# Review of "*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.

This paper analyses thermal and dynamical features of a glacier in Mt Qilian Shan (North Western China). The approach use a 2D thermo-mechanically coupled flow line model constrained on surface velocity and boreholes temperature measurements. Results show that the glacier is mostly cold with a potential basal temperate layer. The authors show that this layer is mostly due to strain heating and advection of warmer ice coming from the accumulation zone. The thermo-mechanical model is well constructed and use appropriate physics especially for calculating CTS position. The study brings insight in a much unknown region in glaciology.

However, this study clearly suffers of how boundary condition are addressed and of too much similarities with the already published *Zhang et al.* [2013]. Authors cannot performed a steady state simulation based on a punctual 20 m depth englacial temperature measurement, this will surely lead to a wrong temperature field. Indeed, near surface temperatures are probably not representative of the steady state regarding the recent context of atmospheric warming.

I don't think the current version of the manuscript deserves publication in The Cryosphere and needs at least a transient approach taking into account the glacier near-surface thermal evolution in response to climate variability before be resubmitted.

## General comments:

- Although the graph are nicely prepared and the structure of the paper is clear, the too obvious similarities with *Zhang et al.* [2013] give the impression of reading exactly the same paper… The only change is the way that thermal boundary condition are addressed which is not a real improvement. I suggest to explore the transient state using available meteorological data to distinct this new study from *Zhang et al.* [2013].

- The thermal surface boundary condition should be better addressed. As I said above, the 20-meter-deep temperature is representative of the climatic forcing on the glacier energy balance during the previous year only. Using this temperature as boundary condition of a steady state simulation will lead to a temperature field probably far from the reality. The authors should, at least, try to develop a parametrization that linked $T_{sbc}$, $T_{air}$ and the ELA elevation based on the available observations on the glacier. I recommend to use in the ablation zone $T_{sbc} = T_{air}$ + constant and find the constant that allows to match the measured $T_{20m}$ instead of using the approach of Wohlleben et al. [2009] which is very qualitative…

- I don't see any dependence of the sliding law to temperature. The authors seem to assume that sliding only depend of the effective pressure which is assumed to be uniformly proportional to the hydrostatic pressure in their study. This is very disputable, modeling sliding in cold area is very unusual in glaciology… Also, surface velocity measurement do not bring the evidence of sliding on this glacier. I think that removing sliding in the model still lead to modeled surface velocities under the measurements uncertainties (see next comment).

- Uncertainty on the surface velocity should be indicated to be able to discuss about the goodness of the fit and comparing velocity measurements at different periods. Is the difference between winter, summer and annual mean velocities are really significant?

- I note that the author have placed the ELA elevation to be able to "fit" their deep borehole data but is this ELA elevation really correspond to what is observed on the field?

## Specific comments

I think you could write "englacial" instead of "en-glacial" everywhere.

*P 1 – line 1* : Remove first sentence

*P 1 – line 3* : Mt Qilian Shan located in

*P 1 – line 6* : match well (remove well before "but clearly")

*P1 – line 7 :* "because the flow branch is ignored" : this assertion is not really supported by anything in the paper and many other reason could be invoked

*P1 – line 7 :* "agree closely" : I don't agree, this is not a close match

*P1 – line 9 :* ~~were~~ highly : are highly

*P1 - line 9 :* Remove (for example … temperature)

*P1 – line 10 :* I don't think we can speak of the "work of Wohlleben et al. [2009]" talking about the qualitative assumption made is this paper

*P1 – line 13-14 :* Like (…) LHG12 : this is not true. Most important parameter are surface conditions including snow cover thickness and summer melting intensity.

*P1 – line 18-19 :* Sentence too long

*P2 – line 11-13 :* Bad example : what is the link with a full stokes model here ?

*P2 – line 13* : In addition = not appropriate here

*P2 - line 14* : “can be strongly influenced” : this is the main control !!

*P2 - line 22* :  remove “extremely”

*P 3 – line 12 :* explain why you are interested in parametrizing transverse profile ?

*P3 – line 18 -29 :* Give uncertainty on the measurement

*P4 – line 20-24* : There is no interest to detail the shape of the profile in the active layer

*P4 – line 28-29 :* Give the assumption of the model

*P5 – equation 6* : reference ?

*P6 – equation 10* : value of Γ is not discussed

*P 8 - line 12 :* The authors claim a close match between model and observations at 80-90 m depth in the deep borehole: this is the point where the two curves (data and model) are just crossing! This not shows a good agreement between data and measurement.

*P 8 – line 33* : Is there moulin on this cold glacier ?

*P11 – line 1-2* : Remove sentence