I have read the paper a second time after the author's revision. The authors invested a lot of energy in the revision, which is recommendable. Some of my comments have been addressed, other have been ignored or implemented differently. Some model results are still dubious to me (mostly: the surprising seasonal mass-balances and the mass fluxes per elevation band). Overall I am still convinced that there is a potential for model improvements, but the current version of the paper discusses the model limitations in an appropriate way.

I have a few minor comments listed below and would like to take the opportunity to reply to three topics raised in the interactive discussion.

Points raised in the discussion

From the three points below, only point 1 needs concrete action in the manuscript. Points 2 and 3 are here just for the sake of scientific debate.

1. Elevation feedback

From the author's response to Reviewer #1 and myself I found two contradicting statements:

To reviewer #1 you wrote: "*The negative feedback between terminus elevation and mass balance is missing and the only way for a melting glacier to reach equilibrium with climate is by melting completely.*" And to my question about whether glaciers can melt on elevation bands you write: "*This allows glacier tiles to gain or lose mass at elevation bands"*. I still have trouble to understand what you actually mean in your answer to reviewer #1 and in the text: if you loose mass at an elevation band you could include an elevation feedback by letting the band's elevation decrease until the bedrock is reached (which you probably won't do because of obvious complications in the code). However, you are able to stop the melt when an elevation band is melted completely. So some negative feedback should already be included in your model, and you might revise the answer to reviewer #1 by saying that the *area* is left unchanged, which is better than leaving the entire elevation band after it has melted.

2. Regional parameter sets.

To my comment about regional calibration, you write: "*It is not clear why a single global parameter set would be more robust than regional parameters sets*."

Let me make an example based on JULES. How would it be if the model parameters for, say, "clay porosity" or "tree leave albedo" would be different between England and Wales? The equations of wind motion or ice melt do not follow arbitrary frontiers. I might be wrong, but this glacier module is probably the first module in JULES to use regional parameter sets.

Don't get me wrong: I understand that parameters need to be tuned, especially in a "physically based model" with many parameters. I just say that using parameters based on RGI regions is suboptimal, for several reasons:
- it creates unphysical differences between neighboring regions (such as 13, 14, 15 in High Asia)
- it hides model deficiencies (or errors in forcing data) by tuning the model on a smaller set of observations (sometimes only one or two glaciers per region)
- in a global model like JULES, it will hinder the acceptance by the wider community and the module will have more difficulties to enter the main codebase

3. Energy balance

In the revised version you added analyses of mass fluxes (which can be done by more simple models like degree-day models as well), but not of energy fluxes. I believe this is a missed opportunity.

Detailed comments

P6 L26-27: remove "this is because..."
P9 L6: add Marzeion et al., 2012 to the references list.
P10 L23: please add reasons for the negative bias. In linear a model with enough degrees of freedom, minimizing RMSD will always minimize the bias too. So the first thing that comes to mind is stat systematic problems in the model and/or the forcing data are preventing this bias minimization (confirmed by the supplementary analyses). In short: there seems to be a structural problem in either the model or the forcing data.
P10 L29: "Our mass balance model does include sublimation". I am curious: since you have a latent heat flux, why don't you simply convert it to a mass loss? This is the typical way to compute sublimation in glacier energy and mass balance models.
Figure 4: you might consider add Maladeta to Figure 3 and spare a figure.
Table 6: consider making a bar-plot out of it for more readability
P13 L30: here you talk about sublimation. This contradicts your statement above.
Fig. 12: to make the figure more readable you could remove the x and y axis labels for the interior plots, since they are the same for each plot.
Figure 13 and corresponding analysis in the text: I have trouble to understand why the upper elevations see a reduction in melt while the lower parts do not? The provided explanation ("reduction in mass loss as glaciers disappear towards the end of the century" holds even more true for lower elevations. Or is this due to regional differences, the high latitude arctic having more mass below 2000 m a.s.l? This needs more explanation in the text.
P16-L16: about elevation feedback - see main comment above.
P18 L20: "*Changes in solar radiation can be an important driver of melting.*": It is a bit sad that you didn't take my advice about analysing the energy fluxes...