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The Cryosphere Discussions

Interactive comment on "21st century estimates of mass loss rates from glaciers in the Gulf of Alaska and Canadian Archipelago using a GRACE constrained glacier model" *by* Lavanya Ashokkumar et al.

Lavanya Ashokkumar et al.

lashokkumar@email.arizona.edu

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We would like to thank the reviewer for detailed comments regarding the glacier model. In this short summary, we will discuss the major points addressed by the review. We believe the issues brought up can be addressed in a revision to the satisfaction of the reviewer. Several of the review points arise from a miscommunication on our part in describing our model. Guided by all of the reviews, we plan to carefully edit the manuscript for clarity.

We are aware that our glacier model is not one of the more complex models as some of those in GlacierMIP2. The primary novelty of our model is its use of GRACE *monthly* observations as calibration data, in contrast to single regional mass balance trend estimates from other geodetic observations. While Huss and Hock., 2015 calibrate regional models with a single trend estimate for each region, we use 163 monthly GRACE observations which therefore include seasonal components and year-to-year variability. We have not made this point clear enough, as it seems to have caused some confusion to the reader (see e.g. page C5 last paragraph). We plan to address this in our revision.

The major difference in our glacier model to others stems from the way in which the glacier area feedback is performed. We have used volume-area scaling compared to the advanced flowline or glacier thickness based area-evolution used in some other models. We note that 5 out of 6 models in GlacierMIP and 5 out of 11 models in GlacierMIP2 have used volume-area or volume-length or volume-area-length scaling to account for glacier geometry change.

We are planning to work on the model calibration and validation with suggestions from all reviewers, hence it is likely that some of the results and discussion will change in the revised submission. The minor comments from page C4-C10 will be addressed in the detailed submission.

1. **Use of terminologies and reference to tables/figures**: The term extrapolation has been used in several instances, and we will rewrite/clarify these uses to properly characterize the constraints on the complex glacier models. It was our intent to describe the limitations of using a small number of in-situ mass balance estimates as model constraints for a large region, but we will rewrite these instances. We will revise the manuscript to use more appropriate modeling terminologies according to the standards of GlacierMIP and GlacierMIP2. We agree that the table 1 was incorrectly mentioned as table 5. And, the y-axis label in Figure 1 and Figure A5 has been corrected (com-

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ments from referee 1). In Figure 2, we have shown the temperature and precipitation after bias correction which are used as model inputs for future projections. As you may notice, there is large bias from precipitation (even after bias correction) from the GCM. This figure is similar to representation of temperature and precipitation from GCM in Figure 4 in Radic et al., 2014.

2. **Section 2.3: Inaccuracy of the modeling terminologies**: We agree that some of the formula used is confusing to understand the model setup and processing. We were primarily following the terminologies and formula for model setup as in Wahr et al., 2016. The equation 8 and 9 represent the downscaled temperature and precipitation at elevation bins, instead of temperature or precipitation at glacier. Further, it was a typo (L151) that we mentioned h and Δh as average elevation of glaciers. We plan a careful edit of the manuscript for accuracy and clarity in our revision.

3. **Model validation** (Page C2): For model validation, we will be including a section on model validation in our revision that examines the distribution of modeled mass balances over the glacier population. It was also a suggestion by referee 3 (point 2b). In contrary to the models in GlacierMIP1, we have not considered direct observations in the calibration step, which enables us to compare our model for individual glacier mass balance. As pointed out by the referee 3, we would like to examine the model validation to understand the performance of all individual glaciers in a region. We would like to clarify that the L184 - 187 refer to model calibration step, where the glacier model is optimized with GRACE monthly observations.

4. **Higher order dynamics** (Page C3, paragraph 1): We agree that we have not incorporated higher order of dynamics in our glacier models like some of the models in GlacierMIP and GlacierMIP2 (Hock et al., 2019; Marzeion et al., 2020). Here our intent was to describe how our model includes constraints on the seasonal components

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and inter-annual variability. In the revised submission, we will remove these terms and clarify our descriptions of GRACE constraints.

5. **Figure 2**: The precipitation rates from different GCMs are shown (lower panel). We will analyze the GCM precipitation again and check if this is incorrect.

Conclusion a: The regional bias between the observed mass balance (GRACE) and modelled mass balance is shown in Figure 1 (curves are offset). The comparsion is shown in the form of mass balance time series.

Conclusion b: The intent of this study is to calibrate glacier models with GRACE $monthly$ observations, in contrast to GlacierMIP where the model calibration was based on **single** regional estimate of mass balance. Here we are not trying to replace direct observations of mass balance, instead our model calibrated from GRACE monthly observations does not require any in-situ observations in model calibration.

Conclusion c: For the sentence "The Arctic Canada South has greater sensitivity of mass balance rates..". In the revised submission, we will perform the model calibration with inputs from ERA5 and optimization of parameters and we will revise the conclusions about the higher sensitivity in the ACS.

References

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