The Cryosphere Discuss., 7, C1588–C1624, 2013
www.the-cryosphere-discuss.net/7/C1588/2013/

The Cryosphere
Discussions
Open Access

Interactive comment on "Feedbacks and mechanisms affecting the global sensitivity of glaciers to climate change" *by* B. Marzeion et al.

B. Marzeion et al.

ben.marzeion@uibk.ac.at

Received and published: 29 August 2013

We would like to thank Eric Larour for obtaining the reviews, and we would like to thank all 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 Daniel Farinotti:

General Comments

1. **Comment:** The only general comment I have concerns the presentation of the manuscript: I would strongly encourage the authors in trying to simplify every single sentence as much as possible. The use of footnotes and rather prominent use of explanations in parenthesis are probably one of the most immediate signs of a text that doesn't runs as smooth as it could. This often hampers the reading - which is a pity in light of the cleanliness in which the experiments are set up. See the "stylistic comments" section for some example and concrete suggestions for reformulations.
   Response: Thanks for the thorough work! We have tried our best to improve the text (see also below for your stylistic comments). Probably, the readability was also generally improved by (i) the reduction of the material presented (we removed a few of the sensitivity experiments, which were criticized by all reviewers), and (ii) because we forewent the upscaling for Antarctic peripheral glaciers, which was also criticized by two reviewers and necessitated some awkward formulations.

Specific comments

1. **Comment:** P2762 L10-12: This is somewhat unclear: What do you mean with "because of geographic distribution" (what is so special about it?)?
   Response: We simply mean that all glaciers are on land, and many at high latitudes (see P2773 L23ff). We removed the ambiguous part of the sentence.
2. **Comment:** P2762 L11: "anomalies" with respect to what?
   Response: Anomalies with respect to the mean of 1961-1990 – we changed it to "projected future changes".

3. **Comment:** P2762 L17-19: At this stage, it is not possible to understand how "sensitivity" is defined (there is no general definition for it in glaciology, is there one?). Similarly it is unclear with respect to what this sensitivity is "decreased [...] by a factor two to three".
   Response: We changed it to "...global glacier mass loss rates caused by..." and added "compared to rates of mass loss when hypsometric changes are neglected"

4. **Comment:** P2765 L24: Why "area mean monthly solid precipitation"? "Mean" over what? Do you mean "precipitation per unit area"? The confusion arises also because of the sentence at P2767 L8.
   Response: Yes, changed to "precipitation per unit area" – On P2767 L8, it is actually the precipitation integrated over the surface area – we changed it to $\int P_{i,\mathsf{clim}}^{\mathsf{solid}}$.

5. **Comment:** P2766 L25: $\mu*$ looks like a "melt factor" in PDD models. The wording "sensitivity" sounds rather artificial to me...
   Response: Actually, we think temperature sensitivity is more accurate - even though we agree it's no commonly called this in the literature (but Oerlemans, e.g., calls it mass balance sensitivity to temperature).

6. **Comment:** P2766 L8: Where is the bias correction coming from? This also brings me to the question if the model parameters are glacier-specific. $T^{terminus}$ definitively is. What's about the other ones?
   Response: The bias correction is estimated from the cross validation and described later in the manuscript – we added a pointer. The bias correction is glacier-specific as well (as should become clear later in the manuscript), T_{melt} is optimized globally.

7. **Comment:** P2766 L16: Why "linearly adjusting"? What do you mean with "linear"?

   Response: "linearly" because the changes in area and length are linearly related to the difference between their equilibrium values corresponding to the current volume and their current values. This is described in the next sentence, starting with "I.e., the surface area change dA of a glacier during each mass balance year t is calculated as..."

8. **Comment:** P2766 L7: Why "towards"? It sounds like there is a time-lag or something similar. Is there one?
   Response: There is - see Eq. 2, two lines below.

9. **Comment:** P2766 L20: For consistency you should write "dA(t)" and "$\tau_A(t)$", isn't it? This is particularly true for τ_A. Moreover, I would suggest placing Eq. (5) directly here, since the question "how is τ_A computed?" immediately comes up.
   Response: We added the time dependence to the variables, and linked to the equations for the relaxation time scales.

10. **Comment:** P2766 L20: Somewhere you need to state something like "dV(t)=B(t)*A(t)".
    Response: Done.

11. **Comment:** P2766 L22: Directly in the text, give the values and units for c_A and γ that you used.
    Response: Done.

12. **Comment:** P2767 L23: Give the values and units for c_L and q. Place Eq. (4) at the point where you introduce τ_L.
    Response: Done. We added a reference to the equation where we introduce τ_L.

13. **Comment:** P2767 L8: Over which period is the "climatological solid precipitation" determined?
    Response: It's calculated over the preceding 30 years – we clarified this.

14. **Comment:** P2767 L15-16: State the actual number of glaciers that have "available mass balance measurements in Cogley (2009)"
Response: Done (it's 255 after those with questionable records or missing meta data are excluded).

15. **Comment:** P2767 L16-18: Not sure to understand correctly: What you do is to "scan" through the past time series and look for a given period that fulfills your condition Eq. (6)?
Response: Exactly!

16. **Comment:** P2768 L2: Ok, but the "t" mentioned here is a particular "t", not "any t" as it was in Eq. (1) to (5), correct? Shouldn't the "t" have a particular symbol then? Like t^* or so?
Response: It is, in fact, $t^\star$. We also expanded the explanation around this a bit further.

17. **Comment:** P2768 L3: Is the number 109 correct? Or should it read 1308 (=12*109)?
Response: 109 is correct, since we are using annual time steps. We added a specification why it is 109.

18. **Comment:** P2768 L5-7: Not sure again: Do you run the glacier model over 109 years for each of the 109 possible μs?
Response: That is correct.

19. **Comment:** P2768 L11-15: So the assumption is that the glaciers of a given region are in equilibrium with a climate all at the same time, right? It may be worth stating it explicitly. But isn't there an elevation dependence to be expected? Is the sentence at L20-21 going in that direction?
Response: They would not have been in equilibrium at the same time. Rather,

under the forcing of that period, they would have responded by eventually obtaining their present day hypsometries. We expanded the explanation around this.

20. **Comment:** P2768 L16-17: Interpolating the bias? What is the argument for doing that? And how do you justify it?
Response: In most cases, $\beta^\star$ is very small, since it is by definition the minimum of the 109 values of β we obtain, and for most glaciers b crosses zero. However, there are some glaciers for which $t^\star$ presumably lies outside the CRU period (1901-2009). We introduce $\beta^\star$ in an effort to take into account that this may be the case.

21. **Comment:** P2768 L25: You are certainly aware that there is RGI v3 available by now. Do you stick to v1 for allowing direct use of your previous results? It may be worth stating it.
Response: Both for comparability, but also for practical reasons: The separation of ice complexes into individual glaciers needs lots of manual quality control. We are working on more reliable automatic ways to do this, but for the moment, we could not afford to repeat the process.

22. **Comment:** P2770 L1: What are the "statistics" you need?
Response: Minimum, mean, and maximum elevation. Clarified in the manuscript.

23. **Comment:** P2770 L1-2: Where did you get the information about "year of observation" from? RGI v1 did not include such info!
Response: In some regions it did, and for the other regions there are either years, or date ranges, given in the meta data. Since this is approximate in some cases, we increased the error we assume for the area measurement (described in Marzeion et al., 2012, TC).

24. **Comment:** P2770 L4-7: Wow! In each iteration step you need re-computing all $*$

variables, don't you? How often do you need to iterate for achieving conversion? And how do you afford iteration at all, with about 170,000 glaciers contained in RGI? (This is rather a curiosity than a question that needs an answer in the paper :-))
Response: The * variables do not have to be determined in the iteration for each glacier, since $t^\star$ is interpolated, and $\mu^\star$ does not depend on past glacier hypsometry. We get a convergence typically after 10 iterations, and need a few seconds of cpu time for each glacier. Since the model is readily parallelized, a global run of a few hundred years takes a few days on a few cpus. However, the iteration does not scale well going much further into the past than 150 years...

25. **Comment:** P2770 L25: What are "climatologies of the anomalies"? Should the climatology (= mean) of an anomaly not be zero by definition? Is this "climatology of the anomalies" something similar as the "delta" that is used in delta-change methods?
Response: With "climatological", we only refer to taking the 30 year mean.

26. **Comment:** P2771 L16: Please quantify "small": Is it < 1%, < 5%, < 10%?
Response: < 5% – we added this to the caption of Fig. 1.

27. **Comment:** P2771 L7-8: I don't understand what you mean: Is the error getting larger with increasing length of the modeling period? Or is it the other way round?
Response: Exactly. The error would keep growing even after the glacier is in equilibrium, since it is determined from how the model performs driven by climate observations, when the glaciers are not close to an equilibrium.

28. **Comment:** P2771 L9: I would state 128$\pm$27 mm, instead of "of the order of 130mm". The same comment applies for the other numbers given in the section.
Response: We state the exact numbers now. Note that they have changed, since we do not upscale for Antarctic peripheral glaciers anymore.

29. **Comment:** P2771 L18-21: Oh! This is quite an important statement, isn't it? According to what you state we (= the world's climate) are close to a "change point"! You may want to highlight this finding!
Response: Technically, this can be considered a turning point – but given the small absolute amplitude ($<$ 5 % of the glacier volume), it is probably more of academic interest only.

30. **Comment:** P2771 footnote: How did the "upscaling" took place? More information is required! Moreover, why didn't you use another volume estimate, in order to avoid the strange $>$100% volume loss? And you don't need to take too serious the even more annoying question about why you didn't omit these glaciers from the estimate ;-)
Response: This problem is solved, since we decided to omit these glaciers from the estimate :-). We first did not want to do so for the sake of comparability with the Marzeion et al. (2012) paper, but that can be maintained by applying a scaling factor that is nearly independent of the climate model applied. We updated the text and all the figures accordingly.

31. **Comment:** P2772 L4: It seems appropriate to me adding a sentence like "[...] since energy input from liquid precipitation was neglected." (or similar) at the end of the line.
Response: Done.

32. **Comment:** P2772 L9-12: Stating that ignoring warming precipitation changes according to 4K warming would do x and y sounds rather awkward to me. Wouldn't it be more appropriate speaking about a given percentual precipitation variation? The same comment applies to Fig. 4.
Response: That is problematic, since the percentage precipitation change at the glacier sites varies a lot between models. Since we are trying to isolate the precipitation and temperature effects leading to the combined response in Fig.

2, it would also not help to use idealized precipitation scenarios. However, we changed Fig. 4 to show absolute values instead of anomalies with respect to Fig. 2, which makes it a lot more intuitive to understand.

33. **Comment:** P2772 L12: At "lower" than what? The 4K mentioned before?
Response: This sentence was deleted, since Fig. 4 was changed.

34. **Comment:** P2773 L5-7: The positive correlation is easily explained, but what's about the "amplification"? Can you give an explanation similar to what you gave for temperature? I found it quite illuminating :-)
Response: It's generally found that precipitation increases by more than the global mean in mid- to high latitudes, e.g. see Fig SPM-7 of the IPCC's AR4. This has to do with general circulation as well as thermodynamics.

35. **Comment:** P2773 L12-ff: Ok, these scenarios are rather funny, aren't they? The second would imply only reduction of width and thickness, the third implies area expansion at high elevation, and the third one even implies a moving bedrock! It may be worth stating that you are well aware that these scenarios are purely synthetic. No modeler would ever consider using such things – I hope ;-) Moreover, I would suggest giving a "title" to each of the scenarios, since your following subsections refer to these scenarios. Taking your subsection titles, I would suggest "Constant hypsometry, infinite ice volume"; "Constant terminus elevation, variable hypsometry and volume"; "Constant surface area, variable hypsometry and volume"; and "Constant hypsometry, variable volume". The following subsections should then be named accordingly, and the same wording used in the caption of the figures. And by the way, you may want to cite Gabbi et al., HESS, 2012, who performed similar sensitivity experiments addressing the ice volume distribution at the catchment scale.
Response: We had some discussions about these sensitivity experiments already before the submission to TCD, and since all the reviewers agreed that they

are highly theoretical, we decided to leave out the two most artificial ones. In the revised paper, we only look at "constant hypsometry, infinite ice volume" (this scenario actually has been used in the literature), and "constant terminus elevation, variable area and volume" – since this allows to isolate the effect of glacier terminus retreat (or advance) on the mass balance.

36. **Comment:** P2774 L4-6: State explicitly that the model by Marzeion et al. (2012), is accounting for all effects simultaneously! At the moment, you are only implying it.
Response: Done.

37. **Comment:** P2774 L15-16: At this stage it is unclear why you need the two different weighting methods. I was wondering if you need the weighted version at all, since the result is the same at this stage. The difference becomes clear only later. You may give a hint here, or introduce the weighting only when you need it.
Response: We removed this part of the sentence.

38. **Comment:** P2774 L20: Here and after: What "seasons"? Do you mean "melt and rain season"? State it explicitly!
Response: Yes, accumulation and ablation seasons - text updated.

39. **Comment:** P2774 L26: Ok, 1mm SLE yr^{-1} is the value of your function for ΔT=0. State it explicitly.
Response: At ΔT=0, or: a contribution independent of ΔT. Clarified in the text.

40. **Comment:** P2775 L11-16: It is really not clear to me how you can make this attribution... Clarification is required.
Response: This is based on the assumption that the length of the ablation season is linearly related to the annual mean temperature. Clarified in the manuscript.

41. **Comment:** P2775 L20-24: Shouldn't you compare the coefficients of a linear fit first? The numbers would be different than now (actually they would be even higher), wouldn't they?
Response: The way the model works, one would expect to have a quadratic relation with temperature. Therefore, we prefer to discuss it first. We are including the linear relation only in order to be able to directly compare with previous studies.

42. **Comment:** P2775 L24: Why "variability"? You are using the past climate as such (i.e. same mean temperature etc.), not only the variability, isn't it?
Response: Agreed, we deleted "variability".

43. **Comment:** P2776 L1-5: Well, if the "sensitivity" is a function of the glacier state itself, of how much use is the concept at all? In particular, how do you argue for using a constant "sensitivity" in your model, that is run for 300 years or so?
Response: Good question! However, note that the sensitivity of a glacier to a certain climate change in our model is not fixed (even is $\mu^\star$ is constant in time), since changes in the terminus elevation and surface area will make the glacier more sensitive to a given ΔT if the glacier is large, and less sensitive if the glacier is small.

44. **Comment:** P2776 L14-19: Isn't this rather trivial? Why would someone expect something else?
Response: At least it has not been acknowledged in some high profile publications - see in the conclusions section.

45. **Comment:** P2777 L2: Why "negative feedback"? Higher terminus elevation means higher, (i.e. less negative) mass balance, isn't it? This is probably called a "positive feedback".
Response: No, it's actually a negative (i.e., stabilizing) feedback: negative MB $\rightarrow$ higher terminus $\rightarrow$ more positive MB.

46. **Comment:** P2777 L6-7: The sentence is unclear: Did the two mentioned references allowed for adjustment of the terminus or not?
Response: Terminus elevation is not allowed to respond in those references. Clarified in the manuscript.

47. **Comment:** P2777 L16: "close to equilibrium": Well, "equilibrium... that's rather the state when all glaciers are gone!
Response: Yes - but they are still close to that in the sense that the mass loss rates are low, see Fig.

48. **Comment:** P2777 L19-23: I do not understand what you want to say. In particular: What does your last sentence imply?
Response: During the transient period, the glacier terminus may be both higher or lower than the equilibrium value. What is the case depends both on how strong climate variability is compared to climate change, and what the response time of the glacier is. Therefore, it can be expected that the relation shown in Fig. 11 is more noisy during periods of high mass loss rates, which indeed is the case. Clarified in the manuscript.

49. **Comment:** P2778 L3-4: "weaker" and "stronger weakening" compared to what? Shouldn't the next sentence then begin with "In contrary..." in order to illustrate what you mean?
Response: This part of the manuscript was removed.

50. **Comment:** P2778 L5-27: If I'm honest, this is the only part of the paper I didn't liked: The issue you are addressing seems to me a very particular artifact that may occur when a glacier that has reached zero volume is not defined as "gone" in a model. This is a "bug" which is that easily fixed... I don't think there is the need of much discussion for that. By far more important is what happens during the time the glacier has a volume $>$0. However, this point is hardly addressed at

all. I would appreciate more discussion on that.
Response: This part of the manuscript was removed.

51. **Comment:** P2779 L26: Have you an explanation for why the precipitation compensation is smaller in your case?
Response: The Giesen & Oerlemans model as a few more parameters, and depends on more atmospheric variables. Since atmospheric variables cannot generally be considered to be independent of one another, it is not surprising that sensitivities to one of them are different between models. But it would probably take some quite detailed analysis to pin down what exactly causes this, and we would prefer not to speculate.

52. **Comment:** P2782 L1-2: What do you mean? This is not clear to me.
Response: We mean the complete loss of glaciers – clarified in the manuscript.

Stylistic comments

1. **Comment:** P2762 L1: "," after "the past"
Response: Done.

2. **Comment:** P2762 L6-8: It took me quite a while understanding what you mean. Maybe placing "to a large degree" after "is governed" (instead of vice versa) would help? Or maybe you can reformulate the sentence.
Response: Done.

3. **Comment:** P2762 L1-23: The wording "mass loss" occurs 7 times in about 10 sentences! There is certainly a way of avoiding that much repetition :-)
Response: True, it's a lot of "mass loss"es, but the advantage is that it's nonambiguous this way, which we prefer over variety.

4. **Comment:** P2762 L15: "it" should read "is" or "will be"
Response: Done.

5. **Comment:** P2762 L16: consider "potential" instead of "projected"
Response: Since we show that it not only affects the potential mass loss, but also the actually projected mass loss (which is less than the potential), we prefer to keep it this way.

6. **Comment:** P2763 L4: "probably" can be removed.
Response: Leclercq et al. (2011) is the only reference for the global scale we are aware of the goes beyond 1850, and the error bars at the time are large enough that it might have been a few decades sooner or later – we prefer to keep "probably".

7. **Comment:** P2763 L13: Start a new sentence after the first "total mass".
Response: Done.

8. **Comment:** P2763 L16-17: The construction for the citation looks somewhat involved. Try to simplify the whole sentence.
Response: Done.

9. **Comment:** P2763 L23: Why don't you give only the range (as you did in the previous line)?
Response: Done.

10. **Comment:** P2764 L1: I think you can remove the footnote.
Response: Done.

11. **Comment:** P2764 L5-10: This is a good example for a rather complex sentence. At L5 you can remove "overlap of"; "of the ensemble of climate projections" can be replaced by "in the climate projections"; and L8-10 could almost be omitted. Try to be as concise as you can.

Response: Turned into 3 sentences. Lines 8-10 are rather important, since we hope to explain later on why quite different climate scenarios may produce quite similar mass loss rates.

12. **Comment:** P2764 L11-12: Also this sentence may be removed.
    Response: See above - we prefer to keep this sentence.

13. **Comment:** P2764 L17: "However," instead of "But"
    Response: Done.

14. **Comment:** P2765 L1: Remove "conditions"
    Response: Done.

15. **Comment:** P2765 L6: "To date" instead of "But"
    Response: Done.

16. **Comment:** P2765 L11: Remove "- but not all –", or explain which of the "relevant mechanisms" are not "captured".
    Response: Done.

17. **Comment:** P2765 L17: Remove "by keeping various aspects of glacier hypsometry fixed in the model" – at this stage it is unclear what you mean.
    Response: Done.

18. **Comment:** P2766 L6: "location and elevation of the" can be removed.
    Response: Done.

19. **Comment:** P2766 L25: "through the glacier steepness" can be removed
    Response: Done.

20. **Comment:** P2767 L5: Remove "roughly"
    Response: Done.

21. **Comment:** P2767 L8: Remove "surface"
    Response: Text changed.

22. **Comment:** P2768 L8-9: reword to "[...] the corresponding temperature sensitivity μ^* yields the smallest difference between modeled and observed mass balances"
    Response: Done.

23. **Comment:** P2768 L14: Add "by solving" (or similar) after "i.e."
    Response: Done.

24. **Comment:** P2770 L14: Remove "glacier" in "leave-one-glacier-out".
    Response: Cross validation is often done temporally, and could also be done temporally here (i.e., base the calibration for each glacier on a number ob observed years except for a left-out window) - we prefer to be specific here.

25. **Comment:** P2770 L20: Maybe you can make an introductory sentence before you start with the individual subsections. It would be easier to follow if one knows what you are aiming at.
    Response: Done.

26. **Comment:** P2771 L3-8: You can write these sentences without brackets.
    Response: Done.

27. **Comment:** P2771 L17-19: I would consider removing the sentence. It opens more questions than it answers: What is a "true equilibrium" (why "true"?)? What does it mean that a climate system "is not itself an equilibrium"? The wording doesn't make much sense to me (of course a climate system is not an equilibrium; it is a climate system ;-)).
    Response: We removed "true", and note we are saying the "state of the climate system" is not an equilibrium. We think it's worthwhile to make this point, because its sensible to assume that the glaciers might respond differently to equilibrated climate forcing – it's just not practicable to investigate.

28. **Comment:** P2771 L5: Remove "relative to the same time period"
Response: Done.

29. **Comment:** P2771 L19: 15mm SLE K^{-1} is the "glacier sensitivity", not the "glacier mass loss", right?
Response: Done.

30. **Comment:** P2772 L12: Move "(400mm SLE)" at line 10, after "mass loss"
Response: Text deleted.

31. **Comment:** P2774 L14: "easier to understand" than what? What do you mean?
Response: The text says "rates of glacier mass loss are easier to understand than accumulated mass loss", and the explanation is before that: "subjecting the glacier to climate change essentially becomes equivalent to transplanting a glacier into a future climate, without taking into account the temporal evolution of climate and glacier change that lead there"

32. **Comment:** P2774 L24-25: You can remove "mean goodness of fit (measured by" - and the closing parenthesis of course.
Response: Done.

33. **Comment:** P2774 L27: Insert "as" before "discussed", and "in" after it.
Response: Done.

34. **Comment:** P2775 L10: Start a new sentence after "and ablation".
Response: Done.

35. **Comment:** P2776 L11-12: Reword into "The remaining differences are due to the patterns in precipitation anomaly, and history [...]"
Response: Done.

36. **Comment:** P2776 L20-25: Most of this paragraph is repetition. Moreover, the writing style is rather colloquial. I would suggest removing the paragraph (or reformulating and condensing it).
Response: The point here is that what was said before is true also for quite small mass losses. We reworded the sentence.

37. **Comment:** P2777 L9-10: Maybe I don't understand what you mean, but "higher mass-loss rates" are the only way for getting a higher volume change in the same time frame, isn't it?
Response: Yes, but you could get a higher mass loss in the end, without having higher rates "at all times".

38. **Comment:** P2777 L11: "presence" (or something else) instead of "availability"
Response: Done.

39. **Comment:** P2777 L17: Check the wording: What does it means that the "effect" is a "function" of something? How do you measure this "effect"?
Response: Essentially, Fig. 11 shows that terminus elevation changes are a negative feedback, and that the strength of the feedback depends on past mass losses. Therefore, the "effect" is a function of "past mass losses". We reworded the sentence in the conclusions to better reflect this.

40. **Comment:** P2777 L27: Consider "arguably" instead "presumably"
Response: Text deleted.

41. **Comment:** P2779 L3: Remove "us"
Response: Text deleted.

42. **Comment:** P2779 L7: "large", not "great"
Response: Text deleted.

43. **Comment:** P2779 L15: State the two “reference times” explicitly.
Response: The reference times are given in the next line.

44. **Comment:** P2779 L16: Repeat your number when saying "out estimate"
Response: Done.

45. **Comment:** P2780 L19: "Our" instead of "The"
Response: Done.

46. **Comment:** P2780 L16-ff: Split this sentence!
Response: Done.

Comments to Figures

1. **Comment:** Fig. 1: Remove the "two sigma"-shading. It is hard to see, and complicates the figure.
Response: Done.

2. **Comment:** Fig. 2: (1) Why are some lines stopping at $\Delta T \approx 4$? Do these model runs go up to a certain period only? (2) Avoid the abbreviation "% w/o AA" in the y-axis.
Response: (1) The maximum temperatures reached by the different GCMs depend on their response to the forcing, and of course on how long their projections are. Some don’t even get to ΔT = 3 K. (2) Since we omitted the upscaling, this has become unnecessary.

3. **Comment:** Fig. 4: x-axis should read "precipitation anomaly associated with..."
Response: The x-axis shows temperature values and therefor should be labeled with that. However, we reworded the caption in order to make clearer what is meant.

4. **Comment:** Fig 6. Reword the caption into "Global glacier mass balance, expressed in terms of global mean sea level equivalent (SLE), as a function of anomalies in (1) global mean (open circles, dashed lines) and (2) glacier area weighted (filled dots, solid lines) temperature. Results refer to the "constant hypsometry, infinite volume" scenario. Colored lines show [...]. Different panels refer to different RCPs" The statement about the different panels should be included in Fig. 8, 9, and 10 as well.
Response: Done.

5. **Comment:** Fig. 8: Add "(fully adjusted geometry)" (or similar) to the x-axis label. Can you put a time frame until when this mass losses are expected to occur?
Response: Done. The time-dependent behavior is shown in Fig. 7.

6. **Comment:** Fig. 9: Why this different behavior in the different RCPs? In (a), (b), and (d) the different glacier-adjustment scenarios cluster very differently: By 2300, they are more or less equally spread for RCP 26; in RCP45 "terminus & area fixed" is almost the same as "full"; and in RCP85 "full"+"terminus fixed" and "area fixed"+ "terminus & area fixed" cluster at two different levels. Why is that? Doesn’t this compromise the generality of the results that you have described in the text? And by the way, what’s the "jump" around year 2100 in RCP26?
Response: With the removal of half of the experiments, the "clustering" is gone (it was caused by one feedback dominating others, which was a result of the feedback strength depending on mass losses, and mass losses depending on scenario). The "jump" in 2100 comes from a change in the number of model ensemble members.

Response to Paul Leclercq:

Major Comments

1. **Comment:** Calibration and interpolation of mass balance parameters
   The mass balance model is very simplistic, which to a certain degree is understandable in a study of global glacier changes. The calibration of this mass balance model on observations (p 2767 ln 14 - p 2768 ln 10) makes a detour by looking for a period in which the present glacier geometry would have been closest to balance. For the glaciers for which no mass balance measurements are available, the time of this period is determined by spatial interpolation and subsequently used to derive the MB parameters for these glaciers. This is a very peculiar procedure: if e.g. for one glacier with MB observations the equilibrium period is found to be centered around 1910 and for another glacier with MB observations the period is centered in 1970, then for a third glacier in between these two the period will be centered around 1940, while in reality this could be a period of strong disequilibrium for all three of the glaciers. I do not see a solid ground for this spatial interpolation of the timing of periods. The fact this procedure gives smaller errors in the cross validation than direct interpolation of the parameters is not enough to justify it. With such a simplified approach compensating errors cannot be excluded and the method should be legitimate on a priori grounds. I therefore think the determination of model parameters for glaciers without mass balance observations should be changed.
   Response: We agree that the smaller error of the parameter μ from the interpolation of t^* rather than from direct interpolation alone does not justify our procedure, but it serves as an illustration that the detour is actually worth taking.
   Most importantly, note that $t^\star$ is *not* a period where we assume the glaciers to be in equilibrium. Instead, the time difference now-$t^\star$ mainly reflects the timescale of

   response of the glaciers. We implicitly assume that this is a regional characteristic, which is sensible since it depends quite a lot on climatological precipitation (the other assumption that is needed is that climate variability also is regionally characteristic, since it will influence the actual lag, and thereby $t^\star$).
   There is another reason for keeping the parameter estimation this way: One motivation for this study is the explanation of the mechanisms at play in the results in Marzeion et al. (2012). It would not be meaningful to do this with a fundamental change in the model setup.
   In the manuscript, we extended the section describing the model. Particularly, we explain that $t^\star$ is not a period of equilibrium, but that if the climate of time $t^\star$ had been maintained, the glacier eventually would have contracted until it reached its present-day hypsometry.

2. **Comment:** Uncertainty/error analysis
   The uncertainty in the model parameters is determined using a leave-one-out cross validation. For a proper uncertainty estimate this method requires independent observations. This is problematic as the mass balance records against which the glacier model is tested are often strongly correlated if they are within the same region, and therefore not independent. The problem is amplified by the inverse distance interpolation, which gives nearby measurements larger weight in the model fitting. This probably leads to an underestimate of the uncertainty in the model results. Therefore the uncertainty has to be re-evaluated, for example by using samples of correlated MB records instead of a leave-one-out procedure in the cross validation.
   Response: The reviewer addresses an important point here, and arguably one we didn't discuss appropriately in Marzeion et al. (2012), even though we did in fact do what the reviewer suggests (note however that for the determination of the total error it is not the correlation of the mass balances themselves that is important, but the correlation of the errors of the modeled mass balances). If the

correlation of errors was 1, the total error would be the sum of the errors; if the correlation of the errors was 0, the total error would be the root of the squared error sum.
The mean correlation of the mass balance errors in our model is 0.025. But more importantly (and answering the reviewer's comment), we find no systematic dependence of the correlation of errors between two glaciers on the distance between those glaciers. The correlation of the correlation of errors with the distance between the glaciers is 0.008. So while there is correlation in the mass balances of nearby glaciers, there is hardly any correlation in the errors of the modeled mass balances of nearby glaciers, such that we can treat those errors as independent.
We added a paragraph to the manuscript explaining this.

Other points

1. **Comment:** p 2762 ln 15: is liquid
   Response: Done.

2. **Comment:** p 2765 ln 6-10: these issues have been discussed before, also at a global scale: e.g. Zuo and Oerlemans (1997) have stressed the importance of the imbalance between climate and glaciers at the start of SLR calculations, and Oerlemans et al 1998 also discuss the effect of changing glacier geometry on the response to future warming for a sample of 12 glaciers modelled with dynamical glacier models.
   Response: We added the two references, and specified in the manuscript that to date, there is no global *quantification*.

3. **Comment:** p 2766 ln 11: the gridded climate data sets most likely underestimate the precipitation on glacier as they do not capture the orographic effects. Is this

accounted for in some way?
Response: Yes. There is both a precipitation lapse rate, and a general correction factor. This is described in detail in Marzeion et al. (2012). It's hard to decide what to include in the model description in a manuscript that relies on a previously published and described model. We decided not to repeat the full description for sake of brevity in this manuscript, and point the reader to the published model description.

4. **Comment:** p 2771 ln 3: use the same units throughout the paper: cm SLE or mm SLE
   Response: It's hard to reconcile the reviewers' requests here... In the end we decided to keep it mixed, but to be more consistent (e.g., all annual rates are in mm, all volume estimates in cm, etc. – i.e., larger numbers in cm, smaller numbers in mm).

5. **Comment:** p 2769 ln 19: here and in other parts of the study sensitivity analysis are performed based on runs with different CMIP5 outputs which are then sampled in steps of 1 K temperature change. This results in rather crowded figures (Fig 2, 3, 4, 6) and the combination of the effects of spatially hetereogenous climate change and change in glacier geometry. Firstly, I think that for this sensitivity study not all climate models and not all climate scenarios are needed. Secondly, I would suggest to use a uniform warming, or precipitation change, on all glaciers to study the effects of changes in glacier geometry, a second set of experiments to show the relative importance of temperature and precipitation changes, and finally perform a seperate experiment to show the effect of spatial differences in the projections of climate change. I think such an approach would simplify the presentation and discussion of the results.
   Response: The idealized sensitivity experiments that the reviewer suggests would indeed be interesting, but they would serve another goal than what we present here. Here, we want to address the "real world" sensitivities, and for

this, it is both necessary to use spatially heterogeneous fields, as many different GCMs as possible in order to quantify the uncertainty that is connected to projection ensemble spread, and different scenarios in order to address scenario uncertainty. Additionally, we need the different scenarios since one particular goal of the study is to find out why the glaciers respond similar to greatly differing scenarios.

6. **Comment:** p 2772 l 9: the two do not cancel, they are seperate experiments. Maybe rephrase into something like: "The mass gain calculated by increasing precipitation only is of the same magnitude as the mass loss calculated from changing both temperature and precipitation.
   Response: Agreed. We changed Figure 4 and reworded to clarify.

7. **Comment:** p 2772 p 12-14: To me it is not clear what is meant here. You mean for small climate change the mass loss calculated from the full forcing is larger than the mass gain for forcing the model with only precipitation anomaly and the other way round for large climate change?
   Response: Text deleted/clarified (see above).

8. **Comment:** p 2772 ln 23: Maybe include a reference to Gregory and Oerlemans (1998) who found the same with exactly the same explanation.
   Response: Gregory and Oerlemans (1998) rather discuss the effect of the seasonality of the arctic amplification, but we added this reference where we included the seasonality discussion in response to Alex Gardner's review.

9. **Comment:** p 2773 ln 20: keeping the area constant while increasing the terminus elevation is a very unphysical experiment. It implies that the surface area at high elevations of the mountain ranges increases with climate warming. Of course this leads to less mass loss (see p 2778), but a change in mountain topography like this is so unrealistic that I suggest you leave this experiment out.

   Response: Because of the very theoretical nature of this and the fourth sensitivity experiment, we deleted both from the revised manuscript.

10. **Comment:** p 2775 ln 17: "ignoring the" suggests that this quadratic approximation derived from eq 1 is a true, or at least better, description of real mass balance sensitivity. Although it is clear that the sensitivity is non-linear due to many feedbacks, I haven't seen any proof that a quadratic approximation is better than a linear one, so I suggest you leave the phrase (i.e ignoring ...) out.
    Response: Done.

11. **Comment:** p 2275 ln 18: are you sure this is not the number for the contribution of the Greenland ice Sheet (their page 476, 3rd paragraph, 5 line from below)?
    Response: Yes, it is, thank you! Corrected.

12. **Comment:** p 2776 ln 24: the fairly constant rate of glacier mass loss in the 20th century is an intriguing issue. The authors also seem think it is important as a large part of the discussion is devoted to the subject. Therefore, I wonder why they do not show an experiment comparing modelled mass change with geometric adaption and with constant geometry for the 20th century. Then maybe the "supporting" could be replaced with a firm answer.
    Response: During the 20th century, climate variability is of a similar magnitude as climate change. Because of the complex spatial pattern of climate variability, the response of the glacier mass balance to the experiment suggested by the reviewer is spatially complex (this is also the reason for the large error bars associated with our linear sensitivity determined from that experiment). So while we agree that this is a good idea, it would take much too much room in the present manuscript.

13. **Comment:** p 2777 ln 6: in the approach of Van de Wal and Wild (2001) and Slangen et al (2011) the mass balance is not dependent on the terminus elevation as in the model used in this study. I therefore wonder if the comparison is that

straightforward.
Response: This is exactly what we meant. Clarified.

14. **Comment:** p 2777 ln 11-12: The disappaerance of glaciers is a good explanation for the difference after substantial climatic change. However, also for limited mass loss (150-200 mm SLE, i.e. a range that is reached in both scenarios) the difference between no terminus change and full geometric change seems to be larger for RCP 2.6 than for RCP 8.5. What explanation can be given for this difference?
Response: The reason is that there is a multitude of small glaciers that disappear faster in RCP8.5 than in RCP2.6. It can also be seen in Fig. 8 that it is the case even at very small mass losses of the order of 10 mm SLE. This is mentioned where Fig. 8 is discussed.

15. **Comment:** p 2778 ln 3-4: I think you should skip this experiment (see above) but still the advise to rephrase "weak": "smaller mass loss", "with stronger reduction of mass loss for warmer"
Response: That section was deleted.

16. **Comment:** p 2779 ln 1: This experiment could be relevant for several glaciers that loose mass with a lowering of the surface but show limited retreat. However, the model used in this study cannot account for surface lowering as the mass balance is determined by the terminus elevation instead of the surface elevation.
Response: Agreed. But also this section was deleted because the experiment design had the same problems as the "constant surface area" experiment.

Response to Alex Gardner:

General Comments

1. **Comment:** My most impactful comment is to do with the design of the sensitivity experiments described in Sect. 4. Apart from holding the hypsometry constant with infinite ice volume (Sect. 4.2.1), I found the rest of the experiments poorly designed and not useful for understanding glacier behavior:

   (a) (4.2.2) Applying a constant terminus elevation simply forces the glacier to retreat to lower elevations, which will accelerate glacier loss but doesn't isolate the impact of glacier retreating to higher elevations.

   (b) (4.2.3) Simulating a glacier with constant surface area that can retreat to higher elevations as it melts is nonsensical and the fact the it is still counted as a glacier even after all the glacier ice is melted makes it confusing to interpret.

   (c) (4.2.3) A constant surface area and terminus elevation suffer from both of the problems stated above.

   I felt that the results of the simulation did not add any value to the paper and distract from the other relevant findings of the manuscript. I would suggest to only compare 4.2.1 with the full model results for characterization of the sensitivity of the glacier mass budget to hypsometric feedbacks.
   Response: We deleted (b) and (c) from the revised manuscript. Note that (a) however is a misunderstanding: We not only keep terminus elevation constant, but also glacier length, so that the glaciers do not retreat to lower elevations – they just keep their elevation range constant. This is relevant not only because it does allow to isolate the effect of terminus retreat (or advance), but also because

it prevents glaciers from reaching a new equilibrium, and it is informative to quantify how much effect this has on mass balance projections, given that there are studies that neglect this effect (e.g. van de Wal and Wild, 2001, and Slangen and van de Wal, 2011, both cited in the manuscript).

2. **Comment:** Global mean temperatures are compared with glacier area weighted mean temperatures to show that there is a nearly 2-Â■fold amplification in warming over glacier surfaces (Arctic amplification). This is then used in Fig. 6 to show that glaciers are less sensitive to temperature than may be implied from the global mean temperature. I found this analysis to be not all that useful as only changes in ablation season (summer) temperatures will influence the rate of glacier mass loss. Since there is almost no Arctic amplification in summer, the comparison between global and glacier area weighted temperatures may be a bit misleading. Seasonality needs to be accounted for. Maybe a comparison between annual global warming and ablation season global warming would be of more interest.
   Response: That's a good point, at least regarding arctic amplification. But there is also evidence in the manuscript that the spatial patterns of temperature change are relevant for explaining the global glacier response nevertheless: Tab. 3 shows that the RMSE of fitting mass loss with area weighted temperature changes is about half a big as the RMSE when using global mean temperature change. This implies that differences in the temperature change patterns of the difference GCMs are responsible for about half of the differences in the mass losses – or in other words: That area-weighted temperatures are a better predictor for glacier mass loss than global mean temperatures, even if seasonality is neglected.
   We added a paragraph to the discussion section addressing this point.

3. **Comment:** As noted by the authors, upscaling of Antarctic glacier mass change is problematic. Since these glaciers account for 25% of the global glacier volume (Radic et al., 2013) and are in near equilibrium in present climate (Gardner

et al., 2013) upscaling from the global mean may significantly bias temperature sensitivity results. Maybe it would be better to simply exclude Antarctic glaciers from your analysis? This would also fix the confusing $>$ 100% glacier volume loss shown in Fig.
   Response: We removed the upscaling for Antarctic glaciers. This somewhat reduces the comparability with Marzeion et al. (2012), but we agree that there is no solid ground for the upscaling, and in fact evidence against it.

4. **Comment:** I am also a little nervous that the optimization of $\mu*$ is designed in such that it will compensate for errors in the CRU climatology to improve the fit with observations. This can lead to unrealistic temperature sensitivities and therefore unrealistic response to changes in forcing. This comment is supported by the poor model fit when $\mu*$ is spatially interpolated. I'm not sure how the authors can best tackle this issue. Maybe they can provide some comparisons between in situ derived climate sensitivities (weather station and mass balance observations) and CRU derived climate sensitivities for a couple of different regions. I would be more than happy to provide in situ data for the Canadian Arctic.
   Response: We think it would be very valuable to compare "high quality" climate sensitivities obtained from local meteorological observations and mass balance observations with the sensitivities obtained by our method. We also appreciate the offer of in situ data and will probably come back to it later – but we think a decent comparison of this kind would be beyond the scope of this paper, and also a little out of place. It should be done nevertheless.
   However, we are not able to follow why the "poor model fit when $\mu^\star$ is spatially interpolated" supports the suspicion that the "optimization of $\mu^\star$ compensates for errors in the CRU climatology to improve the fit with observations" – in fact, we would argue the opposite: glaciers are typically situated in regions of high topographic complexity, which leads to small scale patterns of climate that are neither resolved by gridded climate observations like CRU, nor by GCMs. Even if the

ice surfaces of two different glaciers necessarily are exposed to identical energy balances under identical atmospheric conditions, one can therefore not expect for two glaciers to have identical optimal values of μ in our model, even if the two glaciers situated within the same grid cell. Obviously, gridded observations like CRU are not without fault, and particularly in remote regions, where many glaciers are located, there will be problems. If those problems are in the climate variability of the CRU TS 3.0 data set, this would become apparent in the validation with observed mass balances. If the problem is in the CRU CL 2.0 data set, the way we determine $\mu^\star$ would compensate for it (if that compensation didn't work, it would also become apparent in the validation), and since we are only using anomalies from the GCMs, the compensation would apply here as well.
Things are different of course if there are substantially more problems in the driving data sets outside of the time range of the validation than within - which is possible (maybe even likely), and discussed in Marzeion et al. (2012).

Specific Comments

1. **Comment: P2762** L4: delete "in order"
   Response: Done.

2. **Comment:** L7 "to a large degree is governed by" $->$ "is largely governed by"
   Response: Done.

3. **Comment:** L12 "are vastly stronger than on" $->$ "are amplified relative to the"
   Response: Done.

4. **Comment:** L15 change all: "at the glacier sites it liquid" to "is liquid"
   Response: Done.

5. **Comment:** L19-23: I'm not sure there has been enough glacier hypsometric change for large glacier regions (i.e. those important for sea level change) over the 20th century to support this statement. Huss (2012) clearly shows that hypsometric changes have played an important role in 20th century rates of mass loss for the Alps but Gardner et al. (2012, TC [pg.1116]) found that changes in hypsometry and glacier area over a 50 year period had little impact on the rates mass loss for Baffin and Bylot Island glaciers in the Canadian Arctic.
   Response: It is certainly not true for every glacier, nor every glaciated region. But on the global scale, we find that the (linearly approximated) sensitivity of the glaciers excluding hypsometric change is 1.25 mm SLE yr^{-1} K^{-1}, while including the effect of hypsometric changes, we arrive at 0.41 mm SLE yr^{-1} K^{-1}. These numbers are based on scenarios, not the 20th century, and we agree that the effect probably has been smaller there. We replaced "reduced by a factor of two to three" with "strongly reduced" – but we think it is still valid to claim that this will have played a role in the 20th century as well.

6. **Comment: P2763** L13-18 Are these estimates comparable i.e. do they all include peripheral glaciers? If so maybe the authors could just provide a single range followed by citations to all studies.
   Response: Yes, they all include peripheral glaciers. It's difficult to combine into one range, since the way Radic et al. do their estimate doesn't allow for "classical" error bars.

7. **Comment:** L20 "R"epresentative "C"oncentration "P"athways
   Response: Done.

8. **Comment: P2764** L3 "climate scenarios for instance" $->$ "climate scenarios. For instance"
   Response: Done.

9. **Comment:** L14 "E.g.," $->$ "For example,"

Response: Done.

10. **Comment:** L23 "times as negative as they have actually been observed" $->$ "times more negative than observed"
Response: Done.

11. **Comment:** L5-Â■6 merge into single paragraph
Response: A new paragraph was inserted between those lines in the revised manuscript.

12. **Comment:** L9 "hypsometry changes" $->$ hypsometric changes ??
Response: Done.

13. **Comment:** L12 "relevant mechanisms to" $->$ "relevant mechanisms required to"
Response: Done.

14. **Comment: P2766** L5 I think it is important to point out that in the model $\mu*$ is static and is the largest limitations of the model when studying glacier sensitivities to changes in forcing.
Response: This should become clear later in the manuscript, where we added a few explanation on $\mu^\star$.

15. **Comment:** L5 See general comment about the optimization of $\mu*$
Response: Also see the general comment for our reply :-)

16. **Comment: P2767** L7 How sensitive are your results to the selection of the length response time scale?
Response: Not very sensitive. In Marzeion et al. (2012) we wrote: "The relative uncertainty of the response time scale of a glacier's surface area and length to changes in volume is estimated high at 500 %, following the analysis of explicitly modeled glaciers' response times, integrating an ice dynamics model of

a glacierized mountain range over $>$ 1000 yr (Jarosch et al., 2011). Even so, the uncertainty that enters our model through the time scales of glacier response is small compared to those entering through the mass balance, and the volume-area and volume-length scaling."

17. **Comment:** L9-Â■13 Is this an appropriate assumption for ice caps?
Response: Probably it's not, and should be changed in the future. Note however that the response time scale's uncertainty is estimated high (see above), and that many of the smaller ice caps in the model aren't treated as ice caps, but divided into glaciers based on the topography of the ice surface (which admittedly may cause it's own problems).

18. **Comment: P2768** L7-9 The fact that the interpolation of $t*$ produce a better fit than the interpolation of $\mu*$ support my earlier comment about the optimization of $\mu*$. Some spread in $\mu*$ is to be expected but I would expect strong spatial correlation. I don't think L20-Â■22 is necessarily what we would expect on a regional scale in nature.
Response: As said above, we have difficulty following the reasoning. But perhaps this helps: for the glaciers with mass balance measurements, we find greater spatial coherence in $t^\star$ than in $\mu^\star$, such that $t^\star$ is a better regional characteristic than $\mu^\star$.

19. **Comment:** L23 to P2769 L9 Should this come before the model description?
Response: We can see your point. However, it seems natural to start the section with the mass balance equation (Eq. 1), after which the variable need to be explained.

20. **Comment: P2771** L4 8.1 $+/-$ 0.3 cm sea level rise $->$ 8.1 $+/-$ 0.3 cm SLE
Response: Done.

21. **Comment:** L9 130 mm SLE K^{-1} $->$ 13 cm SLE K^{-1}

Response: Done.

22. **Comment:** L20 vanishing of the dampening effect for delta T >1K seems very low considering the vast majority of glaciers are located at the very cold poles and will still have relatively short melt durations with a 1K warming. Antarctica and the Canadian Arctic North contain 33% of the glacier area and will only experience a 2-3 month melt season with 1K of warming. Maybe this is sufficiently offset by other regions.
Response: True - it's not vanishing, it just becomes smaller. Corrected.

23. **Comment: P2772** L15 see general comments about summer versus annual T
Response: We added a paragraph to the discussion – also see reply to the general comment above.

24. **Comment:** L27 Changes in terrestrial snow also play a large role in the arctic amplification
Response: Added to the sentence.

25. **Comment: P2773** L3-7 Are GCM projections of precipitation over mountainous regions reliable enough to say anything meaningful about precipitation changes over glaciers or will we need to wait for further improvements in GCM model physics and resolution?
Response: Probably you can even say that they are reliable enough only over relatively small parts of the planet, and Fig. 5b illustrates the large scatter.

26. **Comment:** L17-22 I would recommend removing this section
Response: The latter part was removed – see reply to the general comment above for the first part.

27. **Comment: P2774** L1-3 I would recommend removing this section
Response: Done.

28. **Comment:** L15 See general comments about mean annual versus glacier weighted temperature
Response: See reply to general comment above.

29. **Comment:** L26 It is not readily clear to me why the 1 mm SLE yr-1 does not match initial rates of mass loss from the equilibrium studies. I must have missed something.
Response: It actually does match the initial rates, it's just hard to see in Fig. 1 since there is quite a rapid decline in the first few years.

30. **Comment: P2775** L18 & 19 "estimate it to" $->$ "estimate it to be" occurs twice
Response: Corrected.

31. **Comment: P2776** It would be very helpful to examine the seasonality of the forcing when discussing spatial heterogeneity to forcing.
Response: Agreed. Unfortunately, it is not simple to do for us, as the length of ablation seasons is glacier-specific and variable in time, and we did not save this information from the model runs. But we do mention the importance of seasonality on the new paragraph in the discussion.

32. **Comment:** L24-26 I agree that hypsometric feedbacks have contributed to 20th century rates of mass loss but I would be very cautious about making any broad conclusions as to why earlier rates of glacier mass loss were as negative later rates. I suspect that earlier estimates may be revised downward as speculated by Gardner et al. (2013).
Response:

33. **Comment:** Sections 4.2.2 - 4.2.4 I would remove these entirely.
Response: Sections 4.2.3 and 4.2.4 were removed. For Sect. 4.2.2, see above.

34. **Comment: P2779** L10 If might be helpful to reference recent observational studies such as Jacob et al (2012) and Gardner et al. (2013) to support this statement.

Response: References added.

35. **Comment: P2780** L3-7 See general comment about hypsometric changes for large regions.
Response: See response to specific comment No. 5 (we are assuming this is what you mean).

36. **Comment:** Fig2 % volume loss $>$ 100% is just confusing. Maybe just exclude Antarctic glaciers if you are unable to model them
Response: Antarctic glaciers were excluded.

37. **Comment:** Fig4 Not all that intuitive to understand. Maybe just remove this figure.
Response: We show absolute values instead of anomalies now – this should make it easier to understand.

38. **Comment:** Fig5 & 6 see general comment about mean annual temperatures
Response: See replay to general comment.

39. **Comment:** Fig7, 9, 10 Why not show 1&2 standard deviations for consistency?
Response: Done.

40. **Comment:** Fig6-10 and decimal to RCP IDs (i.e. RCP85 $->$ RCP8.5)
Response: Done.

Interactive comment on The Cryosphere Discuss., 7, 2761, 2013.