The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-214-RC2, 2018

The Cryosphere Discussions

Interactive comment on "Modeling the response of Northwest Greenland to enhanced ocean thermal forcing and subglacial discharge" *by* Mathieu Morlighem et al.

Anonymous Referee #2

Received and published: 20 December 2018

General comments:

The authors explore the sensitivity of Northwest Greenland's marine-terminating glaciers to decadal-scale increases in thermal forcing and subglacial discharge. Using the Ice Sheet System Model (ISSM), they run an ensemble of 21st century experiments with thermal forcing increasing by up to 3 deg C, and subglacial discharge increasing up to a factor of 10. The model uses two parameterizations that determine the terminus location: one for calving, driven by tensile stresses, and the other for undercutting, driven by thermal forcing and subglacial discharge. It makes innovative use of ECCO ocean output, along with new bed topography data from NASA. The authors find a wide

Printer-friendly version

Discussion paper

range of glacier responses, with some glaciers sensitive to small increases in thermal forcing, and others quite stable. They argue that bed topography controls the rate and magnitude of retreat. The paper is clearly structured. It places the problem in scientific context, lays out methods and parameterizations, quantifies the results, draws general conclusions, and discusses model limitations. The experiments are a significant step toward Greenland-wide projections of the evolution of Greenland's marine outlet glaciers. However, some sections are written in a cursory way without enough details and justification. In particular, the paper seems to rely on some implicit assumptions that are not fully explained and defended, thus casting doubt on the validity of the model calibration. Although the study is timely and important, the methodology and description should be improved, as described below.

Specific comments:

First, I will restate what seem to be the underlying assumptions in Section 2: The terminus location of marine-terminating glaciers (at least in Northwest Greenland) is determined mainly by (1) mass transport; (2) undercutting driven by thermal forcing (TF) and subglacial discharge, as quantified by Eq. 2; and (3) calving proportional to ice velocity and tensile stress, as described by Eq. 3. The steady-state terminus location is determined by a balance between (1), which advances the front, and (2) and (3), which drive frontal retreat. Processes (2) and (3) are largely independent of each other. Marine glacier retreat of the past decade can be attributed primarily to increased thermal forcing and undercutting.

One way to test the validity of these assumptions would be to calibrate the model by fitting simulated termini to observed locations prior to retreat. The model could be initialized using observed or model-derived values of velocity, tensile stress, runoff-derived discharge, and ocean thermal forcing, appropriate for a period before the recent retreat (say, late 20th century) when the termini were relatively stable. Of these fields, TF might be the least certain (as suggested on p. 7, l. 2), so one approach to initialization would be to invert for TF based on pre-retreat terminus locations. This would give a baseline TF to which anomalies would be added.

In this study, however, there is no calibration based on pre-retreat termini. If I understand Section 2 correctly, the model is initialized to 2007 geometry and then run forward with a linear sliding law and RACMO-derived runoff, along with the undercutting and calving parameterizations of eq. 2 and 3. The calving parameter sigma_max is adjusted in each basin to match the observed retreat of the past decade.

This approach left me wondering how much of the simulated decadal-scale retreat is associated with recent increases in TF (as shown in Fig. 2b for ECCO). Part of the retreat could be a model transient that would occur without increasing TF, because of biases in SMB, basal friction, or other factors. Based on information given in the paper, I don't know how to make this judgment, and to be confident that eq. 2 and 3 capture the essential physics (albeit with uncertainty in empirical parameters).

If it is not feasible to calibrate the model based on pre-retreat terminus locations, I would ask the modelers to describe the difficulties and explain why their approach is preferred.

Comments with page and line references follow below.

p. 1: The abstract is longer than necessary. Some of the details and elaborations could be left out (for example, the sentence beginning "While these parameterizations remain approximations. . .").

p. 2, l. 11: The last sentence of this paragraph might fit better at the end of the Introduction section.

p. 3, l. 10: In this paragraph, please state the ISSM grid resolution (or range of resolutions).

p. 3, l. 17: It would be helpful to see an equation for the Budd sliding parameterization, along with the chosen parameter values (such as a sliding proportionality constant C). Using this parameterization, how close is the fit of simulated glacier velocities to observed velocities?

p. 3, l. 23: Since undercutting is closely related to processes that might be classified as calving, it would be helpful to state that calving and undercutting are considered to be independent processes for purposes of the paper.

p. 3, l. 29: For readers not familiar with the Rignot et al. paper, please describe the motivation for choosing this particular functional form for undercutting, and these parameter values. For example, why is the B term needed? Are there theoretical reasons to expect alpha and beta to have roughly these values, or are they strictly empirical? Why the dependence on h?

p. 4, Fig. 1: This is a very useful and visually attractive figure.

p. 5, l. 1: Please give equations showing how TF is computed.

p. 5, ll. 9ff: This is a helpful explanation of the interplay between bed topography and thermal forcing.

p. 5, Fig. 2: This is another useful figure, which helped me visualize the model forcing, but the caption is not very informative. For instance, the caption for Fig 2a could state that the initial calving front is at x = 0 km and the fjord mouth is at x = 80 km (if I'm interpreting it correctly). For Fig. 2b, please give the source of the data.

p. 5, l. 17: "repeat TF of year 2016 until the end of the simulation." I found this confusing. Do you mean that TF from 2016 is the baseline to which the TF anomaly is added?

p. 6, l. 9: Please see the above comments on model calibration. There is no explanation here of why sigma_max is the specific parameter chosen for calibration, or of why calibration to the observed retreat is preferable to calibration to pre-retreat terminus location.

p. 6, l. 10: Landsat-derived ice front retreat is mentioned here for the first time. I suggest describing the observed retreat, perhaps with a reference to the left column of numbers in Table 1, as part of the background discussion.

p. 6, l. 16: Can you say approximately how many CMIP5 models were used to compute this average anomaly, and what is the spread among models?

p. 6, l. 17: I'm not sure the 2-degree Paris target is entirely relevant here, given that the target might be exceeded, and we want to know the consequences of missing the target. It would be more appropriate to choose an upper limit based on the CMIP5 spread.

p. 6, l. 24: Can you say why you increase the TF anomaly instantly, instead of phasing it in linearly as might be more realistic? Similarly for SMB/discharge.

p. 6, l. 26: It is unclear exactly what is being repeated here.

p. 7, ll. 3-5: Referring again to the comments above, I'm wondering if a bias correction to the ECCO data would be more defensible than adjusting sigma_max. Then you would more likely be capturing the recent retreat for the right reason.

p. 8, l. 2: How informative is it that ice front retreat is in good agreement with observations when sigma_max is tuned? Does this suggest that there is something fundamentally "right" about the parameterizations, or does it simply reflect high sensitivity to modest changes in sigma_max?

p. 8, l. 9: The text states that the model overestimates the recent retreat of Kakivfaat Sermiat, but Table 1 shows a small underestimate.

p. 10, Fig. 5: This figure nicely illustrates that the TF anomaly is the primary driver of retreat. However, I wondering how the shape of the figure would be different if anomalies were ramped up gradually. Also, I suggest that the caption briefly state what is left out of the fixed-front simulation, such as SMB changes.

p. 11, l. 17: I couldn't find a description of the control experiment in which the ice front is held fixed.

p. 12, ll. 3ff: The paper makes a strong case for the important of calving dynamics. At the same time, it does not quantify mass changes due to a more negative SMB, or discuss possible feedbacks of SMB on dynamics. For instance, could a decreasing SMB potentially cause much more mass loss than dynamic retreat? Could SMB-driven thinning significantly modify the calving dynamics (e.g., through reduced tensile stresses)? I realize that SMB changes are beyond the scope of the modeling study, but it would be helpful to talk about them in Section 4.

p. 12, l. 7: This statement about models with fixed calving fronts seems too general. For example, consider a model without a physically based calving law, in which calving is simply prescribed at the present-day CF. Suppose the model is forced with increasingly negative SMB. This could result in significant thinning, and perhaps ungrounding, of ice all the way to the CF, without necessarily moving the CF. This isn't to say that moving boundaries aren't an improvement, but just to acknowledge that models without moving boundaries may still be able to make useful projections, which might not be overly conservative (especially if SMB dominates the mass balance).

p. 12, l. 15: I agree that this is an interesting result, which might not have been guessed ahead of time. Given the result, it would be helpful to comment (either here or in Section 2) on the robustness or theoretical justification of alpha and beta.

Technical corrections:

p. 2, l. 1: Please be consistent in capitalization of "Northwest" vs. "northwest"

p. 2, l. 11: don't -> do not

p. 2, l. 19: Maybe "on the edge" -> "on the verge"

p. 2, l. 27: To me, "plan-view" suggests 2D in the xy plane. Maybe "3D"?

p. 2, l. 28: "a lot of" -> "much". Also p. 3, l. 24.

P. 2, l. 33: Define RCP

Fig. 1 caption: “are used to calibrate the thermal forcing.” Also, capitalize “south” in the figure.

p. 5, l. 6: Maybe reword as “. . .the assumption of uniformly distributed melt generates only. . .”

p. 5, l. 11: As worded, the subject of “decreases” is “calculation”, which isn’t intended. Maybe change to “The calculated effective depth”

p. 5, l. 17: “future simulation” -> “simulations of future climate”

p. 6, l. 7: Hyphenate “real-world”

p. 7, Fig. 3 caption: undercutting rate should be m/day instead of m/yr?

p. 7, l. 7: 4 -> four

p. 9, l. 6: “about 8, 13 and 23 km upstream are distances we find. . .”; awkward wording.

p. 9, l. 9: position -> positions

p. 9, l. 11: “under no warming condition” -> “without further ocean warming”

p. 10, l. 5: Run-on sentence

p. 11, l. 2: “10 km or so” -> “$\sim$ 10 km”

p. 11, l. 3: project -> projects

p. 11, l. 23: advances -> advance

p. 11, l. 25: retrograde (no hyphen)

p. 12, l. 17: sensitive -> more sensitive

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-214, 2018.