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Interactive comment on "SeaRISE experiment revisited: sources of spread in multi-model projections of the Greenland ice-sheet" *by* F. Saito et al.

Anonymous Referee #1

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In this paper, Saito et al. examine and revisit the SeaRISE experiments for the Greenland 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.

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Discussion Paper

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.

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.

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

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their analysis.

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?

Detailed comments

Title: experiments instead of experiment

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'

p1386 L11: ranging from 8.5 to 142.6 cm.

p1387 L4-7: rephrase sentence

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).

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

p1388 L8: Isbrae

p1388 L10: referred to

p1388 L11: localized

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

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p1388 L19: have several degrees of freedom

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).

p1390 L4: remove 'previous'

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.

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.

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

p1392 L7: Previous studies present ...

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.

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p1397 eq. 3: what are the units of T_B? Does γ have units?

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.

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.

p1399 L24: both having an identical initial topography.

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?

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

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.

p1402 L28: the the

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)?

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.

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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 obser-vatiojns of the present-day ice sheet in terms of imbalance, velocity field and surface elevation (ice thickness).

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.

p1411 Appendix 1: Demonstration of the benchmark experiment

p1411 L19: SeaRISE has a similar configuration

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.'

Interactive comment on The Cryosphere Discuss., 9, 1383, 2015.