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Interactive comment on "21st century ocean forcing of the Greenland Ice Sheet for modeling of sea level contribution" *by* Donald A. Slater et al.

Anonymous Referee #3

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General Comments: The manuscript details the implementation and results of applying two different ocean forcing strategies (termed the retreat and submarine melt implementations) to a suite of AOGCMs contained in CMIP5 for use in the upcoming ISMIP6 to inform the next IPCC report (AR6). The authors present the model parameterizations, including their motivations and the limitations of each implementation. Then, they apply the implementations to a set of CMIP5 models and present the resulting forcing parameters (subglacial runoff and ocean thermal forcing) and model projected retreat and submarine melt rates.

The work presented in the manuscript presents an important step forward for modeling ice sheet response to various warming scenarios and consequent contributions to sea level rise. The authors do a great job presenting their model implementations and the

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motivations for each, and overall the ideas are well-organized and logically presented. However, as currently written the manuscript suffers from several key problems, outlined below, that must be addressed before it is suitable for publication.

1. Lack of clarity. Specifically, a clearer introduction would eliminate a lot of potential confusion later in the manuscript surrounding how ocean boundary conditions influence ice sheet mass changes and how these are both important in models. As currently written, the distinction between process understanding within the field and effective modeling of those processes is unclear. There is a constant switching between observations and modeling that leaves the reader guessing which one is currently being discussed, and descriptions of how the processes are linked would greatly improve clarity for the modeling components of the writing. In some cases, significant detailed disciplinary knowledge is required to explain and justify the limitations and assumptions used within the model.

2. The discussion section is underdeveloped. Many interesting discussion points are presented and then left hanging without further exploration. Similarly, some of the assumptions and simplifications presented throughout the methods and results are not fully discussed, even where more information is available to inform a discussion (e.g. the magnitude of bias corrections and their interpretation; the influence of uncertainty in bathymetry; the potential uncertainty stemming from the assumption that submerged ice area remains constant).

3. The writing needs work both for content and grammar. As previously noted, the writing is overall unclear, with a lot of extraneous words at the expense of sufficient content in some places (e.g. p6 line 25: what is “inefficient” about current parameterizations? Are they computationally expensive or are they simply ill constrained?). Passive voice, dangling modifiers, and phrases not clearly linked to their parent idea are prevalent throughout, along with unneeded words and phrases (including connecting phrases such as thus, therefore, however, then, here). These detract and distract from the real power of the manuscript. Comma usage is quite poor (it is pointed out completely

only in the abstract line comments, below), including inconsistent use of the Oxford comma and missing and incorrect comma usage, particularly around non-compound sentences. Lastly, please proofread for subject-verb agreement, overall grammar, and consistency in formatting (e.g. capitalization of the Greenland Ice Sheet). This includes literature references, which are currently not consistently cited for the same ideas and lack cohesive formatting throughout (i.e. in-text citations are in random orders) and section references, which are an interesting mix of high level and lower-level references and do not consistently point to the most logical section (thus leading to confusion rather than clarity).

Specific ("line") comments:

Abstract:

p1 Line 1: Please expand on what oceanic "changes" you mean p1 Line 3: Provide some examples of what you mean by "key physics" and make "limitations in processing understanding" less ambiguous p1 Lines 3 and 15: unnecessary comma p1 Line 9: comma before respectively

Introduction:

p1 Line 20: passive voice p2 Line 2: dangling modifier p2 Line 6: comma needed after thus p2 Line 8: CMIP6 is used as an acronym before it is defined p2 line 19: it would be helpful for the reader to succinctly describe what CMIP is in this paragraph, as you have done for ISMIP. p2 Lines 21-22: ice shelves and floating ice tongues (and remove comma – not a compound sentence) p2 Line 22: clarify model design, or it sounds like you are designing the ocean forcing itself p2 Line 32: inconsistent reference ordering p3 first full paragraph: clean up language and extra words p3 Line 9: use of Oxford comma needs to be removed or added throughout manuscript

Methods:

Overview:

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p3 first paragraph: many of these ideas are repetitive with information presented in the introduction (though with different sets of references). The temporal words (past decade, since) are misleading relative to the information presented (warming in the late 1990s) and references (2010). p3 line 27: the links between calving rate, glacier retreat, and ice sheet mass loss have only been tenuously drawn. Please include a clearer description of these physical processes prior to discussing their modeling. p3 line 31-32: both italics and quotations does not match the abstract formatting p3 Line 33: taking part in ISMIP? p4 eq 2: for the reader not intimately familiar with Slater et al 2019, another sentence about kappa (how it is calibrated, under what conditions it is applicable/scalable) would be helpful. p4 eq 1 and 2: switching the order of presentation of these two equations would provide order consistency with the presentation of the retreat, then submarine melt, implementations throughout the text p4 line 34: the "or CMIP6" is confusing here. It might be helpful to instead note above, where you are addressing your use of CMIP5 inputs, that the process would be identical for using CMIP6 inputs.

Atmosphere:

p5 line 3: define acronym MAR p5 line 4: the use of "physically downscaling" is confusing, especially given the later statement that the downscaling is done statistically. Removing "physically" would improve clarity. p5 line 8: repetitive statement p5 section 2.2.2: If I am understanding correctly, hydrologic drainage basins are determined based on hydrologic potential (fine). Then, subglacial runoff is determined using surface runoff for those previously delineated basins. I think the authors need to better support and acknowledge the inherent assumptions here, including: 100% of surficial runoff reaches the bed and the surficial runoff reaches the bed with a similar spatial distribution, such that subglacial drainage basins with surface melt volume are appropriate for estimating subglacial melt volume. I would also like to see the use of f=1 substantiated. p6 line 5: how is Qj(1995-2014) for RACMO or PROJ calculated? Is it a mean? Median? Cumulative? p6 line 9: it would be helpful to provide some basic

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information on the bias corrections within the text (e.g. range and median+uncertainty). p6 lines 9-10: “we note that it might be thought preferable” is very wordy and passive language p6 line 11-13: this sentence could also be made stronger, particularly by quantifying the insignificance of the difference between RACMO and MAR (and noting the range of annual variability).

Ocean:

p6 line 16: needs parenthesis p6 line 22: what atmospheric process? For calculating surface runoff? p6 line 30-31: remove “details in” p7 line 10: quantify “some distance”. What criteria did you use to determine the extent of the sector beyond the shelf break? p7 line 13: quantify “coarse resolution” p8 line 7: as for the runoff, it would be helpful to present the range and some statistics (range and mean/median with uncertainty) on the applied bias corrections

Retreat Implementation:

p8 line 14: in the last equality, how are the units converted from salinity to temperature? p8 line 16: how were these constants determined, and are they valid for use here? p8 line 25: the thermal forcing itself is actually described in section 2.4.1, not section 2.3 p8 line 31: I’m not sure why this equation is presented independently of equation 2, which is the general form. I don’t think this equation is substantially different enough to warrant a second presentation, particularly since the text notes the projection is relative to 2014. p9 lines 1-5: the information on kappa presented here should be included the first time the equation is mentioned.

Submarine melt implementation:

p10 line 10: what criteria were used (e.g. slope) to determine when a feature was large enough to be considered “blocking”? p10 line 29: no capitalization on where p11 line 2: I’m not entirely convinced of the validity of using JUST the ocean bottom value for the thermal forcing, particularly if it’s not the highest thermal forcing within the vertical

profile. What rationale can be provided to suggest this won't underestimate melt rates? p11 line 14: typo

Results:

p12 line 6: section heading misses emphasis within section on glacier runoff p12 line 7: the use of "and" is confusing and suggests multiple runoff values are prescribed. Perhaps "each tidewater glacier/hydrological drainage basin" or "each tidewater glacier using its hydrological drainage basin" p12 lines 15-18: these sentences are more speculations than observations p12: The switch in referencing between largest glacier by flux and region between the text and figures is confusing. Suggestions to increase clarity are: add the glacier names to Figure 3 when the sectors are introduced and more importantly to Figure 7 (a and b) where the data shown is actually for individual glaciers and not the entire region. p12 line 30: inappropriate semicolon p13 line 3: section 2.4.1 refers to the section on thermal forcing, making this statement confusing. I've stopped noting odd section references after this point p13 line 16: the supplementary panel figures are not labeled with letters p14 lines 23-24: inconsistent use of sector names (e.g. At Humboldt Glacier (NO), little increase...) p15 line 3: The total count note is helpful, but confusing if you don't know offhand that 58 is the number of glaciers.

Discussion:

p15 line 25: anthropomorphizing of ice sheet models ("they see fit") p15 line 26: this statement implies that you have not already contrasted modeled ice sheet response between the two implementations p16 line 1: this suggests that averaging retreat over a population of glaciers resolves the fact that we cannot currently accurately represent calving or fully account for bathymetry in models of glacier termini. I would strongly disagree. Regional averaging may improve our modeled representation of retreat, which is of scientific import for further modeling and informing future investigations, but it does not fundamentally "ameliorate these issues". p16 lines 12-21: this reads as results, not discussion p16 line 23: subglacial runoff and ocean thermal forcing cannot be com-

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pared directly, as they are volume and temperature measures, respectively. p16 line 25-26: impact on what? retreat? mass loss? p16-17 lines 27-2: I would like to see this idea explored further (and clarification on why the authors switch from generalized "retreat and submarine melt projection" statements to just the "retreat implementation"). What are the implications of this compensation across model suite versus across retreat scenario comparisons? p17 lines 3-8: again, develop this idea further p17 lines 18-19: rewrite – currently not a sentence p17-18 lines 33-6: This paragraph leaves out the problem of incomplete bathymetry observations, a key area where improved models will still be limited by lack of observations. p18 line 10: other processes DO play a role, not MAY play a role. Perhaps the authors mean to emphasize that the other processes MAY play a SUBSTANTIAL role? p18 line 13: dash needed between ice sheet and ocean p18 lines 17-20: can you make the argument that the physics of plumes are well understood if we severely lack constraints for key constants?

Figures:

Overall: It would be helpful to use a different color scheme for showing comparisons of model runs than those colors used for plotting different sectors. This would allow the reader to more readily distinguish between sector-based results versus those averaged over the entire ice sheet that show model variability.

Figure 2: a zoom-in of the shaded portion of panel c (which is unlabeled but presumably indicates the time period used for the bias correction) is needed. As shown, it is difficult to see the similarities and differences between the datasets used to make the bias correction, and the zoomed out version suggests some apparently large differences between RACMO and MAR that are not substantially addressed within the text.

Figure 3: Add the resolution of the climate model shown in panel a for clearer comparison with panel b (which has a stated resolution).

Figure 5: a is missing units; the yellow and red points/lines are not labeled

Figure 6: b-why is such a large portion of the ice sheet showing no-data (entire drainage basins do not have subglacial runoff values)?

Figure 7: caption – subject verb agreement; b – is this also showing the largest glacier by ice flux for that sector? Also, the figure is missing units

Figure 10: The labeling with glacier name and region is quite helpful

Figure S1: The SE and NW colors are difficult to tell apart. Figures S3-S8: why not utilize some of the white space where there are no subplots as adequate space for the legend (particularly where it has been separated across multiple plots)

Acknowledgements: Michalea's name is spelled wrong

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