The Cryosphere Discuss., 6, C1763–C1776, 2012
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The Cryosphere Discussions

Interactive comment on "Past and future sea-level change from the surface mass balance of glaciers" *by* B. Marzeion et al.

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We would like to thank Valentina Radić for obtaining the reviews, and we would like to thank the reviewers for providing their detailed, very constructive and helpful comments on our manuscript. We were able to address all their points (as detailed below), and without doubt their suggestions have lead to significant improvements of our manuscript.

Response to Regine Hock:

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Interactive Discussion

Discussion Paper

1. **Comment:** The authors model the surface mass balance, and seem to neglect refreezing, a component that is large in some regions. Or is refreezing including in their 'surface mass balance', and hence it is the 'climatic mass balance' (according to Cogley et al., 2011). The description of the model seems to suggest that it is only surface mass balance. This is fine, however, as for calving, some discussion should be included about the possible impact of re-freezing. In any case it should be made clear whether or not re-freezing is included not.
   Response: Our model neglects resolving any particular process of the surface mass and energy balance of the glaciers, in favor of applying a bulk estimate of the specific mass balance, depending on temperature and precipitation alone. But neglecting to resolve processes does not imply neglecting the impact of these processes. E.g., as pointed out by the reviewer, our model does not resolve refreezing, but since our model is calibrated with, and validated against mass balance measurements that include effects of refreezing, the impact it has on the mass balance is included in our model. To some extend, it may be hidden in e.g. the optimal parameter values of $T^{\text{prec solid}}$ and T_{melt}, but it will also be represented by the model's error. The same reasoning applies to other processes impacting the surface mass balance, such as aeolian snow transport and avalanching.
   We added this discussion to the manuscript.

2. **Comment:** Structure
   Although overall the paper is very well structured, I suggest that a data chapter is added, where all data sets are explained. Currently the dataset are 'hidden' in chapters where the model parameterizations are explained, however, it would be easier to read if model description and model application (including the use of the datasets used in this specific application of the model) are clearly separated. The structure is especially awkward in chapter 6, which claims to be RESULTS, but starts with description of data gaps and the GCM data. These subchapters

under 6 seem mislabeled and should better be combined in a Data chapter together with the CRU and other relevant data sets to force the model.
Response: We added a new section (Forcing data and treatment of data gaps) in order to improve the structure. We also added some more background information on the forcing data sets, in particular on the RCP scenarios, and a reference to more information.

3. **Comment:** Related to 2.) a little more information should be given about the climate scenarios. What is the difference between the different RCPs? How where the GCM results downscaled?
Response: We added some information on the RCP scenarios in the new section (see above). Here, we also added a description of how we applied the GCM data to our model.

Detailed comments

1. **Comment:** Abstract
a) It would be good if a little bit more information about the modeling was included here, for example that the model was forced with monthly T/P data from CRU, and how many GCMs were used.
b) line 2: add: "... individual glaciers (excluding the ice sheets)" to make clear that this paper is not about ALL glaciers in the world but all except for the ice sheets. It is clarified in a foot note, but should preferably be made clear in the abstract as well to avoid confusion.
Response: We extended the abstract along the lines suggested by the reviewer.

2. **Comment:** Page 4, L4, 'amount' should be replaced by 'number'
Response: Corrected.

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3. **Comment:** Line 19: glaciated should be glacierized (according to Cogley et al., 2011)
   Response: Corrected.

4. **Comment:** Line 19: 'ice shields' should be 'ice sheets'
   Response: Corrected.

5. **Comment:** Line 25-26: I would omit the first part of that statement because this can not be generalized. Changes of the ice sheets could in principle affect more people if the changes were to raise the sea-level sufficiently.
   Response: This is true, but since we base our statement on the condition that the glaciers "potentially contribute stronger to sea level rise within the 21st century than these ice sheets", we believe the reviewer's caution is already expressed in the text, because we imply that out statement would be wrong if the ice sheets turned out to change more than anticipated.

6. **Comment:** Page 6 (MB model)
   For the symbol of surface mass balance, "MB" is unfortunate because in an equation it suggests that MB means M*B, rather than a mathematical/algebraic entity. The authors my consider to adopt the terminology/symbols of Cogley et al., 2011.
   Response: In the entire text and figures, we changed the symbol for mass balance to B (and the symbol for width to W).

7. **Comment:** Page 6 Equation 1
   Units should be added here. Specific units can be in m w.e. or kg/m2. Units are important here because depending on the units used in the paper Equation 7 needs to include the density of water. I assume that units are in kg/m2, but it would be good to say that.
   Response: As we state on page 3186 line 3, we assume an ice density of 900 kg m^{-3}. Not specifying the units (mm w.e. or kg m^{-2}) in Eq. 1 implies assuming a water density of 1000 kg m^{-3}, which is within 0.02% of the true density of water

at temperatures around the melting point. This error is substantially smaller than the error of assuming a constant ice density, and is therefore negligible.

8. **Comment:** Page 8, 2.1.3
Since the RGI inventory is not well known, I suggest that you add a sentence what it is, including the total area.
Response: We extended the text as the reviewer suggests. The total area according to the RGI is given in table 3.

9. **Comment:** Page 8, L10
Please explain how you identified ice caps in the RGI as this is not part of the RGI. How many ice caps did you find?
Response: If the topography allowed to clearly identify drainage basins, the glacier complex was separated into flow sheds and treated as a number of individual glaciers. Otherwise, the glacier was inspected manually, and treated as an ice cap if appropriate. Overall, 29 glaciated areas were identified as ice caps this way. We added this explanation to the manuscript.

10. **Comment:** Page 22
Structure needs adjustment (see above). Part of what is described here is not Results but datasets and methods.
Response: We added a new section (Forcing data and treatment of data gaps) in order to improve the structure.

11. **Comment:** Page 28, L10
Why overestimated? Since calving is not included the actual mass loss should be underestimated by your model. Whatever is calved off (always a mass loss) needs to be added to the surface mass loss?
Response: It is possible that the model overestimates the sea level contribution (NB: not volume/mass loss) from a calving glacier because it is unaware whether the ice that is lost (via surface mass balance) was afloat or not, and counts it as

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sea level rise, while it actually would not have contributed to SLR if the melting ice was afloat. It is therefore not calving which is the source of this potential overestimation, but being afloat is a common precondition for a glacier in our model to be affected either by calving or overestimation of SLR contribution.

12. **Comment:** The mass balance model computes specific balances, however, results are presented in mm SLE. Please explain how you convert, specifically which ocean area you assume.
Response: We assume a constant ocean area of $3.62 \cdot 10^{14}$ m^2. We added this explanation to the text.

13. **Comment:** Figures 17 and 19
It will not be possible that all y-scales are the same, however, I suggest that the authors attempt to use as few scales as possible (using the volume % scale). Currently comparability is difficult because every single subplot has a different scale. A number of regions have rather similar % volume losses and could be plotted with one and the same scale.
Response: We changed the figures to suing only 3 different % volume scales.

14. **Comment:** Table 1
What is SS? I assume stand. Deviation. The caption should include the symbols/abbreviations used in the table. Also sigma would be more appropriate?
Response: SS is the skill score, see Sect. 3, where we also added a brief explanation of the skill score.

15. **Comment:** RCP is sometimes written with capital letters, sometimes not. This should be consistent (see e.g. Fig. 22). I think it is usually written RCP.
Response: We changed the text and figures and use RCP consistently now.

16. **Comment:** Table 2 should be combined with one of the others listing all regions. For example, Table 1 and 2 can be combined.

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Response: Combining all tables that list data by regions seems impracticable, since it wouls also involve table 3. Tables 1 to 3, as they are now, are thematically separated: table 1 contains results from the cross validation, table 2 contains information on the model procedure, and table 3 contains results from the model. We would prefer to keep it this way.

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Response to Graham Cogley:

Substantive Comments

1. **Comment:** P3180 L10-12 It would be appropriate to cite Pfeffer et al. 2008 here (Science, 321, 1340-1343).
   Response: We included the reference.

2. **Comment:** P3182 L19-21 It would be helpful to give the resolutions of the CRU datasets.
   Response: We added the spatial resolutions to the text.

3. **Comment:** P3184 L16-17 "Since dates of the glacier outlines are not given ...". Dates or date ranges are given for about half of the outlines in RGI version 2.0, and if it were possible to update this paper to the newer RGI version this information could be exploited to reduce the uncertainty in glacier area.
   Response: We are aware of version 2 of the RGI and would have preferred to have used it for the reasons the reviewer mentions. Unfortunately, running the watershed algorithm to separate the ice outlines into individual glaciers was too time consuming (several months) in order to be completed after the release of the RGI version 2.0 and before the IPCC-related submission deadline. But also because of the amount of manual labor involved, it is unreasonable to repeat the step at this stage. Additionally, while the uncertainty we artificially introduce to account for dating uncertainty could be decreased, it is a very small source of uncertainty compared to the uncertainty of the mass balance model itself, and quickly overtaken in the error propagation.

4. **Comment:** L25 A brief explanation of the "motivation" of eq.5 would make it more accessible to readers unfamiliar with volume-area scaling. The first term in the

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parenthesis on the right is simply A(t+1).
Response: It is not strictly A(t+1), but the equilibrium value for A corresponding to V(t+1). We added a pointer to the term in Eq. 5 where we describe the reasoning behind the equation.

5. **Comment:** P3188 L5 "all overlapping 31-yr periods". But I am not sure what is being done here. I understand a "climatological value" to be a multi-annual average for a fixed reference period, and I therefore infer that P(t) and T(t) are 31-yr running means. What I do not understand is why the sensitivity in year t should be a function of P and T in 30 other years. More explanation is needed.
Response: P(t) and T(t) are indeed 31-yr running means - but $\mu(t)$ should not be misunderstood as the sensitivity for a given year t, the model rather assumes that the sensitivity is constant over time.
We perform this procedure in order to identify a climate condition that is realistic for the site of the glacier and at the same time would lead to the glacier being in equilibrium. In that sense, t is merely an index into different climate conditions that occurred at the glacier site, and from which the sensitivity can be deduced.
We added this explanation to the manuscript.

6. **Comment:** P3190 L3 "glacier's". I have been wondering why the symbol MB was adopted for mass balance in eq.1, and it now appears that B (presumably the first letter of Breite) represents the width. The development would be more accessible to more readers if mass balance were B and width were W.
Response: In the entire text and figures, we changed the symbol for mass balance to B and the symbol for width to W.

7. **Comment:** L15 Eq. 15 assumes that the glacier is a rectangle, which may be harmless but seems odd.
Response: We are aware of this, but we think it is consistent with understanding B (now W), L, V derived by scaling as the scales of the width, length, and volume,

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and not necessarily as their real values.

8. **Comment:** L20 Define "skill score" briefly.
   Response: We added a brief description what the skill score represents.

9. **Comment:** P3193 L7-8 Name the four worst-performing regions of Figure 10. In three, there are only one or two measured glaciers, but some discussion of the worst performer of all (with eight measured glaciers [Low Latitudes?]) might be warranted. It may also be useful to discuss in this paragraph some of the individual outliers in Figure 8. There are too few extreme observed balances (left and right centre) to judge the model's ability to reproduce them, but all of the really bad instances of mis-modelling (top and bottom centre) seem to be in the Low Latitudes or Southern Andes. (In passing, it is not very easy in some figures to distinguish the regions by their colours, although on the whole the colour coding is a good idea.)
   Response: Surprisingly, it is not the Low Latitudes (yes, that is the point that stands out) that is performing worst, but Scandinavia (light green dot, 52 glaciers in the region), as mentioned in the text. The reason why the Low Latitudes stand out in Fig. 10 perhaps lies in the logarithmic x-axis – but admittedly, they are the region where model performance is clearly worst according to other indicators (as discussed in the Discussion section). The second-worst performer is Western Canada & US (39 glaciers). But we are reluctant to discuss the "ranking" of the regions with respect to bias, because the Kolmogorov-Smirnov test tells us all of them (except Scandinavia) are consistent with a normally distributed sample with zero mean. Perhaps the important message of this is how difficult it is to obtain meaningful statistics for those regions where there are few mass balance measurement.
   Regarding the colors: we know that it is not possible to determine every region from the colors – it is surprisingly difficult to find 18 colors that are easily distinguishable. Because of this problem, we tried to set the colorbar such that at

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least the greater area is discernible (blue is North America/Greenland, green is Eastern Atlantic, yellow/orange is Eurasia, violett is Low Latitudes and Southern Hemisphere).

10. **Comment:** L22-23 Does "sampled" mean "measured" here?
Response: Yes, corrected.

11. **Comment:** L25 "artificially reduces the number of close-by sampled glaciers": this is obscure to me. I think it may mean that the remote glaciers have a disproportionate impact on the dependency. If so, I am not sure of the correctness of the remark, but as noted in the next sentence it relates to a weak relationship.
Response: What we mean is that by construction, you have to withhold information from the model during the cross validation which it can use when the model is applied. We reformulated to clarify.

12. **Comment:** P3195 L8 This remark about interannual variability is correct, but it is also true, and perhaps more interesting, that climatic change (a trend in n) will lead to a trend in ϵ.
Response: True – we added a sentence to the manuscript.

13. **Comment:** L18-22 See comment at P3184 L16-17. 5% is likely to be a generous but not unreasonable estimate of the uncertainty in area.
Response: We agree – but as we say above, even the 5% are very quickly overtaken by other error sources (within a few years).

14. **Comment:** P3196 L20-21 It would be more direct to say "the regional density of mass-balance observations is well correlated with the density of weather observations".
Response: Text changed accordingly.

15. **Comment:** P3197 L21 I think this should read "for one series of geodetic volume change measurements".

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Response: There exists a series of geodetic volume change measurements for Hintereisferner, and it is being used in the validation, but in this example, we only show one measurement for clarity.

16. **Comment:** P3198 L27 Modelled area changes less reliable than modelled volume changes: I do not follow this argument. Surely the two are tightly coupled through eqs. 5-7.
Response: They are obviously coupled, but not that tightly: in Eq. 5, the parameter τ_A allows the behavior of A to differ considerably from V (depending on the history of the mass balance, A may be growing while V is shrinking, and vice versa). This shows that while the mass balance model is performing reasonably well (mass balance directly coupled to volume), our parameterization of ice dynamics is doing not so well (translating volume changes into area change).

17. **Comment:** P3200 L4-6 Antarctic balances assumed equal to the mean for the rest of the world: Analysis of ICESat data by G. Moholdt suggests that this is a poor assumption. The analysis is not yet published, but I understand that the present authors are aware of it.
Response: We added a paragraph to the discussion pointing out that the assumption is hard to justify. Depending on the publication schedule, we will add an appropriate reference.

18. **Comment:** P3202 L21 "characterized by big glaciers" (see also L15, L18): this concept needs to be better focussed, perhaps in terms of the regional mean or median glacier area. Western Canada and US, in particular, may be inappropriately classified at L17.
Response: We added the regional mean glacier surface areas to justify this statement.

19. **Comment:** P3206 L15 "Since our model ...": I am struck by the apparent absence of any allowance in the model for internal accumulation (sometimes called

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refreezing). The omission of frontal ablation from a model of surface mass balance is understandable, but the omission of internal accumulation needs to be justified, or at any rate discussed. Generalized as the model necessarily is, it appears that its various thermal parameterizations (e.g. as described in sections 2.1.8, 2.1.9 and 2.2.5) will not capture the tendency for some surface meltwater to be retained with the glacier.
Response: Our model neglects resolving any particular process of the surface mass and energy balance of the glaciers, in favor of applying a bulk estimate of the specific mass balance, depending on temperature and precipitation alone. But neglecting to resolve processes does not imply neglecting the impact of these processes. E.g., as pointed out by the reviewer, our model does not resolve refreezing, but since our model is calibrated with, and validated against mass balance measurements that include effects of refreezing, the impact it has on the mass balance is included in our model. To some extend, it may be hidden in e.g. the optimal parameter values of $T^{\text{prec solid}}$ and T_{melt}, but it will also be represented by the model's error. The same reasoning applies to other processes impacting the surface mass balance, such as aeolian snow transport and avalanching.
We added this discussion to the manuscript.

20. **Comment:** P3207 L5-6 "Depending on the scenario": My reading of the lower panel of Figure 24 is that the broad peak comes in the fourth quarter of the century in RCP 8.5, the middle of the century in RCP 4.5 and the second quarter of the century (say 2030-2050) in RCP 2.6. Perhaps "Depending on the scenario" should be expanded somewhat. (See also P3178 L18.)
Response: We expanded the description of timing of the peak mass loss rates along the lines suggested by the reviewer, by explicitly stating the time ranges for each of the scenarios (also in the abstract).

Stylistic Comments

We followed all of the reviewer's suggestions.

Interactive comment on The Cryosphere Discuss., 6, 3177, 2012.