreat, alongside the SMB-height feedback, in driving the projected mass loss.’ and focusing on the historical consistency of our projections (line 64-71).

Comment: Line91: Is 4km resolution enough for the simulations? Did the authors conduct a mesh convergence study to ensure the model behaviour is not restricted by the choice of grid size? I refer the authors to this article: Cornford SL, Martin DF, Lee V, Payne AJ, Ng EG. Adaptive mesh refinement versus subgrid friction interpolation in simulations of Antarctic ice dynamics. Annals of Glaciology. 2016;57(73):1-9. doi:10.1017/aog.2016.13

Response: We have included a figure showing the differences due to grid resolution (Fig. 11) and presented results from the grid sensitivity study more in-depth (line 327 – 334).

Comment: Line96-97: How is ocean forcing set at the ice-ocean boundary?

Response: Retreat of marine-terminating outlet glaciers is prescribed as maximum ice front position (via a retreat mask) applying a semi-empirical parameterization according to Slater et al. (2019, 2020). The parameterization is now described in more detail in Sect. 2.2.

Comment: Line97-98: 'All floating ice is assumed to calve immediately'. Is removing all floating ice a reasonable setup for Greenland projections? How much of a difference would it cause? Could you provide some evidence (citations) to justify this approach?

Response: We added a justification and a reference (line 84-88).

Comment: Line103-105: Can you explain in detail the SMB-elevation feedback with equations and/or add the relevant references?

Response: We explained the SMB-elevation feedback, added references and provided more detail and references regarding the parameterization (line 92-105)

Comment: Line107 (section 2.2): Is the runoff depth averaged or depth integrated? The sentence suggest location of ice front position is prescribed as a function of maximum ice front position, far field ocean temperature and runoff. How is maximum ice front defined? How do you adjust the ice front positions (e.g. do you remove or calve ice out of the prescribed boundary)? Again, adding some equations will help. It will also help when you later on explain uncertainty caused by the choice of parameters, i.e. what are the values of parameters? What are the ranges of these parameter values in the sensitivity simulations? What are the calibrating variables (Line 112 mentioned 'calibrated values', but 'calibrated values' of what variable? Sea level contribution, mass loss, or other index?)? Equations and tables may be helpful.

Response: We provided a more detailed description of the parameterization, added an equation and refer to the two papers, which describe the parameterization and its calibration in detail (line 106-119).

Comment: Line161: delete the first 'approach'.

Response: Deleted.

Comment: Line166: Could the authors add a figure with total SMB applied to the ice sheet for the two ensembles? The discription of this paragraph is a bit confusing. Did you use absolute values of SMB and ST from the ESMs? Why is it necessary to divide the reference part and anomaly part?

Response: We added two schematic equations to make the difference more descriptive (line 177-191). The reasoning for this partition is explained in the introduction (line 57-63).

Comment: Line176: unit is missing

Response: Added unit.

Comment: Line177: What are the four forcing modes? I believe this is not mentioned before. Also, I cannot quite put the numbers together 10(ESMs)*3(SSPs)*3(sensitivity params)*3(grids)*2(init methods)=540 simulations. How are 304 and 192calculated? What does ’full forcing’ mean? I think here a table would be helpful.

Response: We omitted the part with the different forcing modes. The number is the product of all variations we run. Note that we run different scenarios only for some ESMs.

Comment: Line184: ’ACCESS1.3’ was not explained before this location.

Response: Removed the explicit mentioning of the ACCESS1.3.

Comment: Line204: To my understanding, the two experiments here are using two different initial geometries: one with ESM spinup, the other with ERA spinup, same basal friction product (ERA spinup), and same SMB forcing (ESM) for the 1000 year relaxation. In this case, the first sentence of this paragraph is a bit misleading (second half). Furthermore, the authors concluded that ’the initial geometry is not decisive for the resulting ice sheet geometry, and it’s the mismatch between SMB and friction that leas to a deviation from the ERA5 initialization’, which I think is questionable. Here is my argument: When we look at Figure 3a and b, they shows a similar pattern of surface elevation difference comparing to ERA5 initialisation. The 1000 year relaxation of ERA5 used geometry from ERA5 spinup, basal friction from ERA5 spinup, SMB from ERA5; the two experiments here used geometries from ERA5 and ESM spinups (slightly different as the authors said), basal friction from ERA5 spinup, SMB from ESM. The differences can’t be dominated by geometries, because the one using ERA5 and shows the same pattern with the other. The only explanation would be different SMB products caused the geometry differences in this 1000 year relaxation. Maybe I have misunderstood the setup of experiments, in which case, the authors are welcome to correct me, and maybe explain a bit more in the text.

Response: We clarified this part, by rewriting and making the reasoning for this experiment more clear. We also decided to simplify by showing only one of the experiments, as this is sufficient to make our point.

Comment: Line223: Fig. 4b→ Fig. 4

Response: Yes, that was incorrect.

Comment: Line281-285: I find describing parameters as low, medium and high hard to understand in depth. Could you give the numbers, maybe as well as the calving front locations when applying different parameters?

Response: This is now better explained in Sect. 2.2

Comment: Line300: Usually we cite figures in order, so the citation of Fig. 10 would normally be before the citation of Fig. 11.

Response: We removed Sect. 3.3.1 (see response to general comments).

Comment: Line307: Period dot missing. How does SMB-induced upstream counteracts the effective removal of ice via discharge?

Response: We removed the respective section.

Comment: Paragraph from Line310: I guess the proportion numbers will change according to the applied SMB products of ESMs?

Response: We removed the respective section.

Comment: Line319: How did you calculate discharge from outlet-glaciers? How are the proportions in Figure 10 calculated in the simulations with different SMB forcings?

Response: We removed the respective section.

Comment: Line326: If you have done simulations with other ESMs and have the relative numbers, could you present them in the main text or supplementary? For example, the MPI-ESM1-2-HR run results mentioned here are not presented (in Figures/tables).

Response: We removed the respective section.

Comment: Line360: I think the number should be 40mm rather than 80mm according to the reference. However, Goelzer et al., substract control simulations from projections, while this study used projection results directly, so I'm not sure how comparable are these two numbers.

Response: We refer to a spread (2-sigma-range) of 80 mm for ISMIP6 projections forced with MIROC5 under the RCP8.5 scenario assuming medium sensitivity to retreat forcing, as is discussed in Sect. 4.3 in Goelzer et al. (2020). We have clarified this in line 346 - 354. Indeed, Goelzer et al. subtracted control projections, as many simulations showed a large model drift after the initialization. In Sect. 3.2 we have expanded on our explanation as to why we include the drift (largely stemming from the historical forcing) in our projections. Even though technically this is a difference to the ISMI6 study, we believe that comparing uncertainty due to ice sheet model to uncertainty due to climate forcing is still valuable here.

Comment: Paragraph of Line385: The arguments of this paragraph is hard to get.
(1) This sentence 'control experiments only offer....rather than providing conclusive information about its future response to historical forcing.' seems to indicate historical period is not reconstructed confidently, while the last paragraph conclude the historical run matches well with observations. (2) The argument on committed sea-level contribution in historical time is also confusing. To estimate committed sea-level contribution by 2015, it was suggested a constant value per year is necessary; then after 'However' suggest the annual mean constant is hard to get due to interannual variability; in the end proposed a solution is to use moving mean over time. Why can't we apply SMB with interannual variability to model simulation? If the model need yearly SMB, can't we simply remove the interannual variability by annual mean?

Response: We revised this paragraph and clarified that we aim here to discuss the interpretation of model drift after the historical period and the calculation of committed sea-level contribution (line 376-387).

Comment: Paragraph of Line415: Differences caused by mesh resolution has been mentioned, and 16km is suggested to be enough. I suggest to present the mesh convergence results in the supplementary as evidence.

Response: We have included a figure showing the differences due to grid resolution (Fig. 11) and presented results from the grid sensitivity study more in-depth (line 327 – 334).

Comment: Line 416-420: Are this argument relevant or supported by this study?
The argument about parameterization seems to be contradict with the argument that 16km mesh grid is enough?

Response: We have rewritten this paragraph to clarify out intent (line 406-414). It is not a contradiction. The strength of the parameterization is its grid independence, but at the same it’s also its weakness, as it fails to capture and explicitly resolve the small scale processes responsible for outlet-glacier retreat. We aim here to point to the necessity for further development.

Comment: The last paragraph is also not very relevant to this research.

Response: We have removed the sentence.

Reply to comments by anonymous Referee (R2)

We would also like to explicitly thank Referee 2 for their feedback and suggestions, which helped significantly to improve our manuscript.

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: Following both reviewers’ suggestions, we have streamlined the storyline and restructured the manuscript to create a more coherent narrative. Following the suggestions of both referees, we removed section 3.3.1 and instead focused on the novel initialization aspect and the impacts of the initialization on the projections, as well as to expand on the analysis of the historical period.

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 included a figure and analysis of the grid dependency (Fig.11).

3. Comparison of absolute SMB forcing vs anomalous SMB forcing. We clarified the difference of absolute SMB forcing vs. anomalous SMB forcing and included a schematic equation to make this more accessible (line 177-191).

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 uncertainty due to initialization or absolute vs anomaly forcing.

Responses to specific comments:

Comment: Line 9: I have some concern. You match the grey area in Figure 4, but how is the spread in SLC before ~1990 explainable?

Response: The spread in sea-level contribution is due to the fact that we display sea-level contribution relative to 2015. We clarified this in Sect. 3.2 and we have also added an additional Figure (Fig. 4) displaying the absolute mass loss (in Gt) of the different ice sheet configurations as well, showing that the initial masses differ between the configurations.

Comment: Line 11. “Atmospheric forcing is downscaled with MAR …”. I am curious what you did in the historical period? How are the climate data treated in the historical period? You should spend some time describing it, as it is a major difference to the initMIP/ISMIP6 approach where every user come “as is” to ~2015. I am curious how the retreat parametrization works in the historical period. The ISMIP6 retreat and height gradient parametrization were only available in the future projections in ISMIP6. Maybe it improves the projections once you have a consistent forcing from ~1960 onwards?

Response: We have extended the description of the historical setup in Sect.2.3.2, as well as the analysis of historical simulations in Sect.2.3.3).

Comment: Line 13 “Furthermore, we disentangle the importance …”. You should consider dropping it. You focus on the initialization, I guess.

Response: We removed this part.

Comment: Line 40: What do you mean by ice sheet model uncertainty?

Response: We have added more explanation (line 28-30).

Comment: Line 88: There are many higher order models available in the community so please precise „higher order model” or give a reference.

Response: A reference is given in line 75.

Comment: Line 88: How is the 3D temperature field treated in the depth-integrated viscosity? You should also say/discuss if the basal ice flow is coupled to basal temperature or not.

Response: We have added further explanations and references to Sect. 2.1 and mention that in our setup basal ice flow is independent of basal temperature in Sect. 2.3.1.

Comment: Line 90: Are the 11 vertical layers equally spaced or refined to the base? For a thermomechanical coupled model, I would expect them refined to the base to resolve the shear heating at the base.

Response: They are indeed refined towards the base. We added specifications (line 80-81).

Comment: Line 90: power law: Please be precise or give a citation which type of law is used (e.q. Budd, Weertman, Coulomb, …).

Response: We specified the type of the power law (l.80).

Comment: Line 91: Maybe I missed it, but in the results sections, you don't show the grid dependency. Therefore, your conclusions (e.g. Lines 21, 369/370, 414) are not supported.

Response: We have included a figure showing the differences due to grid resolution (Fig. 11) and presented results from the grid sensitivity study more in-depth (line 327 – 334).

Comment: Line 92: please define date of "present-day"

Response: We have removed this part and specified this aspect further in Sect. 2.3.1.

Comment: Line 93: Are bedrock and elevation smoothed? Please explain why it is smoothed.

Response: We have added an explanation (line 132-135).

Comment: Line 94: I don't understand "prescribed". Do you mean initialized? Because it then evolved …

Response: Correct, changed 'prescribed' to 'initialized'.

Comment: Line 94-103: I am bit confused about the terminology used here. You use "climate forcing" and "atmospheric forcing" from ESM's. I understand what you mean, but please differentiate between climate and atmospheric forcing/data and what is finally used for the ISM. I suggest something like (very basic): "We use ESM climate data which has been downscaled in order to prescribe an appropriate boundary condition for the ISM."

Response: We describe this in Sect. 2.2.

Comment: Line 103: Please explain briefly, why the elevation-feedback parametrization is necessary (MAR is running on stationary surface elevation).

Response: Yes, we included a description of the SMB-elevation feedback and explained why it is necessary to include a parameterization (line 92 - 96).

Comment: Line 103: Maybe add "… anomalies with respect to reference period".

Response: We added 'with respect to a reference period' (106-107).

Comment: Line 121/122 Is 1960 really present-day?

Response: We reformulated to "closely resembles recently observed conditions".

Comment: Line 129: ice sheets (don't use plural for ice sheet unless you refer to GIS and AIS) -> ice sheet configurations

Response: Good point! We changed ‘ice sheets’ to ‘ice sheet configurations’.

Comment: Line 129: relaxation: if the relaxation belongs to the initialization procedure, I would not explicitly name it later. Should be clear form the init-approach described here (see captions of Figure 1 and 2.). I do understand this further relaxation. I would have been expected that the inverse transient approach minimizes the drift already. The additional 1000/500 years make the approach a bit questionable … and how they final states (geometry, ice masks, ice velocity) correspond to the target. This could be presented and explored better.

Response: We adapted the suggestion of the referee to refrain from explicitly naming the relax for the remaining part of the manuscript unless it is explicitly necessary. We added further explanation as to why perform the additional relaxation (l.137-138). Furthermore, we added information on how the initial ice sheet configurations compare to the target ice thickness and added a figure to the supplements (Fig. S1)

Comment: Line 134 to 136: I found “ensemble” confusing here. It is only one climate forcing experiment (ERA5) and not an ensemble. Maybe use “initialization approach”?

Response: By ensemble we refer to the ensemble of projections.

Comment: Line 135 and Line148/149: What match well? And how is ‘well’ defined? That is no shown.

Response: We added a reference (line 149).

Comment: Line 141: computer resources: Is 4, 8, 16km really very expensive (with 11 layers temperature-coupled) compared to e.g. Blatter-Pattyn or FS (11layers and temperature-coupled) with a much higher resolution? I guess a 4km resolution with your model is rather cheap … But I can only guess without having any number.

Response: We agree that our model is relatively cheap, even at 4km resolution. However, the argument that we are making here is a more general one. We say that using one initialization to branch the projections off of, generally allows for a larger ensemble of projections, which is potentially interesting for other ice sheet models, especially when including a larger range of forcing or when testing a larger range of parameters. We believe that exact numbers of computing costs would not benefit the manuscript, since they are not relevant for the study.

Comment: Line 142-144 the paragraph is not well connected to the text. Suggest dropping it.

Response: We removed this paragraph..

Comment: Line 145: “Historical period”: I found this chapter much too short. The title of your paper is “Historically consistent …”. For me, there are many open questions. So, I am wondering how the retreat parametrization is behaves in that period? What about the height-elevation feedback? Is that changing over time? What really means “historically consistent”, that was never mentioned.

Response: We extended this chapter and elaborated on the historical consistency (258-268).

Comment: Line 153/154: “Note that ESM’s …” Please give a reference. Basically, ESMs generate no SMB.

Response: We added a reference to support this statement (e.g. line 169).

Comment: Line 152 and 166: This contradicts with your statement in Line 103 where you say that anomalies (and absolute) are used. Well, ok, I re-read it: so please make it more prominent in the text where needed …

Response: We make this more clear by adding schematic equations (line 177-191).

Comment: Line 155: add that sensitivities are also performed to grid resolution

Response: This is already mentioned in line 81, as well as line 199.

Comment: Line 184: ACCESS-initialization -> please rewrite: "… except for the ESM ACCESS of the ESM-init ensemble". Is there a need to mention ACCESS separately? I mean, the full range of -41Gt to -110Gt (about 0.1-0.3mm SLE over 100 years) is very small compared to the projections and the historical runs.

Response: We refrained from the explicit mentioning of the ACCESS run.

Comment: Line 188: observational data -> observed geometry/ice mass. Well, its not surprising that the initial state matches the observed geometry, because that's the target of the initialization (although I don't how the additional relaxation works …) . But I am curious how the ice surface velocity matches observations.

Response: We agree that this would be interesting to further investigate. However, for now we decide to keep our discussion focused on the target geometry for which we have added a figure to the supplements (Fig. S1).

Comment: Line 189: maybe rewrite to: "…the limited ability of the inversion in the initialization approach …". You may also consider that floating ice in your simulations is directly calved off.

Response: We rewrote to 'the limited ability of the inversion in the initialization approach to compensate for biases in the initial SMB'.

Comment: Line 190: You introduce the simulation ID ERA5-init, so please use it: Figure 1 shows the state of ERA5-init (with medium ocean forcing).

Response: Yes, accepted.

Comment: Line 191 common pattern in what? Please be precise.

Response: We specified and elaborated more on this aspect (line 214-232).

Comment: Line 192: ERA5-initialization -> ERA5-init

Response: OK.

Comment: Line 193: I don't understand why you focus on the comparison of ERA5-init to ESM-init with NorESM. I found the argument that it is the in-house product very weak and not well supported. In case you want to focus on the in-house model drop all other experiments and refresh the title

Response: We specify that we do this for purposes of an exemplary more in-depth analysis (line 221-232).

Comment: Line 200: “This is a residual …” -> could be tied together with statement in Line 188.

Response: We decided not to change this part, to ensure clarity.

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.

Response: The results of this paragraph are presented in Fig.3. We clarified this part, by rewriting and making the reasoning for this experiment more clear. We also decided to simplify by showing only one of the experiments, as this is sufficient to make our point.

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.

Response: 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. We clarified this in Sect. 3.2 and we have also added an additional Figure (Fig. 4) displaying the absolute mass loss (in Gt) of the different ice sheet configurations.

Comment: Line 223:

(1) ESM-initialized simulations -> ESM-init

(2) Fig 4b -> Fig. 4

(3) I guess observations == IMBIE?

Response: OK

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?

Response: (1) 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 make this more clear in line 251-252.

Comment: Paragraph 3.2 (2) In legend of Fig. 4: ERA5 medium, low , high -> ocean forcing? What is applied to the ESM’s? Medium, low, high or no ocean forcing? Please explain. Ah, ok. It is explained in the figure caption. Suggest dropping ERA5 low & high as you do not refer to these simulations in the text. IMBIE “max corr“ and “uncorr.” and they grey shaded box needs further explanation.

Response: (2) We added further explanation of correlated and uncorrelated errors in the respective figure caption (Fig. 4 and Fig. 5). We explicitly mention ERA5-high and -low runs in the text (line 260-263).

Comment: Paragraph 3.2 (3) Would it be possible to add an observational dataset from 1960 onwards? That would help to judge the different models.

Response: (3) We rely here on the available IMBIE data set.

Comment: Paragraph 3.2 (4) “fairly well”, “agree closely”, “strong agreement (Line 237)”: How do you specify agreement? Though the SLE is an integrated scalar value, I am curious if you can say a bit more about spatial patterns? Without giving any number like e.g. RMS it is hard to follow. In the IMBIE period it seems that all models behave well, I cannot see that ERA5 behaves much better than some of the models from the ESM-init ensemble (like MPI-ESM1-2HR or IPSL-CM6A-LR).

Response: (4) We have extended this analysis to provide more detail and further specified ‘good agreement’ and ‘match well’ (Sect. 3.2).

Comment: Paragraph 3.2 (5) Your interpretation with interannual and interdecadal variability is very speculative as it is not shown by any numbers or figures.

Response: (5) We discuss this aspect in Sect. 2.3.2 and added relevant references (line 169-170).

Comment: Line 233: Maybe add after “… set to 0” that the forcing is then representative for the 1960-1989 period (reference period), if so. I am curious to see a figure. I would have expected that some models have a negative SLE in 2100 (mass gain) to reach the 1960-1989 state.

Response: We added a figure to visualize the mass loss in the control projections (see Fig. 6)

Comment: Line 237: drop “interannual variability”. That was not shown and I don’t see the context.

Response: We think that this is relevant and decided to keep it. Furthermore we added relevant references.

Comment: Line 237: I am not fully convinced. Reaching a small model drift is a difficult task and not always the intention: E.g. ‘paleo spin-ups’ aim to reach the trend of observational period. Generally inverse/data assimilation models have a larger drift, but they match observations (at least for the date of the inversion). So, you did transient inversion and another relaxation. How does your model results ‘perform’ in 1960?

Response: The aim of our spinup is to first create an ice sheet state in balance with 1960-1990 climate with minimal drift. This is achieved in all cases. This state is assigned to year 1960. The present-day ice sheet mass loss trend is then fully dominated by response to the climate forcing 1960-2015, which is the intention. This is now described better at the end of Sect.3.2 and supported by an additional figure (Fig. 6).

Comment: Figure 5 caption: ERA5-initialization -> ERA5-init. What is the simulation ESM-own SMB?

Response: We removed ‘ESM-own SMB’.

Comment: Maybe try to use another color scheme that shows the different SSP scenarios. Its hard to follow the “linear” statement in Line 242.

Response: It is true that the individual lines are difficult to distinguish. We have carefully tried out different color schemes and line styles to best visualize our results and came to the conclusion that a drawback remains in any combination. In this figure, we want to focus the attention to the generally large spread of results and the overlap between the scenarios (visualized by the colored bars at the right side of the plot), while also highlighting the small difference between the two init-ensembles. This figure should merely give an overview, while detailed results for each simulation can be found in Tab. A1. For now, we have changed the color scheme to a more color blindness friendly palette.

Comment: Line 244-245 What is the motivation to compare rates of 2040-50 and 2090-2100 with observations?

Response: Rates of change give important information about the acceleration of mass loss and are relevant for practitioners, when it comes to planning of protective measures in coastal areas. We added an explanation (line 286-288).

Comment: Line 257: I am trying to follow. Both, ESM-init and ERA5-init are in balance due to the relaxation. If you add an anomaly, the simulated SLE of the ISM is in both cases (ESM init vs ERA5 init) rather similar, or? That's not surprising as they own the same climate forcing.

Response: We clarified this in Sect. 2.3.3.

Comment: Line 269 "… ice thickness of the initialized ice sheets …" -> initial ice thickness

Response: OK

Comment: Line 271: it is bit confusing. What is the difference between "(ESM-initialization - ERA5-initialization)" and "(ESM-initialized - corresponding ERA5-initialized) "

Response: We omitted 'corresponding' to avoid confusion.

Comment: Section 3.3.1. I acknowledge the detailed results of this chapter. But I don't see the new results and how they are related to previous studies.

Response: We removed the respective section.

Comment: Line 290 and 300: to be honest, I get a bit lost with all the different scenarios. "fully forced", "partially forced", medium/low/high ocean forcing; "SMB-only", "Retreat-only" and "No-SMB-height-feedback-experiment" vs NorESM or the fully ensemble. A table could help.

Response: We removed the respective section.

Comment: Line 310-317: That's a good analysis. But I don't see how it is related the overarching aim of the paper.

Response: We removed the respective section.

Comment: Line 342: "exceed" is not a fair comparison as you use different scenarios (CMIP5 vs CMIP6, see Payne et al (2020)).

Response: We rephrased and clarified this part (line 341- 345).

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).

Response: We clarified this in Sect. 2.3.3

Comment: Line 406: I think, another restriction is the retreat parametrization, which "overrides" the ISM dynamics.

Response: We revised this part and no longer mention 'restrictions'. We discuss the retreat parameterization in line 406 - 414.

Comment: Line 414: the grid dependency was not shown.

Response: We have included a figure showing the differences due to grid resolution (Fig. 11) and presented results from the grid sensitivity study more in-depth (line 327 – 334).

Comment: Code and data availability: Retreat masks and MAR forcing for the historically period for all ESM should be made available.

Response: We added the locations where Forcing and output are publicly available.

Reply to community comments:

CC1: 'Basal Heat Flow BC' William Colgan, 07 May 2024: This study is clearly relevant and timely for community ISMIP7 efforts. I applaud the authors for the depth to which they explore the influence of choice of surface mass balance forcing on projected sea-level contribution.

I wonder, however, if the authors might perhaps acknowledge, in even the smallest possible way, the influence of choice of geothermal heat flow as the basal boundary condition? For example, using the same CISM model, we have found initialized ice-bed temperatures to vary by 10°C, based on choice of heat flow map (https://doi.org/10.5194/tc-18-387-2024). Although the Shapiro and Ritzwoller (2004) heat flow map is in widespread use, it is a global model that is known to be poorly constrained in Greenland. It yields a different thermal state (and thus very different viscosity) for the ice-sheet than more constrained regional heat flow maps released in the past two decades.

While a summary statement such as "…uncertainty in projected sea-level rise due to climate forcing exceeds any of the other sampled uncertainties" is technically true (since this study did not sample basal BC uncertainty), the actual spread in sea-level contribution influenced by choice of ice-sheet boundary conditions – both surface and basal – is likely greater than suggested here.

With collegial encouragement and best wishes from Copenhagen.

Response: We thank William Colgan for the valuable comment on the matter of basal heat flow. It is certainly true that there is a significant spread in existing heat flux maps for the area underneath the Greenland ice sheet. While we acknowledge that the basal heat flux has an influence on the thermal state of the ice sheet and hence the viscosity of the ice, we expected that the choice of geothermal heat flow map has a negligible effect on sea-level projections in our specific setup. During spin-up the basal friction coefficients are adjusted in order to nudge the ice sheet to match the observed geometry. The bed friction thereby compensates many inaccuracies, including uncertainties in basal conditions. We mention this shortcoming of our setup in section 2.3.1. Nevertheless, we tested the influence of the geothermal heat flux on the SMB-only experiments for three selected forcing cases, by prescribing six alternative geothermal heat flux maps according to the provided reference (https://doi.org/10.5194/tc-18-387-2024) during initialization, as well as throughout the historical run and the future projection. Resulting projections of sea-level contribution are shown in Fig. 1. We find that the differences in projected sea-level contribution due to a different choice of geothermal heat flux amount to a maximum of 0.93 mm at a total of 187 mm, or 0.5% for the strongest forcing (UKESM1-0-LL ssp585). For an intermediate forcing (NorESM2-MM ssp585) the differences are maximal 0.6 mm for a total of 68 mm or 0.9 %, while for the lowest forcing (MPI-ESM1-2-HR ssp126) the differences are not more than 0.45 mm for a total of 21.4 mm or 2.1 %. We therefore conclude that it is justified to refrain from sampling the uncertainty due to the use of different geothermal heat flow maps in our ensemble. We added information about the limited influence of basal heat in our setup in line 139 – 141 and discuss this aspect in Sect. 4 (line 372 - 475).

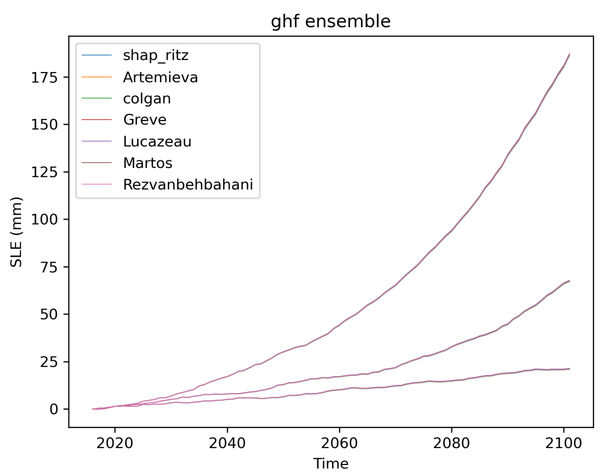

Figure 1: Projections of sea-level contribution for the SMB-only experiment relative to 2015 for three selected forcings (see text above for details). The different line colors denote simulations using different geothermal heat flux maps during initialization, with shap_ritz referring to our standard configuration. Note that differences due to geothermal heat flux are small and lines are mostly overlapping.