Review of: Feedbacks and mechanisms affecting the global sensitivity of glaciers to climate change

Summary:
In this study Marzeion and colleagues determine the "committed" glacier mass loss required for glaciers achieve equilibrium with 1961 to 1990 climate and predicted rates of glacier loss associated with different global temperature anomalies. They also explore the impacts glacier surface elevation and area feedbacks on glacier mass balance by rerunning their model with different feedbacks tuned on and off in their model. The study highlights the impact of "committed" mass loss on future rates of glacier mass loss, the importance of accounting for precipitation phase changes when determining the impact of increased precipitation on glacier mass balance in a warming climate, and the impact of hypsometric feedbacks on century-scale glacier mass budget simulations.

General Comments:
I commend the authors for their commitment to modeling the global glacier mass budget and understanding its sensitivities and feedbacks. Modeling such a spatially and temporally heterogeneous system is exceeding challenging. The authors are leaders in their field and the manuscript is well-written and easy follow and the figures are excellent. I do however have some suggestions. These are only suggestions for improvement and should not prevent the publication of the manuscript if the authors choose not to incorporate the changes.

1. 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.

2. Global mean temperatures are compared with glacier area weighted mean temperatures to show that there is a nearly 2-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.

3. 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.

4. I am also a little nervous that the optimization of μ^* 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 μ^* 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.

Specific comments:
P2762
L4: delete "in order"
L7 "to a large degree is governed by" -> "is largely governed by"
L12 "are vastly stronger than on" -> "are amplified relative to the"
L15 change all: "at the glacier sites it liquid" to "is liquid"
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.

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.
L20 "R"epresentative "C"oncentration "P"athways

P2764
L3 “climate scenarios for instance” -> “climate scenarios. For instance”
L14 “E.g.,” –> “For example,”
L23 “times as negative as they have actually been observed” -> “times more negative than observed”
L5-6 merge into single paragraph
L9 “hypsometry changes” -> “hypsometric changes” ??
L12 “relevant mechanisms to” -> “relevant mechanisms required to”

P2766
L5 I think it is important to point out that in the model μ* is static and is the largest limitations of the model when studying glacier sensitivities to changes in forcing.

L5 See general comment about the optimization of μ*

P2767
L7 How sensitive are your results to the selection of the length response time scale?
L9-13 Is this an appropriate assumption for ice caps?

P2768
L7-9 The fact that the interpolation of t* produce a better fit than the interpolation of μ* support my earlier comment about the optimization of μ*. Some spread in μ* is to be expected but I would expect strong spatial correlation. I don’t think L20-22 is necessarily what we would expect on a regional scale in nature.

L23 to P2769 L9 Should this come before the model description?

P2771
L4 8.1 +/- 0.3 cm sea level rise -> 8.1 +/- 0.3 cm SLE
L9 130 mm SLE K^-1 -> 13 cm SLE K^-1
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.

P2772
L15 see general comments about summer versus annual T
L27 Changes in terrestrial snow also play a large role in the arctic amplification

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?

L17-22 I would recommend removing this section

P2774
L1-3 I would recommend removing this section

L15 See general comments about mean annual versus glacier weighted temperature
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.

P2775
L18 & 19 "estimate it to" -> "estimate it to be" occurs twice

P2776
It would be very helpful to examine the seasonality of the forcing when discussing spatial heterogeneity to forcing.

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).

Sections 4.2.2 – 4.2.4 I would remove these entirely.

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.

P2780
L3-7 See general comment about hypsometric changes for large regions.

Tables
good

Figures
Excellent

Fig2 % volume loss > 100% is just confusing. Maybe just exclude Antarctic glaciers if you are unable to model them

Fig4 Not all that intuitive to understand. Maybe just remove this figure.

Fig5 & 6 see general comment about mean annual temperatures

Fig7, 9, 10 Why not show 1&2 standard deviations for consistency?
Fig6-10 and decimal to RCP IDs (i.e. RCP85 -> RCP8.5)