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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.

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We would like to thank the reviewer for constructive comments about the GRACE processing. In this discussion comment, we would like to broadly address and share our thoughts on these comments, and we look forward to addressing them in detail in the manuscript revision.

The major comments from the reviewer are related to the GRACE processing and its influence on the model sensitivities. We agree that there are issues related to sig-

nal overlap when recovering mass loss from Arctic Canada North, which is spatially adjacent to Greenland. We will also address the errors related to the use of degree one coefficients and C20, suggested by the reviewers. Further, we will provide a clear methodology in the GRACE processing (section 2.1) on how the GRACE signals are recovered for the sea level calculations. One of the interesting analysis has been the calibration of DDF and other model parameters based on the multi-year trend in the GRACE time series.

1. *A major concern are the treatment of signal overlaps between regions, particularly important for estimates of North Arctic Canada: could there be an influence of mass changes in Greenland on the glacier estimates? Looking at Fig. 1, considerable amount signal overlaps exist*.

From our prior work in Harig et al., 2015; 2016, there can be a signal leakage when the slepian functions are recovered from smaller spatial regions, such as the Arctic Canada North when there is a large concentration of gravity anomaly, that is from Greenland. Slepian functions are known to recover for effective signal recovery, in terms of spatial (smaller regions) and spectral resolution from the number of bandwidth. We confirm that the signal leakage is not double counted in the GRACE mass loss estimation. A detailed explaination for signal recovery and issues related to leakage will be demostrated in the form of synthetic experiments as in Von hippel and Harig, 2019 and it will included in the Appendix of revised paper.

2. *Another concern that would need more explanation is the calibration of the degree-day scheme of the glacier models with the GRACE data. As I understand, misfit is calculated as difference of monthly model estimates and GRACE observations. How sensitive are the optimal calibration parameters to the time scale considered for calibration? Would calibration and projection yield different results if, e.g. mainly trends or*

multi-year temporal components were considered?

This is an interesting observation about the multi-year temporal trend and we thank the reviewer for this question. We expect that there could be model sensitivities depending on the time period used in the model calibration. For example, if we were to consider the time period after 2010, mass loss from the three region are higher since 2012, and therefore it will influence the future loss rates based on the calibration period. However, if we were to consider a time period prior to 2008, say 2002 - 2007, it can have a different uncertainty on the future mass loss rates due to lower mass loss rates compared to post 2012 period. We are also likely to have different values for model parameters, say the degree day factor (DDF) or threshold temperature. We plan to do a sensitivity test on a time scale, as it can provide information about DDF and how GRACE contribute to model sensitivity (Wouters et al., 2019).

The main reason for considering the mass loss during 2002 and 2017 in the model calibration, is to account for all the inter-annual and annual seasonalities from GRACE solutions and also to match our model calibration with other glacier model studies in the GlacierMIP. We look forward to addressing the model sensitivities for multi-year trend in the revised submission.

3. *L64: Sun et al. 2016 showed that including the sea-level equation in the estimation of the geocenter motion affects recovered mass trends and annual amplitudes over the ice sheets. You are dealing with much smaller spatial scales (less impacted by lowdegree harmonics). But have you checked the effect? Using Sun et al. 2016 is the recommendation of the SDS centers*.

In our manuscript, we have used the version of degree 1 coefficients based on Swenson et al., 2008 (processed on June 2019) and we noticed that the degree 1 coefficients has not incorporated the effects of atmosphere and ocean dealiasing (AOD). We will record the difference in GRACE mass trend according to the recommendation by the

reviewer.

4. *L66: Similarly to c20 (and c30) – have you checked the sensitivity to the coefficients replaced? SDS recommend Loomis et al. 2019, https://doi.org/10.1029/2019GL082929*.

Our GRACE processing is based on C20 coefficients, described in the technical note 11 (Cheng et al., 2013). We will update the C20 with the technical note 13 and assess the trend changes in Alaska and Canadian Archipalego.

5. *L73: Please clarify at what stage the hydrological signals are removed – subtracted from the final mass time series or as a correction on the gravity field coefficients? Depending on the omission error and the response of your inversion approach, this could make a significant difference. Which temporal components are removed? Full signal? Only trends? Please comment on the reliability of trends and the spread of different hydrological models.*

We subtract the hydrological model prior to estimating the mass loss time series. The mass loss trends are recovered from full signals. Currently, there are several hydrological models available for comparsion. However, we used a hydrological model by GLDAS as in Gardner et al., 2013 that is in accordance with the glaciological communities.

6. **GRACE processing**: In the section 2.1 (Lines 76,83,114,117), there has been few minor questions about the GRACE inversion strategy, adopted from Wahr et al., 2006 and how the uncertainties from inversion are dealt compared to CSR mascons.

In the revised manuscript, we will provide more details about the inversion approach used and detailed description of noise uncertainties will be included.

The other minor corrections in the figures will also be revised in the manuscript.

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