Dear Benoît Urruty and co-authors,

First of all, I want to thank you and your co-authors for revising your manuscript according to the review request. Both reviewers appreciate the efforts you took in adopting their comments. Yet their general assessments diverge. Reviewer #2 requires further clarifications on the experimental setup (e.g. the SMB correction), discussion of specific setup choices, as well as an improvement in the usage of terminology with regard to steady and equilibrium states. Moreover, an explanation is requested for a possible contradiction with the partner study. In my view, these comments are constructive and will serve to make the modelling strategies more easy to follow as well as to better convey nuances in modelling choices as well as in terminology.

In summary, I continue to consider your article draft as highly interesting for publication in The Cryopshere and I therefore invite you to address these new review comments in a second revision round.

Best,

Johannes Fürst

Dear Johannes Fürst,

We want to thank you again for taking on our review process and for the handling of our manuscript up to this point.

We have made considerable efforts to revise the manuscript to address the points outlined above and to address the comments of the reviewers. This has included substantial rewriting and restructuring of some sections. Please see the response to reviewers included below.

In particular we have provided a new detailed Methods section of the manuscript in this latest version, which summarises and clarifies the experimental set-up at the beginning, and closely follows the suggestions of the reviewer. We have also added a section of the methods which clearly outlines the process to modify our mass balance field, and how these fields compare between models and to the output of regional climate models.

We have made a conscious effort to remove or amend any locations in the manuscript where there may have been confusion regarding the terminology of steady states and equilibrium.

We are unsure where you are referring to when you mention the possible contradiction with the companion paper. This is perhaps in reference to a couple of sentences at the end of the abstract as pointed out by the reviewer. We have amended these sentences for clarity.

We want to thank you and the reviewers again for their constructive comments, it has greatly improved the manuscript over the previous versions, and we appreciate the time taken to complete the reviews.

At this point we note that there has been a minor change to the order of the three first authors as it appears in the author list of the paper for this re-submission.

With best wishes,

Emily Hill, Benoît Urruty, Ronja Reese and co-authors

Reviewer #1:

The revised manuscript is in very good shape, and I believe it will be ready for publication after some small revisions as suggested below. The authors may also carefully read the manuscript again, as it is clear that there were several small grammatical mistakes.

Many thanks for reviewing our manuscript. Based on the comments by the second reviewer, we have made substantial changes to the manuscript and during this process we carefully checked for grammatical mistakes.

L35: a necessary conditions => a necessary condition

On suggestion from reviewer #1, the phrase “necessary condition” has been removed from the manuscript.

L59: grounding line retreat => grounding-line retreat

Done

L91: if the => as to whether the

Done

L179: As aforementioned, => We note again that

This sentence has now been removed from the manuscript.

L183: conclude on => learn about

Done

L197: would be wishful => is desirable

Done

L369: start slightly retreated of those => start in slightly retreated positions compared to those

Done

L446: 480 year => 480-year

This sentence has now been removed from the manuscript.

L487: aforementioned => mentioned earlier

We were not exactly sure where this was referring to, “aforementioned” (on previous line 455) has now been removed from the manuscript.

Reviewer #2: Review of a manuscript "The stability of present-day Antarctic grounding lines Part A: No indication of marine ice sheet instability in the current geometry" by Urruty et al. This is a revised version of an earlier submitted manuscript. I thank the authors for engaging with my comments. Some of them have been answered. However, there are several issues that still have to be resolved and clarified on both conceptual and presentation levels before the manuscript can be published. In general, the manuscript is still written in terms of "stable/unstable" ice-sheet geometry irrespective whether the climate conditions (surface accumulation and submarine melting) are changing with time or not; and the general thrust of the paper is still focused on the ice-sheet geometry with unclear and somewhat confusing description what has been done to the climate conditions and modifications to them. To this reader, there is a disconnect between what has been done in the study and how the study steps and the results have been interpreted and described. What follows below, first addresses conceptual aspects, and then the presentation aspects (in some places they're mixed together).

Many thanks for reviewing our manuscript, we appreciate the effort the reviewer has put into this that we think have helped us to improve the manuscript substantially. We have addressed all comments below and revised the manuscript based on them. We want to point out two more general comments:

(1) We have the impression that the reviewer thinks of "Marine Ice Sheet Instability" in a mathematically rigorous way that is only applicable to steady states and has in that sense no application to the real Antarctic ice sheet (as the reviewer mentions, this can never be in a steady state). However, importantly, the concept has in the past been used in the glaciological community in the sense of "having a self-enhancing, irreversible grounding line retreat due to a positive feedback mechanism related to the ice dynamics" (e.g., IPCC AR6 WG1 report, Pattyn & Morlighem 2020). Since this is how "MISI" is usually understood and used in publications and stakeholder dialogue, we decided to keep the terminology "MISI" to make the context of our study clear. We added a sentence to explain this in the introduction (lines 36-38). We however only use "stable/unstable" when referring to a steady state, otherwise we use "reversible/irreversible" to not propagate this any further.

(2) While the Antarctic Ice Sheet is not in a steady state as we construct some of our model configurations to be, we argue that our results can still inform about the state of the current, realistic Antarctic Ice Sheet: (a) as any numerical study, our experiments cannot perfectly reproduce the real Antarctic Ice Sheet. However, we want to stress that we here performed several experiments with a range of ice sheet models that we carefully initialise, which makes our results more robust than studies relying on only one numerical model. (b) we here show that stable, steady states can exist in the geometry of the current ice sheet, i.e., for the current grounding line positions and ice thickness distribution, with the modified surface mass balance fields. It hence means that at the current grounding line positions of the Antarctic Ice Sheet, a positive feedback mechanism causing self-sustaining retreat is not automatically at play (if it was, the steady states we construct must have been unstable). We did further experiments where the grounding lines are drifting through time, and no modification to the mass balance field was applied, and found no self-reinforcing retreat in any of these additional simulations. Taken together, our experiments indicate that in the current geometry no self-reinforcing retreat occurs (note that we do not claim this to be an implication, however, this is our best understanding until we are able to construct a state in the current geometry that shows self-reinforcing, positive grounding line retreat or "MISI" as defined in 1 and claimed in previous studies).

In very broad terms, my understanding of what has been done is the following:

1. The observed present-day geometry of the Antarctic Ice Sheet is assumed to be a steady state one or close to a steady state. This geometry and observed velocities are used in inversion procedures in Elmer/Ice and Úa to construct basal conditions.
2. The climate conditions (the RACMO surface accumulation and PICO submarine melting)[1] are modified in such a way that when the constructed field is used in the mass balance equation, the resulting changes of the ice thickness with time h_t are fairly small or close to zero (depending on the model). Let's call this created field A_{ar} – "alternative reality" climate conditions.
3. In A_{ar}, the submarine melting component is modified again by applying "small perturbations" for 20 years; after that these "small perturbations" are removed and models run for another 80 years.
4. The temporal evolution of the ice flux through the grounding line and the grounding line position simulated during these 100 years are used to establish whether the constructed steady state is stable or unstable.

If this is incorrect then clearly the manuscript does not convey what steps have been taken and it needs to be completely rewritten. Assuming that it is indeed what has been done, the text needs to modified to accurately describe these steps, the assumptions that were made at the outset, and interpretations of the results.

We thank the reviewer for clearly itemising the steps that we have carried out in the methodology of this paper. As you have written them, they are indeed correct. We appreciate that this could be better explained in the manuscript. To address this comment we have made substantial modifications to the Introduction and the Methods section. The changes that have been made are summarised below:

1. We have shortened and streamlined the Introduction to make the approach clearer.
2. At the beginning of the Methods section, we have provided a detailed explanation and justification for the methodological approaches chosen in this study. Namely, we perform two sets of perturbation experiments to 1) steady states, 2) transient states.
3. At the beginning of the Methods, we also provide a summary of how each set of experiments were performed, which in the steady state case, closely follow the bullet points 1-4 outlined in the review. This means that it is clear to the reader early on that a modification is made to the mass balance to create a steady state
4. We have substantially restructured the entire Methods section to provide a clear and logical flow and to clearly distinguish between the two types of model states for which we perform our numerical experiments. It is now split up so that "steady states" and "transient states" are presented separately.
5. As part of restructuring the Methods we have included a dedicated subsection on the modification made to the mass balance for Elmer/Ice and Úa, which both show how the corrected mass balance fields were created but also provides discussion on how these fields look with respect to present day, i.e. RACMO.

One thing to note on your description for Step 1, it is correct that the commonly adopted inversion methodology used in several ice sheet models is assuming that the geometry is in steady state at that snapshot in time. We do state in the manuscript that "Both models also

[1] I appreciate that there are inconsistencies between the observed geometry and ice velocities, and that RACMO and PICO are not the actual surface accumulation and submarine melting, and the need for what the authors call the "relaxation", which also contributes to the field A_{ar}.

apply an additional penalty on the rates of thickness change, to reduce nonphysical ice flux divergence anomalies" (Line 135 in the revised manuscript). However, it is important to note that using dh/dt in the inversion is designed to avoid unrealistically large flux divergences and is not a strong enough constraint to actually bring our models into steady state, because there is a notable drift in our models when we run them forward-in-time after the inversion. We have now clarified this in the Methods section, stating "…the penalty we apply to rates of thickness change, is not sufficient to bring the model into a steady-state, and when the model is run forward-in-time it diverges away from the present-day geometry and ice velocity" (Line 144 in the revised manuscript) as justification for our necessary modification to the mass balance in Step 2.

Among these steps, the central one is step 2 – the construction of the field A_{ar}. This field is such that, if it is held constant in time, the present-day ice-sheet geometry is in (or close to) a steady state with respect to it (at this point, it does not matter whether this steady state a stable or unstable).

This is a good explanation of what we have achieved in Step 2 of our methodology. We have now incorporated this wording into Step 2 in our summary at the beginning of our revised Methods section on lines 86-87 (in the revised manuscript).

Observations show that the present-day surface elevation, ice thickness and the grounding line positions change with time, hence, the present-day geometry is not in steady state with respect to the present climate conditions. If it were, then according to the mass balance $\nabla \cdot (vh) = A_{pd}$, and $h_t = 0$, where v is the ice velocity, and A_{pd} is the present-day ice-sheet mass balance. Because the observed $h_t \neq 0$, the present-day conditions differ from the constructed A_{ar}. Consequently, the authors need to a) articulate this point that they have constructed an "alternative reality" climate conditions; and that each model has its own "alternative reality" climate (as figs. S14-S15 indicate); b) clearly describe how A_{ar} have been constructed for each model; and preferably c) discuss how different they are from the present-day climate. Although, supplemental figures S14 and S15 show the mass-balance correction terms for Elmer/Ice and Úa, they do not address point c). These plots do not show similar fields for PISM; the colors in panels showing Úa results are oversaturated suggesting that these corrections are much larger than the colorbar limits of ±2 m/yr.

We have now created a dedicated section of the Methods to present how and why we went about creating our modified mass balance field. Details are included in the list below.

1. We have used the information provided above to explain why we needed to create our modified mass balance field and that each model has a different modified mass balance field (see Section 2.2.2 Mass balance modification in the Methods)
2. We have included further explanation as to how this was done for Elmer/Ice and Úa. There is a summary in the main text and further information included in the respective appendices. There is no modification made to the mass balance field for PISM as we analyse a state in PISM that includes the present-day trend in mass losses. We have restructured the Methods to make clear the differences between Elmer/Ice / Úa and PISM in terms of the initial ice-sheet model states that are created (former two models are in steady state, PISM uses RACMO for the surface mass balance).
3. In subsection 2.2.2 we provide additional explanation and discussion of the modified mass balance fields and we have included new figures in the Supplement (Figures S1 and S2). We have also modified the colourbar limits and

plotted them on a log scale to account for the higher modification values in Úa compared with Elmer/Ice.

We prefer not to refer to this modified mass balance field as an “alternative reality” as we believe this is misleading, as we don’t want the whole ice sheet model set-up to be considered an alternative reality, as the geometry is very close to the real one. Instead, we refer to it as our modified mass balance field, because this accurately describes what we have done; we have made a modification to the mass balanced field to bring the models into a steady state.

With regards to point b), the current description (lines 156-177) is not clear, especially with both RACMO, which is the surface accumulation/ablation, and PICO which is submarine melting denoted by the same variable b. It would be beneficial for the manuscript to have figures showing A_{ar} for each model in the main text and either absolute or relative differences between A_{ar} and RACMO and PICO fields (essentially h_t terms). Although the authors point out that the imposed mass-balance corrections have small magnitudes and are a small fraction of the total present-day mass balance (line 458), it is not only their total value, but the spatial distributions of those corrections that matters. This is because, as fig. S14 shows, these corrections are both positive and negative, and when integrated over the whole ice sheet, their contributions cancel each other. So it seems to me that the constructed A_{ar} is indeed quite “alternative reality” climate.

In the first round of revisions the Editor suggested we used the mass balance notation from Cogley’s ‘Glossary of glacier mass balance’ for the climatic surface mass balance and then use RACMO/PICO as subscripts. However, now that we have removed inline formulas (see following comment below) we hope that it is not too confusing to leave this notation as it is.

We have however modified the figures to now include all three terms in Equation 1 (Figures S1 and S2), the modified mass balance fields we use, the modification applied to obtain those fields, and the present-day climate field (RACMO+PICO). We have also added discussion to the Methods as to how the modified mass balance fields compare to present-day climatic conditions.

The description of how perturbations for PICO fields constructed for the step 3 (lines 257-275) is confusing, especially with inline formulas and too many terms having very similar notation b_{PICO}. Throughout the text these perturbations are called “small”, however the changes of the ocean temperature 5°C or even 1°C are hardly could be described as “small”. The extra energy supplied to the ice shelves due to such an increase in the ocean temperature is $\Delta Q = C_p m \Delta T$, where C_p is the sea-water heat capacity. The change in the air temperature that would correspond to this amount of extra heat ΔQ would be by about four times larger (assuming the same unit mass of air and water). This is because the air heat capacity is about four times smaller than the sea-water heat capacity. Thus, the corresponding air-temperature changes would be of the order of 4-20°C. This is well above any high-end projections of the climate warming. I appreciate that the magnitudes of perturbations have to be large enough to cause the grounding lines to move, and that they were also applied for a short time of 20 years, but still, they have to be physically reasonable. Some re-wording or clarifications for these values are needed. Perhaps, it might be better to cast these applied perturbations in terms of enhanced submarine melt-rates expressed in m/yr, rather than the ocean temperature changes. The need for such large changes in the ocean temperature suggests to me either low sensitivity of the models to changes in the submarine melting (if the changes in melt-rates that correspond to these changes in the ocean temperatures are large) or issues with PICO parameterizations. At least something needs to be said about the “smallness” of these perturbations.

We have modified the perturbations section of the Methods to remove the inline formulas and updated the notation to follow equations (1) and (2) that are included below.

We do not agree that the perturbations have to be physically reasonable, and we never state so in our manuscript. The point is not how realistic the perturbation is, but as you state, just that the perturbation is sufficient to create a small deviation in the position of the grounding line. We could have chosen a number of different model parameters to perturb in order to achieve this. It is of course true that there could be some reasons in the models that a 5°C perturbation is needed to see a significant grounding line retreat, such as:

- mesh resolution at the grounding line,
- not applying adaptive remeshing at the grounding line
- melt not applied to cells crossing the grounding line
- PICO parameterization, where in the AMSE in particular it does not capture high melt at the grounding line, so higher temperatures are needed

However, we do not feel this discussion is needed in the text as it would make this section of the Methods too long. However, we have carefully been through the manuscript to make sure that all references to small perturbation are referring to a small retreat of the grounding line position and not to the forcing applied to create this perturbation. This includes revising sentences in the conclusions and abstract. We have also added a sentence to the Methods (Section 2.4) that reads “While +5 °C appears to be unrealistically high magnitude change, we want to stress that this perturbation is not designed to be realistic and is only applied over a few decades. Instead, we can think of our small perturbation, as a small movement of the grounding line away from its current position, on the order of a few grid squares or ice thicknesses at the grounding line.” (Lines 242-243 in the revised manuscript).

It is also not clear how these perturbations have been applied. I suspect that for the ice shelves the mass balance was

$$h_t + \nabla \cdot (vh) = A_{ar} - \Delta b_{PICO}, \qquad (1)$$

and for the grounded parts it was

$$h_t + \nabla \cdot (vh) = A_{ar}. \qquad (2)$$

However, this is not clear from the description.

We have now made it clear in the manuscript how the perturbations were applied following the equations outlined above.

With regards to the presentation and description of the results (step 4), I am not sure that the detailed regional analysis adds value to the main results of the study which are a) there exist “alternative reality” climate conditions specific to each model, for which there are steady-state modeled geometries that are close to the present-day ice-sheet geometry, and b) these modeled steady states appear to be stable. I leave it up to the authors to decide whether to keep or remove it, but it’d be easier to read the manuscript if these parts of the text would be expressed more succinctly.

We appreciate the comments in relation to the regional analysis, but we would like to keep it in, because we believe that it is important to stress that the results we find are both for the general Antarctic wide signal but also for any individual regions in Antarctica (including those where MISI is often discussed, e.g. Amundsen Sea). This makes it clear to the reader that there are no compensating effects in the Antarctic wide result, i.e., the behaviour of one region does not cancel out the behaviour in another. However, we have carefully been through the results sections and made efforts to make it more succinct and concise.

We have also made changes to the Discussion section to present the results of our experiments more clearly and succinctly. The key message here is that we have performed a number of experiments both on steady and unsteady states and in none of these experiments did we find any indication of self-sustained, unstable retreat. We can take this to mean that it is unlikely that there is a positive feedback mechanism (related to MISI) is at play right now in Antarctica.

Moving on the presentational aspects, it appears that there is still a confusion between the effects of time-variant and steady-state climates on the grounding lines. The abstract starts with the sentence "Theoretical and numerical work has firmly established that grounding lines of marine-type ice sheets can enter phases of irreversible advance and retreat driven by the marine ice sheet instability (MISI). " However, theoretical and numerical work has firmly established that grounding line can equally not enter phases of irreversible advance and retreat (e.i., be stable to small perturbations). Without this second sentence, the first sentence gives an impression that the irreversibility is the only option for the grounding lines. The last two sentences ". . . his suggests that if the currently observed mass imbalance (external climate forcing) were to be removed, the grounding-line retreat would likely stop. However, under present-day climate forcing, further grounding-line retreat is expected, and our accompanying paper (Part B, Reese et al., 2022) shows that this could eventually lead to a collapse of some marine regions of West Antarctica." indicate that the authors view only the mass imbalance as the climate forcing, and not the temporal variability of the surface accumulation/ablation and submarine melting. Were the authors to use their constructed "alternative reality" climate conditions A_{ar} and apply temporal variations to them (even without long-term trends), they might find the grounding line behavior significantly different from the one they obtained in this study. It is unclear what is the basis of the statement that "under present-day climate forcing, further grounding-line retreat is expected". What is the reason for such an expectation?

We had hoped that the use of the word "can" in the first sentence of the abstract already suggested that grounding lines equally "cannot" enter phases of irreversible retreat, but we appreciate that this was not entirely clear. We have now modified this sentence to read "Theoretical and numerical work has shown that under certain circumstances grounding lines of marine-type ice sheets can enter phases of irreversible advance and retreat driven by the marine ice sheet instability (MISI)."

We appreciate the confusion with the statement about "observed mass imbalance" and that we have not included any assessment of the temporal variability of surface accumulation/ablation and submarine melting on the stability of the grounding lines. For simplicity we have simply removed the second to last sentence from the abstract as it was not necessary. We have also modified the final sentence to make a clearer statement about our accompanying paper and to remove the word expected which we agree was unfounded. It now reads as "However, our accompanying paper (Part B, Reese et al., 2022) shows that if the grounding-lines retreat further inland, under present-day climate forcing, it may lead to the eventual collapse of some marine regions of West Antarctica."

Despite changes made to the introduction section, it continues to rely on the bed slope as an indicator of stability. The authors added a statement that “the retrograde sloping bed is a necessary conditions”. This statement is incorrect. None of the stability conditions derived by Haseloff and Sergienko (2022) and Sergienko and Wingham (2019, 2022) has a necessary condition for an unstable steady state configuration to have a retrograde sloping bed. If the authors disagree, they need to support their statement with mathematical derivations demostrating that these stability conditions indeed have such a necessary condition. The authors’ statement about the retrograde slopes is a widely held misconception that stems from the stability condition for a configuration with very specific conditions derived by Schoof (2012). These conditions are the absence of the lateral confinement, very smooth beds with negligible bed slopes and the smallness of the accumulation/ablation rate at the grounding lines. Only if these conditions are simultaneously satisfied than indeed the stability condition is reduced to a condition on the sign of the bed slope (Haseloff and Sergienko, 2022; Sergienko and Wingham, 2022). However, there are no locations in Antarctica or Greenland for which all these conditions are satisfied. In addition to that, in the presence of feedbacks between the ice sheet and climate conditions there are no general stability conditions that can be related to steady-state properties (Sergienko, 2022). There is no need to promote this misconception that the retrograde beds are the necessary condition for instability; so the rest of the introduction needs to be modified accordingly.

We are grateful for your explanation of the relevant studies and for the clarification on our statements about the retrograde bed slope as a necessary condition. We of course do not want to perpetrate the misconception that bed slope is a necessary condition for instability. We have now revised the introduction substantially compared to the previous version and we no longer include the paragraphs that refer to marine basins of the ice sheet and there is no longer any mention that the retrograde bed slope is a necessary condition for MISI to occur.

The provided reason why the present-day Antarctic Ice Sheet is not in a steady state (lines 72-73) is incorrect as well. The ice sheet is not in a steady state not because of its response to varying climate forcing is long-term, but because the climate forcing itself varies in time, and because this climate forcing is never in steady state. In their experiments, the authors keep their constructed “alternative reality” climate conditions constant to maintain steady-state configurations. This never happens with the real climate conditions, they always vary on a wide range of temporal scales. It appears that the authors confuse steady states with equilibrium states. In the latter ones, the climate can vary with time, and if ice sheets vary in such a way that their mass gains balance their mass losses (both controlled by these time-varying climate conditions), then the ice sheets are in equilibrium with their climate. However, such an equilibrium is not a steady state, because the climate conditions vary with time. Weertman’s (1974) analysis, Schoof’s (2012), Haseloff and Sergienko (2022) and Sergienko and Wingham (2022) stability conditions apply to steady states only, and are not valid to equilibrium states. Currently, there is no theoretical analysis of the ice sheets which are in equilibrium with their time-varying climate conditions.

We have made substantial modifications to the introduction in this latest version of the manuscript such that the sentence that was previously on lines 72-73 has now been removed. We have also made sure that there are no longer any confusing statements with regards to steady-state and equilibrium. We refer to steady state for the initial states created in Elmer/Ice and Úa, where dh/dt=0 (or numerically close). We only use the term equilibrium to describe the initialisation of PISM and define this as the integrated ice sheet mass balance is close to zero.

In the discussion section, comparisons with the results of other studies appear as attempts to reconcile the conclusion of this study that the grounding line is stable with conclusions of those studies that it is unstable. Because the climate conditions used in this study are vastly different from climate conditions in other studies comparison of the results is similar to comparing apples and oranges.

We appreciate the concerns about making comparisons between studies that use different approaches to draw their conclusions. It is true of course that the climate conditions used in this study may be different to others. A few thoughts on this: 1) we refer to papers that have “suggested” that MISI might be underway, using observations of grounding line retreat and bed topography alone, and do not consider the changing climate. 2) we compare our results to papers that have run modelling simulations, but importantly some of these studies also keep climate conditions constant through time, and secondly, do not perform a numerical stability analysis, i.e., applying and reversing a small amplitude perturbation. Therefore, while the climate conditions are different, those studies were not able to conclude on the stability of the current grounding lines. Here, we have shown that we can’t assume that the current grounding lines are retreating due to a positive feedback mechanism related to MISI, and that (albeit using our modified mass balance) we can find stable grounding line positions in the current geometry of the ice sheet. Given that this is the key conclusion of our work, we feel these comparisons to previous studies are necessary. However, we have looked back through the Discussion and made modifications to the text to make the comparisons to previous studies clearer.

- We have removed the sentence “This is also supported by observations that show grounding-line retreat to have recently stagnated, suggesting the current position is indeed stable (Konrad et al., 2018).” As it was not necessary.
- We have made the sentence in which we compare the results of the companion paper to future modelling experiments at Thwaites Glacier clearer by stating that they applied increases in ocean forcing, but kept present-day surface mass balance conditions fixed through time. This sentence now reads “Several modelling studies have also shown that once the grounding lines retreat further inland under future increases in ocean forcing (and in the absence of any increases in surface accumulation/ablation from present day), it is possible that they will enter phrases of accelerated retreat (Joughin et al., 2014; Seroussi et al., 2017)”

A somewhat side note is that it is our impression that it is rare that any two modelling studies (with the exception of model intercomparison exercises) use the exact same climate conditions, and therefore comparing results between studies would become near on impossible. In our opinion, the comparison to previous studies both highlights similarities and puts our results in a wider scientific context, with the purpose of advancing scientific knowledge.

In summary: the manuscript needs a very clear description what has been done in the study; description of the constructed climate conditions and discussion how different they are from the present-day ones, and not only in the integral magnitude expressed in Gt/yr, but the spatial patterns as well; clarifications of the concepts of steady states and their stability that reflect the present level of knowledge; and description of the results in the context of the design of the study.

Following the suggestions of the reviewer, in summary, in this latest version of the manuscript we have:

- Provided a clear description of the methodology and what has been done at the beginning of the methods.
- Included a dedicated section of the methods to how the mass balance fields ("constructed climate conditions") were created for each model and how they differ from one another and from the present-day climate conditions. We have focused on the spatial patterns, have provided new figures and removed the focus on the integral magnitude.
- We have removed any misconceptions around steady states and equilibrium states from the manuscript.
- We have left the regional analysis of our results as they were, but we have reframed the key messages/results presented in the abstract, discussion, and conclusions section to better reflect our key results in the context of the design of the study.

I would like to reiterate that the study has produced interesting results and it would be a pity if the manuscript describing them would misinterpret and misrepresent them.

We thank the reviewer for their interest in our work and for reviewing the manuscript. The reviewer's comments have greatly helped to improve the clarity of the manuscript over the previous versions.

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