The manuscript “An investigation the thermo-mechanical features of Laohugou Glacier No. 12 in Mt. Qilian Shan, western China, using a two-dimensional first order flowband ice flow model” has already gone through one or two rounds of reviews and the authors have carefully addressed the previous comments. I will thus keep my review rather short to avoid pushing the manuscript into a new direction, making it a never ending story. I like the figures.

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General comments

- My main concern is the use of $n = 3$ as the exponent of both the flow law (Eq. 2) and as the exponent in the sliding law (Eq. 13). While there is relatively good evidence that supports $n = 3$ in the flow law, this is not the case for the sliding law. Simulated flow speeds are quite sensitive to the choice of n. For a Greenland example, see Supplementary Fig. 1 in Aschwanden et al. (2016). In my experience this is one of the most crucial parameters in ice dynamics so the authors would need to convince me that their conclusions remain robust.

Specific comments

p. 1, l. 19 as well as later. “. . . of approximately $50.5\,km^2$”. In combination with the word “approximately”, I would only reduce the number of significant digits to “approximately $50\,km^2$.

p. 2, l. 15 It seems appropriate to cite the oldest relevant manuscript. The importance of the surface thermal boundary has been discussed by Blatter and Kappenberger (1988). There is also a bunch of older literature on Storglaciären, northern Sweden that talks about the refreezing of snow melt in the firn area. My memory is a bit more shaky there, I may cite some papers in Aschwanden and Blatter (2005).

p. 2, l. 28 change “basal slip” to “basal motion” so you can avoid talking about the actual mechanism by which the motion occurs.

p. 3, l. 7 change “mainstream” to “main branch”

p. 3, l. 27 change “is an order” to “is on the order”

p. 5, l. 9 The first order model was first described by Blatter (1995) but Pattyn (2002) did a nicer job explaining the equations. I suggest using both citations.

p. 6, l. 16 . . . in the temperate zone refreezes

p. 7, l. 28 change "We denote" to "We use"

p. 9, l. 8–9 Are you really using a "zero surface mass balance" for the relaxation? Maybe I mis-interpret this, but to me, this means that the surface mass balance is zero at every grid point at the surface. That would be an odd choice. Commonly, one uses a constant-in-time surface mass.

p. 10–11, Parameter sensitivity I am not so familiar with the temperature model used here but it appears that the authors try to simulate a polythermal with non-polythermal physics, which may question some of the conclusions. Please correct me if I have misread the manuscript. For example, the authors state that "a larger geothermal heat flux can result in larger TIZs but has limited impact on modeling ice velocity" (I think it should read: "a larger geothermal heat flux can result in a larger TIZ but has a limited impact on simulated ice velocities."). This is not a surprise as the model used here does not soften the ice as a function of the water content. Nonetheless, I agree with the conclusion. Using a polythermal model that includes this feedback does not change the shape of the TIZ and the simulated velocities much (viscosity is a linear function of water content). This has been discussed by Greve (1997a,b); Aschwanden et al. (2012) and recently by Hewitt and Schoof (2017).

p. 12., l. 29–31 This needs to be reformulated. From Fig. 12a, I cannot see why the temperature profile is determined by the thermal boundary condition and by the ELA. "can be remarkably determined" is not proper English. Also it needs to read ". . . , while the ELA controls. . . "

p. 14, l. 18 change "which "cools down" the ice temperature to "which "cools down" the ice. One can warm and cool ice, but temperature can only be lowered or raised.

Figure 1a The map looks nice but please state what the background image is. Also, it's pretty dark so it is hard to see any features. Maybe use a pan-sharpended RGB composite instead?

References

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