Response to Anonymous Referee ♯1

We thank Referee ♯1 for a number of detailed review that significantly helped us to improve the quality of our submission. Our full response is as follows.

In this paper, Saito et al. examine and revisit the SeaRISE experiments for the Green- land ice sheet to identify possible sources in the spread of results. While SeaRISE is a multimodel as well as multiparametric ensemble analysis, the authors try to limit the analysis to a multiparametric one, which enables the identify in a coherent way differences in the spread. The main parameters that are tested relate to different ways on how to parameterize basal sliding (sliding at sub-freezing temperatures), initialization of the ice sheet, mass balance parametrization, allowing advance of the ice sheet margin, ... The authors conclude that major uncertainties (causes of large spread) are due to the initialization method and mass balance parametrization, and to a lesser extent margin migration.
These conclusions are in line with the findings of Nowicki et al; Bindschadler et al., but shed a new light on the influence of initialization (However, at the end of the manuscript this is stated otherwise). While the content of the paper is informative, there are major improvements that need to be made to make the paper more sound and readable.

Thanks a lot for positive evaluation. We took all the points you made seriously. Most of them are already included in the manuscript. Revision related to the additinal experiment will be included in the submission.

First of all, the English needs to be improved. The manuscript should be carefully re-read by a native English speaker to remove small errors and to improve the flow.

English will be checked before the submission of the revised manuscript.

Secondly, the authors miss a great opportunity to properly investigate the major source of spread, i.e., initialization. As a matter of fact, the authors describe that two methods are used for initialization of ice sheet models, i.e., a (1) paleo-climatic spinup and (2) inversion methods to initialize the basal conditions of the ice sheet. They add a (3) third method, based on a temperature spinup with keeping the surface elabation fixed (which in my opinion is not a widespread initialization method but merely a spinoff of the paleo-climatic spinup). However, in the analysis, only two of these methods are evaluated (1 and 3), and of these methods, method 3 is the one that raises major questions. Initialization by spinup of the temperature field is maybe a common method, but the relaxation of the ice thickness for a short period (or no relaxation at all) may lead to spurious behaviour. I therefore wonder why the authors did not use an inversion (control) method as spinup to investigate the parameter that according to their analysis is the most sensitive. Pollard and DeConto (2012) describe a very easy implementable method to optimize basal friction coefficients for any basal sliding law by an iterative method. Convergence is reached after 50 to 100,000 years and results in a steady state surface elevation and temperature field that fits the observed surface. This way the model drift is limited, which enables to

correctly interpret the response of the ice sheet to any climate or other perturbation. Due to the ease by which the method can be applied to any ice sheet model, I urge the authors to implement this method to improve their analysis.

Thanks a lot for your suggestion.

This comment may relate to a comment by the second referee, "The main problem with the presentation of the results is that they over-claim their ability to explain the reasons for the SeaRISE spread." In the present paper, actually we do not intent to identify a definite reason for the SeaRISE spreads (in introduction we state that 'This paper does not intend to cover the sensitivities of all of the aspects'). Due to the original title and/or some improper phrasing in the text, we may give to the readers/referees an impression that we solved the SeaRISE spreads by comprehensive sensitivity experiments, however, we agree, not true. This is one of the reason why we skip to include an inversion experiment.

There are already three models (ISSM, AIF, CISM) in the SeaRISE Greenland experiment that use inversion methods, which are all different: they are different in not only the inversion methods but also in many other aspects (such as basal sliding formulation, surface mass balance and so on), and their results has already a dispersion as shown in Bindschadler et al. (2013), Fig. 3. For the objective of this paper, one can perform an inversion experiment using the same method of the three models, but still it may have much degrees of freedom than the method we tried in the present paper (that is, fixed topography spin-up). Such inversion of, e.g., spatial distribution of the basal sliding coefficients, depends on the boundary condition and ice flow characteristics in individual models, but these are all different among the models. It is possible that IcIES does an inversion under the IcIES configuration, which may not be easy to isolate the impact of the inversion method only because the inversion already includes influences from the other differences. Even if we use the same method of three models for inversion, the result may not give a potential to explain the spread. Using another method (e.g. simple relaxation presented by Pollard and DeConto) may further expand the spread. This is another reason why we skip to include an inversion experiment.

Actually, we already performed several inversion experiments using the method of Pollard and DeConto before the submission of this manuscript. As far as we tried, the method never succeeds to have a reasonable basal sliding coefficients under *standard* IcIES configuration. Even when we allow zero (no sliding) as the lower limit of the coefficients, the interior part is still lower by more than 400m, and the simulated volumes are worse than the results by other spin-up. In order to have a reasonable field, we need modify something from the standard configuration, such as the surface mass balance or the ice enhancement factor (like Pollard and DeConto). It means that we need one or two more 'experiment axis' for this paper, which are quite demanding and really beyond the scope of the paper, because anyway all we want to show is a potential source of the spreads. This is one more reason why we skip to include an inversion experiment.

So, we modify the tones of these phrasing down to "potential" explanations in the revised manuscript to skip a comprehensive series of sensitivity experiments. But, at the same time, even in terms of 'potential' explanations, it is very good to show 'potential' explanations of the impact of an inversion spin-up, or more precisely, that of spatially distributed basal sliding coefficients, for example. During the open discussion, we performed one optimization experiment using Pollard and DeConto, with a different enhancement factor to have ice-sheet topography reasonably close to the observation. Additional sensitivity experiments relating to this variation are also performed just for analysis. The results will be included in the main text of the revised manuscript as well as supplementary, in order to demonstrate as a potential source for the SeaRISE spread. We hope you agree and give us another time to include this additional experiments.

Some discussion should be given on features that have not been tested explicitly, such as the sensitivity to spatial resolution and the importance of marine-ice sheet instability, as a marine boundary is present for major Greenland outlet glaciers and such instabilities have been identified in other numerical model studies (Nick et al., 2012) More detailes on the advance/retreat of the ice-sheet margin need to be given. Does this pertain to a marine boundary or not? What are the conditions for advance/retreat (numerically). Is the process occurring on sub-grid level or simply when $H < 0$, then $H = 0$? How is this generally implemented in the SeaRISE models and in the model presented in this paper?

As mentioned in detailed comments below, the effect of the resolution is already investigated by Greve and Herzfeld (2013). We cited the result in the revised manuscript.

About the advance/retreat. Numerical treatments in the other SeaRISE models are described in Nowicki et al. (2013). First of all, 'None of the models taking part in the Greenland suite of experiment include ice shelves as indicated in Table A1.' ([31] of Nowicki et al., 2013). It is not mentioned explicitly, but most SeaRISE models determine the grounding line by a floating criterion (set $H = 0$ when surface falls below flotation height) or fix the grounding line through the time. So, we really agree the importance of marine-ice sheet instability of Greenland ice-sheet, SeaRISE models do not have capability enough to discuss the marine-ice sheet processes.

There are already M1 M2 M3 experiment in SeaRISE, which are called ice-shelf melting experiment. Since the SeaRISE Greenland models do not have explicit ice-shelf processes, the implementation of the 'ice-shelf melting' varies much among the models, that is one of the reason why the spread of the results are very large (larger than C1 C2 C3 spreads presented in this paper). Nowicki et al. (2013) state that: 'Thus, the current generation of Greenland whole ice sheet models is not yet able to simulate the potential response to a warming ocean, and caution is needed when interpreting the SeaRISE response to this scenario, as the ensemble mean response likely underestimates the true potential response.' With the same reason, the present paper focuses on the climate warming scenarios only, which means that impact of the margin retreat purely by the surface mass balance is discussed.

When we really including marine-ice instability processes, the problem of margin advance/retreat will become more significant than the present paper. Thanks a lot for comment. Such discussion about marine-ice sheet instability is appended in the revised manuscript.

Detailed comments

Title: experiments instead of experiment

Corrected accordingly.

p1384 L8-9: diversion is probably not the best word here. It appears at many other places in the manuscript. Preference for 'dispersion' or 'disparity'

Yes, we do agree. Replaced with 'Dispersion'.

p1386 L11: ranging from 8.5 to 142.6 cm.

Replaced accordingly.

p1387 L4-7: rephrase sentence

Rephrased as: 'There are at least ten characteristics which are different among the ice-sheet models participating in SeaRISE (see Table 2 in Bindschadler et al., 2013), and most of them have two or more variations.'

p1388, section 2.1: You should also investigate the effect of spatial resolution next to the use of different data sets, otherwise this has makes not much sense (see general remarks).

Effect of the resolution is already investigated by Greve and Herzfeld (2013). The paper is already referred in the manuscript but not with above context. We cited the paper again in the introduction to present the previous discussion of the effect of spatial resolution, as: "Effects of some of the characteristics are already argued by previous studies. Greve and Herzfeld (2013) compared 500-year future climate experiment with three different grid spacings, 20, 10 and 5 km and concluded that the sensitivities in the simulated ice sheet volume is insignificant."

p1388 top: Possible sources of spread, instead of Candidates for sources of spread.

Replaced accordingly.

p1388 L8: Isbrae

Corrected accordingly.

p1388 L10: referred to

Corrected accordingly.

p1388 L11: localized

Corrected accordingly.

p1388 L12: present a significant difference in the present-day

Corrected accordingly.

p1388 L19: have several degrees of freedom

Revised accordingly.

p1389 L16: This method is called 'inversion or optimization' method. It is not an initialization by 'tuning'. Basal friction coefficients are obtained by an optimization method (such as control methods). However, more simple approaches exist (that can be called 'by tuning', but is not preferred), such as the method presented by Pollard and DeConto (2012).

The terminology 'tuning' is just what Bindschadler et al (2013) used (see Sec. 2.2.2 in the paper). We modified the sentence to make it clearer, as 'One method is called initialization by "tuning" in Bindschadler et al. (2013), which may be better to call 'inversion' or 'optimization'. This method inverts given data fields, e.g. basal friction coefficients, to adjust present-day observation fields, e.g. surface velocity.'

p1390 L4: remove 'previous'

Removed accordingly.

p1390: The method due to Pollard and DeConto (2012) should be discussed here. Although this method has not been applied to Greenland, but to Antarctica instead, it is a general method that can be applied to any ice sheet and an inversion method that is easy to derive compared to other control methods. Furthermore, mention should be made to Morlighem et al. (2011) in which bedrock uncertainties are also taken into account in the inversion method based on mass conservation and surface velocities.

The two papers are cited here in the revision.

p1391: Treatment of advance of the ice sheet margin. Since this variable seems also to play a more or less important role in the sensitivity of the ice sheet, a more thorough description should be given on how this is implemented numerically and how models generally deal with this in the SeaRISE sample.

We have no idea how other models actually implement these treatment. A text to explain this is added at this part as, "Although detail numerical implementation are not shown in Bindschadler et al. (2013), some participants in SeaRISE describe their methods as either fixing the ice-sheet margin (calving front) or limiting the advance of ice-sheet margin (i.e., only retreat is allowed)." In addition, detail numerical implementation of IcIES are inserted in section 4.4, as "Thickness can be non-zero over all the model domain during one step of the numerical time integration, but those grids that match a floating condition are immediately cut off."

p1392 L3: Most participants adopt some form of the ...

Revised accordingly.

p1392 L7: Previous studies present ...

Revised accordingly.

p1396 L1: "Although it is important, such fine tuning is beyond the scope of the present paper." First of all, this is not fine tuning; Secondly, any form of inversion needs to be performed within the context of this paper, since the initialization phase is found to be the most sensitive parameter in the analysis. Furthermore, the types of initialization presented is probably the most biased in its nature. Therefore, at least an initialization procedure that represents as best the present-day observations of the ice sheet, should be favoured.

This text is deleted in the revision. About initialization, please see our main response above.

1397 eq. 3: what are the units of TB ? Does γ have units?

Both units are degree C. Explanation of the Eq. 3 is revised.

p1397 bottom and 1398 top: How does this method behave as a function of sudden changes in dH/dt once the spinup is done? Are the changes sudden? Are they in line with present-day observations of imbalance of the Greenland ice sheet? The performance of the initialization method should be further discussed, especailly since it is the major sensitive parameter in the analysis.

They are sudden and not in line with the observations, as already described in Bindschadler et al. (2013) (Fig. 1 and p201 right columns). Performance of the initialization is described in the text (results section).

p1398 L21: Why is the advance of the margin not allowed in these runs? Glacier speedup may also result in advance before increased melting at lower elevations reduces the ice thickness. I don't understand why the aspect of 'not advancing ice sheet' can be used as a parameter in a sensitivity study.

Because some models in SeaRISE do not allow ice sheet advancing in the future climate runs, due to their each unspecified reason. We definitely agree with your comment and that is why the IcIES original submission freely allows the advance to include the effect you mention. We speculate the reasons are (1) the 'inversion' models have no constrain how to specify the basal sliding coefficient beyond the present-day ice margins (2) it is much easier to fix the computation domain than to allow domain regeneration for complex models such as finite element models. We inserted a text not in this part but in Sect.2.4.

p1399 L24: both having an identical initial topography.

Inserted accordingly.

p1400 top: if there is an overestimation, please explain where this overestimation of the volumes is essentially situated. Is this at the margin near the large outlet glaciers?

Figures to show the difference in topography will be included in supplementary. Margin is overestimated overall the margin area except for the northwest and northeast region (as shown in Nowicki et al., 2013, Fig. 2c).

p1400 L19: shows the largest response. Moreover, I don't understand this sentence. Configuration 'O': is this the bottom curve (see my remarks on that figure)?

We are very sorry that this sentence is wrong. We correct this sentence, and revised the figure to move the O mark to the right.

p1402: Initialization method. Since the choice of method has the largest impact, some discussion is needed on the realism of the initialization methods used. See also general remark.

This part is already revised according to the comment above (p1397 bottom and 1398 top...).

p1402 L28: the the

Corrected.

p1403 L18: Ice-margin advance has a smaller impact. Is this because overall the margins are retreating and no advance is observed in the model for the future scenarios (should be advancing if basal sliding is cranked up under a relative mild climate scenario, such as C1)?

Thanks a lot. You are right. If climate scenario is strong as C2 or C3, then advance in the ice sheet margin is not significant even free-margin experiments. Discussion following your comment is inserted.

p1407 First paragraph. You should discuss why the sensitivity to initialization does not show up as primary source in the SeaRISE experiments, but does so in this paper.

The sentence in the manuscript is confusing. This part no more exists according to other modification.

p1408 L22: "Thus, a future-climate experiment initialized by fixed-topography spin-up can be considered a suitable approach for characteristic projections by an ice-sheet model." I don't agree with this statement. The analysis does not show it. The question is whether the spinup presented is adequate in the first place and explains the observations of the present-day ice sheet in terms of imbalance, velocity field and surface elevation (ice thickness).

The SeaRISE papers and the present paper all present the result by 'experiment minus control' methods (Bindschadler et al., 2013 p201). It means that, only **changes** from the present-day trend due to changes in forcings are mentioned and discussed, no matter how the present-day simulation is bad (or good). The next section which describes this point moved before this section, to be clearer.

p1410: Prospects: in view of the large sensitivity to spinup and the fact that the authors perform a very limited analysis in terms of spinup, this section needs to be re-analysed in a revised version.

Please see the main response above. We agree this paper is quite limited in terms of spin-up, but rather, that is why we propose a benchmark experiment. Since the resources are limited more or less, one model cannot cover all the variations of the models. Instead, if all the models perform a highly controlled experiment, it is easier to analyze the uncertainty due to model spin-up, within the variation of current ice-sheet model structures. This section is revised to add such discussion.

p1411 Appendix 1: Demonstration of the benchmark experiment

Inserted accordingly.

p1411 L19: SeaRISE has a similar configuration

Inserted.

p1420 Figure 1: What is the order of the curves here. I see 6 curves and 5 letters next to the figure. Is O the bottom curve? Not clear at all.

Sorry for confusing. The letter O is not along the right border but in the graph (around 250, -1.4). In the revised figures all the letters are along the border.