Response to Anonymous Referee #1

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We would like to thank the anonymous referee #1 for giving constructive comments on our paper. We have responded each comment with great care. The original comments of the reviewer are given in italic, and our responses are given directly below in regular.

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].

It's true both studies share a few of similarities. After the attempt of Zhang et al. (2013) on the East Rongbuk Glacier, Mt. Qomolangma (Everest), we've been curious about the thermo-mechanical features of other typical Tibetan mountain glaciers. Does the climate warming really have a great impact on these glaciers and how much are these impacts? The East Rongbuk Glacier is at the southern edge of Tibetan Plateau. The one we get interested this time, Laohugou Glacier No. 12, is, however, at the northeastern edge of Tibetan Plateau. Despite the big different locations and climate backgrounds, both glaciers have been taken as fully cold for quite a long time by our China glaciological community. We hope that, by using similar numerical techniques, we could possibly get some interesting findings that can guide us to a big picture of Tibetan glacier changes. For example, does this 2D flowband model really work for mountain valley glaciers (we can save a lot of field efforts and money if it or something similar works)? If yes, how much can we rely on it? if not, how can we improve it? But first we should test it at different locations. That's the main reason we use a similar model approach and study method to Zhang et al. (2013).

We agree that the past climate change may have a great influence on the glacier velocities and temperature field. The difference between our diagnostic model results and the observations can be either from the assumptions of the model physics or the transient state of glacier change. We really wish we could do the transient study for LHG12 (and the East Rongbuk Glacier). Despite some previous expeditions in 1970s and 1980s, there is very few long-term series of meteorological data available in this area. The glaciological station was established in 2008 and we do not have the radar gemotry data of 2008 either. Thus, our aim is to investigate the current thermo-mechanical state by neglecting the transient impacts. We know by doing this there will be some uncertainties in our model results. We assume the transient effect in past years is stable and our thermal steady-state assumption is effective. We believe that our thermal steady-state model can capture some characteristics of glacier behaviours within the range of historical changes, and that our conclusion that LHG12 is now polythermal should be robust. To be as cautious as we can, we avoid showing precise number of, like, temperate ice zone lengths and thickness in both the abstract and the conclusions.

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*

As suggested by the reviewer, we now prescribe the T_{sbc} in the ablation area by a simple parameterization $T_{\mathrm{sbc}} = T_{\mathrm{air}} + c$, where c is a tuning parameter including the impacts of both the surface energy budget and the steady-state temperature Gilbert et al. (2010). We vary the values of c from 0 to 6 K (with a step-size of 0.2 K) and compare the modeled 20 m borehole temperatures with in-situ annual measurements at site 1 and 2 (Fig. 1). As shown in Fig. 1b (in manuscript), site 1 is located at the center of the confluence area where the convergent flow from the west branch joins the mainstream. Thus, at site 1 it is difficult to find a good c value that predicts close temperature comparisons to the observations. We therefore determine the c value (1.6 K) based on the fittings between the modeled and observed ice temperature data at site 2.

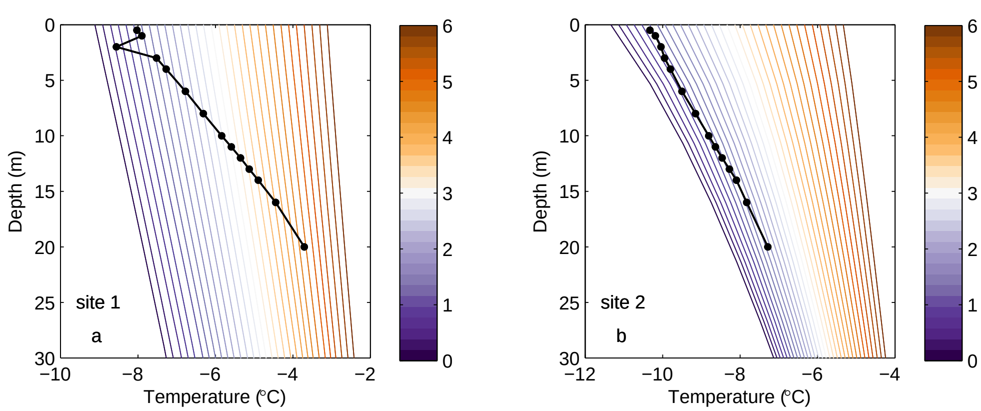

Figure 1: Sensicitity experiments of the tuning parameter c by comparing the measured (black dotted lines) and modeled (coloured lines) 20 m borehole temperatures at sites 1 (a) and 2 (b). The step-size of varying the c value is 0.2 K.

In addition, we also compare the differences between the new (E-new, (Gilbert et al., 2010)) and the old (E-old, (Wohlleben et al., 2009)) parameterizations of the thermal surface boundary conditions (Fig. 2). It shows that the two experiments produce very similar results in terms of modeled ice surface velocities, basal sliding velocities, temperate ice zones, and temperature profiles at the deep borehole (Fig. 2b, c, d). As can be expected, the modeled column mean and basal temperatures in the distance of km 5.0 – 9.1 demonstrate large differences due to the different parameterizaitions in the ablation area (Fig. 2a).

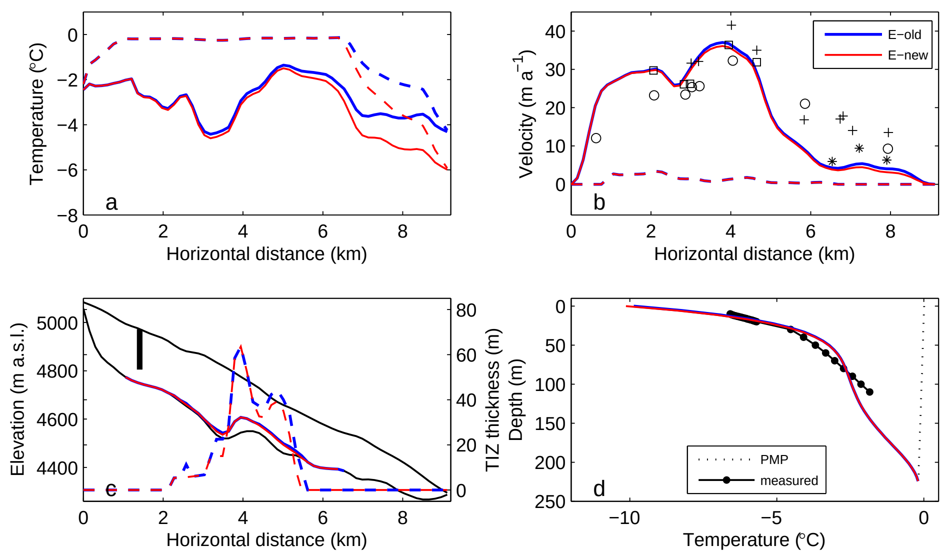

Figure 2: Modeled ice temperatures and velocities for experiments E-old (blue line) and E-new (red line). (a) Modeled column mean (solid lines) and basal (dashed lines) ice temperatures along the center flowline. (b) Modeled surface (solid lines) and basal (dashed lines) ice velocities along the CL. The symbols show the measured ice surface velocities same as in the manuscript. (c) Modeled CTS position (solid lines) and TIZ thickness (dashed lines). The black bar shows the location of the deep ice borehole. (d) Measured (dots) and modeled (coloured lines) ice temperature profiles for the deep borehole. The dotted line shows the pressure-melting point as a function of ice depth.

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).

It's not true. The sliding events are certainly a result of the existence of temperate ice. At the ice-bed interface, we prescribe a non-slip boundary condition where ice is frozen to the bed (cold ice) and a Coulomb friction law where ice is temperate, i.e., the ice temperature reaches the local pressure-melting point. We have clarified this in p6–line9.

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?

We agree the reviewer that the uncertainties of the ice velocity data are important for evaluating our model results. We estimate the data uncertainty below 1 m a^{-1}. But the stakes are not exactly located on the center flowline, which may also bring some unknown uncertainties. The summer (2008) and winter (2010) velocity data we have are not from a single year. They cannot be exactly compared. But from the only overlapped point we have (Fig. 3 in the manuscript), the difference between winter and summer values is non-negligible – it could be up to around 50%. We have added the uncertainties of GPS positioning and the calculated velocities in p3 – line23-24.

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?

The ELA was identified from the Landsat image on September 6, 2011, which is quite close the time (October 1–6, 2011) we drilled the borehole.

Specific comments

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

Changed.

P1 line 1: Remove first sentence

Removed.

P1 line 3: Mt Qilian Shan located in

Changed.

P1 line 6: match well (remove well before "but clearly")

Changed.

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

It's correct that the neglect of the flow branch may be one of many reasons. We have conducted two other experiments by increasing the glacier width as a proxy of convergent effects of the west branch and by adjusting the friction sliding parameters at the confluence area. We found that both basal sliding and convergent effect can largely influence the ice surface velocities in that area. We now add several sentences in p9–line3-10 and also include an additional figure (Fig. 9 in manuscipt)

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

From our point of view, it's quite close, given the facts of the sparse observations and the simplified 2D model we use. But as the reviewer suggested, we now remove "closely".

P1 line 9: were highly: are highly

Corrected.

P1 - line 9: Remove (for example temperature)

Removed.

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.

We thank the reviewer for this comment. We now change the sentence as "strain heating is an important parameter controlling the englacial thermal structure in LHG12." .

P1 line 18-19: Sentence too long

Changed. Now the sentence becomes

"Located on the northeastern edge of the Tibetan Plateau (36 – 39 °N, 94 – 104 °E), Mt. Qilian Shan (MQS) develops 2051 glaciers covering an area of approximately 1057 km^2 with a total ice volume of approximately 50.5 km^3 (Guo et al., 2014, 2015). Meltwater from MQS glaciers is a very important water resource for the agricultural irrigation and socio-economic development of the oasis cities in northwestern China."

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

This example was mainly for underlining the importance of temperate ice. But we agree with the reviewer. The sentence is now removed. Lines 10–13 have been changed to:

"The temperature distribution of a glacier primarily controls the ice flow rheology, englacial hydrology, and basal sliding conditions (Blatter and Hutter, 1991; Irvine-Fynn et al., 2011; Schäfer et al., 2014). A good understanding of the glacier thermal regime is important for predicting glacier response to climate change (Wilson et al., 2013; Gilbert et al., 2015), improving glacier hazard analysis (Gilbert et al., 2014a), and reconstructing past climate histories (Lüthi and Funk, 2001; Gilbert et al., 2010)."

P2 line 13: In addition = not appropriate here

This line has been changed as shown in above.

P2 - line 14: "can be strongly influenced": this is the main control!!

We have corrected it and add some corresponding references. Now it reads:

"The thermal regime of a glacier is mainly controlled by the surface thermal boundary conditions (e.g., Gilbert et al., 2014b; Meierbachtol et al., 2015). For example, near-surface warming from refreezing melt-water and cooling from the cold air of crevasses influence the thermal regimes of glaciers (Wilson and Flowers, 2013; Wilson et al., 2013; Gilbert et al., 2014a)."

P2 - line 22: remove "extremely"

We consider the LHG12 as an extremely continental-type glacier according to the classification of Shi and Liu (2000) who categorized the China glaciers into three types: the maritime (temperate) type, sub-continental (sub-polar) type and extremely continental (polar) type. We prefer to keep "extremely" as an identifier to the sub-continental type.

P3 line 12: explain why you are interested in parametrizing transverse profile?

The LHG12 is a valley glacier which is confined to channels with lateral drag exerted by the valley walls. As you all know, the lateral drag has a remarkable impact on glacier dynamics. To account for the lateral drag in a 2D ice flow model, we may either use a so-called "shape factor" proposed initially by Nye (1965) and impressed again recently by Adhikari and Marshall (2012) or make a parameterization based on glacier widths at all depths (Pimentel et al., 2010; Zhang et al., 2013). By parametering the transverse profile based on GPR measurements, we can derive the widths of glacier cross-sections and parameterize lateral drags at different depths (section 3.1). We now add an additional sentence for explanation in p3–line11-13.

P3 line 18 -29: Give uncertainty on the measurement

We have added a description of the uncertainties on the measurements.

"We measured the stake positions using a real-time kinematic (RTK) fixed solution by a South Lingrui S82 GPS system (Liu et al., 2011). The accuracy of the GPS positioning is an order of a few centimeters and the uncertainty of the calculated ice surface veloctíes is estimated to be less than 1 m a^{-1}."

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

We have deleted the description of temperature variations in the active layer.

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

We now add the assumptions. "By assuming the vertical normal stress as hydrostatic and neglecting the bridging effects (Pattyn, 2002), the equation for momentum balance is given as".

P5 equation 6: reference?

It's (Pattyn, 2002). Now added.

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

$\Gamma = 0.84 m_{\max}$. Now added.

P8 - 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.

LHG12 is a very large valley glacier. Though a lot of field work have been taken on this glacier, the *in-situ* observations are still sparse and temporally discontinuous. This is also one of many reasons that we didn't try 3D Stokes ice flow model. It's true that there are still some obvious disagreements between modeling results and *in-situ* observations. But given these poorly datasets, we are actually quite happy about the curves. However, as the reviewer suggested, we have removed the word "close".

P8 line 33: Is there moulin on this cold glacier?

Yes, we observed several moulins in the middle ablation area in 2009 and 2014.

P11 line 1-2: Remove sentence

Removed.

References

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