Report on revised manuscript "*An investigation of the thermo-mechanical features of Laohugou Glacier No.12 in Mt. Qilian Shan, western China, using a two-dimensional first-order flow-band ice flow model*" by Wang et al.

The authors have not made any change in this revised version of the manuscript concerning the way they constrain their steady state temperature field. I still think that it do not make any sense to constrain steady state temperature on one surface (20m depth) measurement which is obviously not representative of the steady state condition (or the author have to show that the climate has been unchanged in the last 50 years in the region). It also makes no sense to try to fit the upper part (above 50 m depth) of their deep temperature profile with the modeled steady state temperature profile. The high temperature gradient in the upper part of this profile is very likely due transient surface cooling in response to reducing firn area. That would mean that results clearly underestimate the amount of temperate ice using the steady state assumption that way. A better approach would be to try to fit the bottom part of the temperature profile, it doesn't matter if the steady profile fit the upper part or not, there is no reason for that...

Consequently, I still don't think the current version of the manuscript deserves publication in The Cryosphere.

I suggest the author to:

- First, calibrate a steady state temperature field by trying to match (closely) the bottom part of their temperature profile (probably using an ELA more representative of a steady state for the glacier mass balance rather than using the ELA of one particular year!!)

- Then, if no air temperature time series are available, the author should try different past air temperature scenarios in order to get transient temperature field in accordance with the englacial temperature measurement they have. The transient scenario have to include both transient surface temperature and ELA evolution. Also I suggest the author to look if some reanalysis product of air temperature are available in the region for constrain the transient model.

General comments:

- Sorry if I was unclear but I suggested to the author to use in the ablation area a parametrization that link Tair to Tsbc (Tsbc = Tair +c) for performing transient simulation. What does the use of

such parametrization bring now in this revised manuscript??? This is not improving the way that boundary condition are addressed. Figure 7 can be deleted.

- Author should not use one particular year of ELA (2011 here) for modeling a steady state temperature but should use the mean ELA over the last 50 years or at least something close to the steady mass balance ELA… This lead also to wrong surface boundary condition.