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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.

Anonymous Referee #2

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Ashokkumar and Harig present a regional glacier model, constrained through GRACE-based observations, for Alaska and the Canadian Arctic. Generally speaking, more diversity within the glacier model ensemble is needed, and because of this, additions are needed, timely, and of interest to the community.

However, the manuscript contains a multitude of inaccuracies in terms (e.g., frequent use of "extrapolation" when relatively complex models are meant) and references to the literature (e.g., quoting non-modeling studies as background for modeling issues). I've listed them below under specific comments and suggestions, but the high number

of these implies that a general and major revision of the manuscript is needed to get rid of them. Generally, the manuscript makes a somewhat sloppy impression (e.g., tables and figures in the appendix seem to be in an almost random order, and some are apparently not referred to in the text at all; instead, reference is made to tables that don't exists – most frequently, to table 5). In some figures, it is unclear what is shown, or at least the axis labels don't make sense (e.g., lower row of Fig. 2: this cannot be precipitation – or it is the non-corrected precipitation (eq. 13), which would not make sense to show here, either).

In Sect. 2.3, these inaccuracies lead to a very severe problem, because it is not possible to follow the reasoning behind the mass balance model equations, nor is it possible to really understand how the model is (supposed to be) working (e.g., reference to hypsometry, but then lapse rates are calculated based on the difference of regional mean glacier elevation and glacier mean elevation, etc.). There is a lot of confusion of variables. E.g., \Delta h is defined both as an elevation (L151) and as a lapse rate (L172) – neither of which makes much sense to me, considering eq. 8. Overall, it is impossible for me to judge whether the model is set up in a meaningful way.

The model validation, which is central for a study like this, takes four lines in the manuscript (L 184-187) and is otherwise found in (more or less uncommented) Figures in the appendix, and somewhat spread out through the rest of the manuscript (mostly in 4.3 and 4.4). These validation results need to be presented in the main text, and need to be discussed in much more depth. Also, they should be compared to the performance of other, similar models (i.e., those that contributed to the intercomparison in Hock et al., 2019). Since the authors search parameter space for the minimum RMSE, they should also present and discuss the model's sensitivity to these parameter values. Much could be learned from such an analysis. Finally, in the validation/optimization, not even an attempt is made to measure out-of-calibration-sample performance. This is absolutely necessary to have a good estimate of how the model will be doing when not replicating known observations, but e.g. reconstructing or projecting mass loss during periods (or in places) for which no observations exist.

Generally speaking, I get the impression that the authors do not have an adequate understanding of what the state-of-the-art is in regional glacier models. There are frequent misunderstandings of the literature (listed below) – most severely perhaps that the model presented here “intrinsically accounts for higher order dynamics” (L293-294) or is “incorporating higher order of glacier dynamics“ (L334-335), which is simply not true, and implying that it is thus superior to the type of models intercompared in Hock et al. (2019). Nothing is said about ice dynamics in the manuscript, leading me to assume that dynamics are simply ignored (which none of the models in Hock et al., 2019, does). Similarly, there are frequent references to modeling papers, implying that they extrapolate in-situ observations to the regional scale (or something similar, listed below). This is a severe misrepresentation of what these models do.

Overall, the authors create the impression (willingly or not – I cannot tell) that their model approach is superior to what is typically done in state-of-the-art models. (i) The equations they present do not confirm this; (ii) the evaluation is not presented in depth, and not compared to other models' evaluations, such that it is impossible to say how the model is doing in comparison to others; (iii) there are some clear misrepresentations of what other models are doing.

I hope that my relatively strong opinion here is not misunderstood: it is important that more glacier models on regional scales are developed, and I strongly and sincerely welcome efforts to do so. I don't even think it is necessary, or even desirable, that new glacier models are more complex or more accurate than existing ones. But the description of the model, and the motivation of modeling choices, needs to be a lot clearer than is the case here, and there need to be in-depth evaluations of model performance. I cannot and don't want to rule out that the model presented here is reasonable. But there are many indications in the text and equations that it is not (listed in detail below), or the authors were “only” extremely unclear in their presentation.

Based on this assessment, I cannot recommend the manuscript for publication in The Cryosphere.

Specific comments and suggestions:

- Title/throughout the paper: you are referring to the “Gulf of Alaska” and use it as synonymous with “Alaska”, the name of the RGI region (e.g., Figs. 3, 4). Is your region definition different from the RGI's? If so, the direct comparison with the Hock et al. (2019) data is problematic. If it is not different, please use the same name.

- L5: “extrapolate”: I think “project” is a better word, since “extrapolate” invokes associations with simple linear extension of a time series.

- L7: “highest” compared to what? The other considered regions, or globally, or temporally? - It is unclear in the abstract what the ranges of numbers given refer to. Please indicate that they correspond to the lowest and highest ensemble member.

- L8-10: I don't see a reason for singling out ACS in the abstract: Fig. 4 shows that generally, your model is on the high mass loss side of the Hock et al. (2019) ensemble, and really sticks out in Alaska for RCP4.5, in ACN with one ensemble member in RCP4.5 and RCP8.5, and in ACS in RCP2.6.

- L17: remove dot after m in “m.w.e.”

- L22: name of region missing after “-3 Gt yrˆ-2”

- L23: “has” -> “have”

- L31-33: I would argue that “extrapolation of regional mass balance from about 255 direct observations to represent 200,000 glaciers worldwide (Cogley, 2009).” is a misrepresentation of both what is done typically in models (which do not use observations to extrapolate, but as a constraint – and which therefore can produce global numbers that a very different from an extrapolation) and what is done in Cogley (2009), since he doesn't do any modeling.

- L36: “Southern” Hemisphere

- L37: Again, none of the three studies you cite “extrapolates” any mass balances. I think what you want to say is that the models are too weakly constrained, since there are not enough mass balance observations in these regions, leading to large uncertainties.

- L39: “several studies” – please cite examples

- L43-45: Unclear what is meant: what “process”? How could model parameters be able to represent mass balance? I don’t think the choice in Huss & Hock (2015) to use regional observations was based on an inability of the model to produce numbers for individual glaciers (the model is based on individual glaciers).

- L50: It is neither clear what “uncertainties from extrapolation of direct observations” are meant here (see above, the models do not “extrapolate”), nor how this is relevant for “issues in volume-area scaling”.

- L52-53: Unclear what is meant, since Arendt et al. (2013) don’t do any modeling. (However, almost all glacier models do account for spatial variability within a region, so it’s unclear why the reference is made to that paper).

- L58-59: “which perturb the geoid at a spatial resolution of several hundred kilometers” makes it sounds like the perturbation is on the scale of several hundred kilometers, but that is actually only the resolution of GRACE; please rephrase for more accuracy.

- L68: “the glacial isostatic adjustment (GIA) model” – which one? There are many around.

- Sect. 2.1 and 2.2: I’m not a GRACE expert, and don’t know the technical literature well enough to really evaluate these sections. My therefore somewhat superficial impression is that this part of the manuscript is the most mature. However, I’m wondering about the motivation of producing GRACE-based estimates specifically for this manuscript: The overall goal is to project glacier mass change in the three regions

using a model that is constrained by GRACE. Why then not use a previously published GRACE-based estimate? There may be good reasons, but they are not obvious to me, and not stated in the manuscript. At the moment, while the sections read well, they take away focus from the main storyline of the manuscript.

- Fig. 1, upper row: shouldn’t the color bar be labeled “mass change”? Lower row: please include a legend for the lines (which is model, which is GRACE).

- L119: please either indicate that you are here referring only to the period of model calibration, or also introduce the GCM data you are using

- L121: please use either degree day of temperature index; I think temperature index is more correct since it is more general (i.e., also applicable at monthly time scales)

- L123: delete “such as the glacier outlines, area and elevation”, since this is what hypsometry means

- L124: delete “glacier”

-L131-132: you have already introduced ACN and ACS, you can use them here (or add “Arctic” in front of “Canada North”).

- L131: why do you only mention the number for “Gulf of Alaska North”, not Alaska (or Gulf of Alaska) entirely? That they are different from Gulf of Alaska South might motivate to consider them separately, but you could (and should) still also include the southern part.

- L 136: “From this information, we compute the area elevation distribution of glaciers at every 50 m grid spacing.” How is this different from the hypsometry contained in the RGI?

- L145-146: Then why include it in eq. 6?

- L147: “at” -> “a”

- L147: why a degree day, if monthly time steps are used (L141)?

- L148: it's not the number of days above threshold temperature, but – as given in eq. 7, the sum of temperatures above threshold.

- Eq. 7: why isn't T_gl used here?

- Eq. 8: Why do you use mean glacier elevation when you want to consider glacier hypsometry? And why use \Delta h as a basis for the correction, shouldn't this be the elevation of the geopotential surface from which T is taken?

- L155: why only convective precipitation? Is the rest of precipitation ignored? (Also: same questions regarding h and \Delta h as for eq. 8).

- L158: The precipitation and temperature lapse rates should be VERY different, they are completely different things!

- L162: what is "snowfall from all the glaciers"? I don't understand.

- L164: "We ignore the effects of tidewater calving from Gulf of Alaska and Canadian Archipelago since they contribute less to regional mass balance (Larsen et al., 2015)." This is a very strong assumption, which would easily explain a higher temperature sensitivity of this model (i.e., mass loss from frontal ablation is treated like surface mass loss, which implies an overestimation of surface mass loss, which leads to too high degree-day factors in the calibration).

- L172-173: \Delta h is not a lapse rate (see L151), \Delta p should not be a lapse rate according to eq.9 (but it is not clear to me, what is should be); the precipitation gradient d_prec is closest to what I would understand as a lapse rate in eq. 9 (but that depends, of course, on what \Delta p actually is).

- L175ff: I think "parameter space" is meant, instead of "model space". Why choose these three parameters? Do you mean that the modeled mass balance has greater sensitivity to these parameters? Can you show this?

- L175: I don't think you solve for the parameters, but optimize them.

- Eq. 11: units are missing. What motivates the choices of boundaries of parameters space?

- L184-185: How do you measure explained variance? What about a model bias, the amplitude of variability, etc.? How big is the minimal RMSE that you obtain?

- Fig. A3 is referred to before Fig. A2, Fig. A1 is not referred to at all.

- Table 5 is the first table that is referred to (and where is it?).

- Figs. A4 to A6: what are "modeled observations"? How do the model results agree with the observations? This should be discussed in the text, and compared to the performance of other, similar models (e.g., those in Hock et al., 2019).

- L199-200: what does this mean: "closely modeled the climate model data"?

- L200: the delta approach (eq. 13) should remove any bias. What is meant here?

- L207-208: sentence incomplete.

- L213-214: I don't understand the reasoning of "since our model constrained by GRACE observations has secular and seasonal trends in mass balance."

- L213: Is it then correct that the model keeps the area and hypsometry of glaciers constant, even in the projections until 2100? Again, this is a severe limitation compared to other models, and would be a very simple explanation why it projects more mass loss than other models.

- L218ff: I'm not sure if I understand this correctly, but it would not make sense to re-tune the model after the application of anomalies from the GCMs (eq. 12 and 13) to maximize the agreement between model results and observations, since the GCMs will be in very different states of climate variability.

- L221: where is table 5?

- Fig. 2: What is shown in the lower row? The vertical axis label does not make sense, the different GCMs do not have differences in precipitation that differ by a factor large than 2. What are the gray lines in the middle row?

- Fig. 3: why does the middle row of panels say “CESM”? the legend implies that the different GCMs are coded in the line styles. In Fig. 4, it’s in the center column.

- L229ff: Unclear whether this refers to the results when GCM data are used as boundary condition, or if not, why/how/if this is different from what is stated further above.

- L238: where is table 5? The results from the optimization need to be discussed in much more detail.

- L239-249: how do these results compare with other models’ performance in these regions?

- L253-255: “Uncertainties in the volume and mass loss rates depends primarily on the (i) initial conditions of volume, (ii) glacier hypsometry or area changes, and (iii) sensitivity to temperature and (iv) precipitation.” How do you arrive at this conclusion?

- Sect. 4: it is fine that you compare with previous results. However, there are some significant assumptions in you model (pointed out above) that are not addressed, and that easily would explain the increased mass losses you see (and which would be unphysical).

- L270: “Like the existing glacier models”: not all of the use a temperature-index approach.

- L274: they were not extrapolated, but projected using models.

- L278: Which problem? Huss and Hock (2015) is one of the models used in Hock et al. (2019). The other models in Huss and Hock (2019) used very different strategies.

- L285ff: I’m not convinced that this is advantage, unless you show that your model’s performance is actually superior to the other models’ performance. I would not as-

sume so, since the higher accuracy of the regional mean comes at the cost of lower spatial resolution. The results shown in Figs. A5 to A6 let me doubt that your model’s performance is clearly superior. It may be, but you need to show this.

- L293ff: “Our model constrained by GRACE intrinsically accounts for higher order dynamics”: I do not understand at all how a seasonality signal should say something about dynamics. In the manuscript, there is no statement on treatment of dynamics in the model whatsoever, which leads me to the assumption that it is simply ignored. All models in Hock et al. (2019) have some simplified representation of dynamics. Figs. A5 and A6 show mass balance, and in this short time period, there will not be any discernible impact of ice dynamics.

- Fig. 5: please use region names consistently

- Fig. 5: What range is represented? Max-min, or some percentile of the ensemble? It would be useful to have information also on a central value (e.g., mean or median) of the ensemble.

- L303-end: I will not continue with as detailed comments as above, since there will be major revisions necessary, presumably changing the results considerably.

- L303-307: much more interesting than arbitrary changes to glacier geometry and temp/precip would be the discussion of the model’s sensitivity to the parameters.

- L321ff: this should be shown. Based on what is presented in the manuscript, I assume that the higher rates are explained by (i) ignoring the contribution of frontal ablation to mass change in the calibration of the surface mass balance model, and (ii) in ignoring glacier geometry change in the projections.

- L334: this is simply not true. No word is said on “higher order glacier dynamics” except that it is somehow incorporated. Where and how is this the case?

- L356-357: “This method eliminates the need for extrapolation of direct observations for regional mass balance and SLE as in Radi′c et al. (2014).” Radic et al. (2014) do not extrapolate direct observations for regional mass balance.

- L358ff: how do the units used here agree with the monthly time scale mentioned further above?

- L365: again, I don't think \Delta h in eq. 8 is a lapse rate (nor should it be a parameter that is optimized)

- Sect. 4.3. and 4.4 should go into (a new) section on model validation, presented before the results.

- Conclusion a: You have not shown this, since you have not shown such a comparison. I do not see any measurement of regional bias in the manuscript.

- Conclusion b: I don't agree that you have shown that the model's results are good enough to suggest they can replace conventional field observations, and I utterly disagree, given Fig. A5 and A6.

- Conclusion c: I disagree that you have shown "that Arctic Canada South has greater sensitivity in the recent decade, and our model is able to capture this sensitivity". The greater sensitivity is likely an artifact of modeling choice, see above.

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