Reply to the general comments

Modeling is based on poorly grounded mass balance parameterization. Moreover, the results are not very well analyzed nor discussed, which results in speculative and qualitative conclusions. Important field data are not presented in the paper and many of important citations are in Chinese. Thus, I suggest the author to present and carefully analyze available data for this glacier, before setting up a model for numerical experiments.

We only have measured surface elevation and ice thickness by ourselves. We do not have our own surface mass balance (SMB) data. The SMB data measured by other groups has been published as a figure in Yao et al., (2012) as we reference. That is the only data we have, not original mass balance data.

In this revision, besides the previous model-based SMB resulting from energy model of a nearby glacier (Xibu Glacier), we introduce a new formulation for estimating SMB of Gurenhekou Glacier, which we call observation-based SMB. We digitize the SMB of Gurenhekou Glacier for six-year period 2004-2010 from a published figure in Yao et al., (2012) and combined it with our surface elevation data to get the SMB-altitude profile for six years. That is the longest period of observation data available. We parameterize both SMB and its uncertainties as functions of elevation and JJA mean temperature. This is not ideal, but since we do not have an AWS station to calibrate an energy balance model such as Mölg et al., (2012), this is the optimum for this study. Since we want to generalize the results to glaciers, almost all of which do not have any data at all, this is a reasonable approach if we can produce realistic uncertainties.

For each warming scenario (either 0.02 K a^{-1} or 0.05 K a^{-1}), we did four experiments using the best fit SMB, its lower and upper bound of observation-based parameterization, and the model-based SMB, respectively. We get the uncertainty of glacier volume change projection for 50 years and reject the unrealistic projections. Then we did more analysis, including ice volume change as a function of time, the computed retreat and area reduction rates with error bar. We also improve the quality of plots according to the referee's comments. Finally, we modified our discussion section and derive the main conclusions more logically and carefully.

Reply to Specific comments

(1) ***Full Stokes flow model*** *The authors claim "The steep and rugged geometry requires use of such a flow model to simulate the dynamical evolution of the glacier (page 146, line 5–6)". However, the slope of the glacier is never mentioned and bedrock elevations are smoothly interpolated from only several ice-radar profiles. The glacier geometry is relatively simple and no-sliding condition is assumed. As it is expected from the low temperature conditions, computed ice velocity is very slow (less than 2 m a-1). Is full Stokes model really needed to simulate this glacier? It is nothing wrong to use a flow model with higher complexity, but it does not assure the significance of this modeling work.*

The two-dimensional footprint of this glacier is rectangle, a simple shape. But the surface and bedrock has steep slopes. We add the following information to the revised manuscript on Page 148, Line 11: "The slope of the glacier surface varies from 9.6° to 14.5° and has a mean value of 11.3° . The aspect ratio is about 0.06. " We also add the "2.5 km long and 0.6 km width" on Page 148, Line 9. We also add "The bedrock slope is larger than 16.7° along the direction of center line in the top part of glacier, and has a large variation from 0° to 38.7° along the perpendicular direction to center line since this glacier is located in a valley." on Page 151, Line 17.

In order to answer the question if full Stokes model really needed to simulate this glacier, we computed the deviatoric stress tensor τ in the diagnostic simulation by using full-Stoke model. We find that the shear stress in the vertical plane (τ_{xy}) and the normal stress deviators ($\tau_{xx}, \tau_{yy}, \tau_{zz}$) are of similar order of magnitude as the shear stresses in the horizontal plane (τ_{xz}, τ_{yz}). Therefore, it is not suitable to use a simplified model such as SIA which ignores the stress components τ_{xy} , τ_{xx} , τ_{yy} and τ_{zz} .We made diagnostic simulation with the built-in shallow ice approximation (SIA) solver in Elmer/Ice, obtaining flow velocities larger by one order of magnitude compared with the same result with the full-stress model. We add the results of deviatoric stress tensor in Section 3.3.1 (Diagnostic simulation). Hence the choice of a full Stokes flow model - even within the relatively small contribution with respect to SMB - is justified. In any case, full Stokes – if computationally feasible – in our perspective always should be preferred as it a-priori eliminates the source of error (as small as it may be) introduced by approximations based on aspect ratios.

We also do a prognostic simulation without ice dynamics for Gurenhekou Glacier (the glacier change is only determined by the climate mass balance). For instance, we use the model-based SMB and the best fit observational-based SMB respectively with temperature increasing rate 0.02 K a^{-1}. Then we compare the glacier change results of simulations with and without ice dynamics. We find that by using model-based SMB the annual volume reduction rate is 0.97% without dynamics and 1.08% with dynamics, so the ice dynamics make about 10% contribution to the volume change. And by using the best fit observational-based SMB, the annual volume reduction rate is 0.35% without dynamics and 0.24% with dynamics, so the ice dynamics accounts for about 31% of the volume change. Therefore, for this small glacier with relatively small ice thickness and surface speed, the ice dynamics plays a less significant role than SMB, but it is not negligible.

(2) ***Mass balance model*** *Assuming that ice dynamics does not much for the evolution of this glacier, the results of the experiments is highly dependent on the mass balance model. Unfortunately, the mass balance given by Equations (2)–(4) is not convincing. The model result showed thickening of the glacier in the upper reaches, which indicates something wrong with the prescribed mass balance. The authors take three different mass balance gradients based on mass balance modeling reported for nearby Xibu Glacier (Caidong and Sorteberg, 2010). According to this previous work, the mass balance of Xibu Glacier is characterized by steep gradients in the ablation zone and a much smaller gradient in the accumulation zone (Figure 6 and 7 in Caidong and Sorteberg, 2010). Such a mass balance distribution is commonly observed in the field and often assumed in numerical models. Contrasting to such mass balance profile, this study uses a very steep mass balance gradient in the accumulation zone (Figure 3). The modeled mass balance falls within the range of field data. However, the field data show large variations and nothing is explained about the data in this paper. Moreover, the ELA is parameterized to summer mean temperature with a linear coefficient of 79 m K^{-1} (Equation (4)), whereas the value reported for Xibu Glacier is 140 $\pm$125 m K^{-1} (Caidong and Sorteberg, 2010). These numbers are so different and uncertainty is expected to be large. This coefficient was determined from the field data, but again no details are given in the text. Because the model output is totally dependent on the mass balance equations, the authors should derive the equations more carefully and convince the readers with field data and relevant information.*

We mainly agree with the referee and that is why we now add a new SMB formulation based on the limited observations. We wanted to also use the model profile from Xibu Glacier as it is a more theoretically derived structure, and independent of the short observational period. Using both together gives a more reasonable uncertainty estimate. The Xibu Glacier has 3 SMB gradient region, “below ELA”, “near ELA” and “above ELA”. The mass balance has smallest gradients above ELA plus some meters, larger gradients below ELA minus certain meters, and largest gradients near ELA. However, the ELA of our glacier is about 5800 m, very close to the glacier top (the elevation is no more than 6000 m). So we mainly have “below ELA” and “near ELA” region. Hence the gradient in “near ELA” region is larger than “below ELA” if we follow the SMB profile of Xibu Glacier.

Anyhow, as there is justified skepticism to apply SMB profile of Xibu Glacier to Gurenhekou Glacier, we introduced another SMB parameterization. In this revision, we digitized the SMB of Gurenhekou glacier for six-year period 2004-2010 from a published figure in (Yao et al., 2012) and combined it with our surface elevation data to get the SMB-altitude profile for six years. We added this six-year SMB-altitude plot in the revision. For each altitude, we linearly regress SMB on the JJA mean temperature in Lhasa station for the six years, and get a modeled SMB and its 1 standard deviation confidence interval as a function of temperature. Using the assumption that the JJA mean temperature increase by 0.02 K a^{-1} or 0.05 K a^{-1}, we get the uncertainties of SMB for each year and in the whole altitude range.

We do not really understand why the referee thinks a rise in the accumulation area proves the SMB model is incorrect. We agree that changes are much larger in the ablation than accumulation area (as can be seen by trim lines), but there is no obvious reason to us why the glacier could not fill a valley more completely if it received more snow fall than at present – as is possible under future climate forcing. We limit the extra accumulation to 15 m because the glacier head is almost at the top of the valley.

(3) ***Analysis and discussion*** *The modeling results are not very well analyzed and discussed. Because of large uncertainties in the input data, the results have to be evaluated more carefully. One example is the computed retreat and area reduction rates. These numbers are highly dependent on the bed geometries near the margins, where no ice-radar data are available. However, there is no error bar given to the results. The rapid retreat at the northeastern terminus was explained by thinner ice there, but ice radar measurements were not performed in that part of the glacier (Figure 2). Another problem is very limited analysis and discussion on three dimensional glacier geometry change. One of the important advantages of the model is its capability to handle three dimensional geometry. For example, it appears that the uppermost part of the glacier is detached from the main ice body after 50 years (Figure 9b). Is this realistic? Or is this an artifact as the result of ice thickening in this region (Figure 10)? More detailed analysis of the glacier geometry change, e.g. flowline analysis, ice volume change as a function of time, contour maps, improves the significance of the study.*

1. The reason why we know the northeastern terminus has thin ice is a radar line that we mistakenly did not plot “2-2008” near the terminus in Fig. 2, which shows the bedrock near the northeastern is higher than southwest, therefore, the ice thickness near the northeastern of terminus is thinner than that in the southwest since the surface is flat. We add this radar line in Fig. 2 of the revision.

2. The reason why the uppermost part of the glacier is detached from the main ice body after 50 years using the model-based SMB is because most of the glacier (except the uppermost part of glacier) is subject to ablation by mid century using the model-based SMB, and the glacier at places is so thin on the top that surface lowering exposes bedrock. However, the very top of the glacier only takes up a small ratio of the whole surface.

3. We add more detailed analysis of the glacier geometry change, e.g. ice volume change as a function of time, the computed retreat and area reduction rates with error bar. The new SMB parameterization was very useful in establishing a more plausible uncertainty in volume and area shrinkage.

(4) ***Presentation Plots*** *are colorful, but not very well prepared for presentation on a paper. For example, surface and bedrock contour maps are more useful than Figure 4. Flow vectors can be superimposed on the velocity contour map (Figure 5). It is hard to compare two diagrams with different color codes (e.g. Figure 8 left and right). Glacier margins near the terminus can be enlarged to show the details of the glacier retreat (Figure 9). As compared to the previous paper presented by the coauthors (Zwinger and Moore, 2009), there is a lot of things to do to improve the presentation.*

Fig.4 (Three-dimensional glacier geometry) is replaced by surface and bedrock contour maps (which is Fig. 5 in the revision).

It is hard to see the flow vectors superimposed on the velocity map of the whole glacier (Figure 6). So we add a separate flow vector map in Figure 6 (which is Fig. 8 in the revision).

We use the same color bar for surface elevation change comparison in Fig. 8 (which is Fig. 12 in the revision).

Glacier margins near the terminus are enlarged to show the details of the glacier retreat in Fig. 9 (which is Fig. 13 in the revision).

(5) ***Main conclusions*** *The author states in the abstract that "These changes imply that this small glacier will probably disappear in a century" and "significant numbers of glaciers will be lost in the region during the 21st century". Because these statements are very influential, I suggest the author to carefully backup them by data and text. As far as understand, the bases of this statement is the mean rates of volume loss computed for the next 50 years (1.03% and 1.46% for the two temperature scenarios). This is for the next 50 years and no experiments were performed further in the future. The results are highly dependent on the sensitivity of ELA on the temperature change (Equation (4)). Warming by 5 degrees raises ELA by 400 m, which is well above the current highest altitude of the glacier (Figure 3). However, glacier may survive under the 2 degrees warming scenario, because expected shift in ELA (160 m) is still below the glacier top. The authors argue that "The glacier is typical in many respects of many small glaciers in the Nyainqentanglha region". On the other hand, they also write "only about 40 out of 960 glaciers are larger than 1 km^2 " and "it is not clear if Gurenhekou glacier is really representative in its area loss rate." I hope the main conclusions are more logically derived and moderately described in the text.*

Since our results partly have changed, we modify the content in Section 4 (Discussion and Conclusion) accordingly in the revision. The new SMB parameterization we use allows us to get a better idea of uncertainties in our projections, and as a result we moderate our statements. Smaller glaciers will react faster to climate warming than larger ones, this is both due to the larger surface area to volume ratio of smaller glaciers, and their greater sensitivity to extreme years – which as the climate tends to warm will be mainly relatively much warmer years than cooler ones. We feel that if the SMB estimates derived by the two mass balance estimates reflect real uncertainties in SMB then our conclusions are not controversial. While Gurenhekou Glacier may be one of the surviving glaciers, many of the smaller and low lying glaciers will be certain to disappear. In absolute terms such as sea level rise, it is not at all important or controversial since the vast majority of ice volume is stored in the larger glaciers, some of which will of course survive past 2100.

The representativity of Gurenhekou can be assessed by the statistics of the mountain range. According to Bolch et al (2010), most glaciers are in the size class 0.1–0.5 km^2, whereas glaciers between 0.5–1.0 km^2 cover the largest area and most of them face east. Median elevation of the glaciers is situated at around 5820 m. The majority of the glaciers terminate at around 5600 m. So Gurenhekou Glacier (area 1.40 km^2; elevation 5500-6000 m; facing southeast) can be chosen as a representative glacier for many glaciers in the Nyainqentanglha region given that there are few glaciers with observational data (Bolch et al, 2010; Yao et al., 2012).

Other comments

page 146, line 16: Tibetan glaciers are not particularly sensitive to climate warming, Please explain why. Otherwise, better not in the abstract.
We delete this sentence.

page 147, line 6: loose or lose?
It is “lose”, we corrected it.

page 147, line 7–8: However, little work has been done… I think the sentence is odd.
We change it to “little work has been done on projection of glacier mass change on Tibet Plateau by using flow dynamics model.”

page 147, line 15: significant aspect ratio   What do you mean?
We change it to “large aspect ratio”.

page 147, line 18–19: accumulation rates are relatively high. Is it high in Tibetan Plateau?
We do not find any reference support. So we delete the half sentence “since accumulation rates are relatively high while ice velocities are relatively low.”

page 147, line 28: Gurenhekou glacier. Please be consistent "glacier" or "Glacier".
We use “Gurenhekou Glacier” through the revision.

page 148, line 4: southeast » southwest?
It is southeast.

page 148, line 6-8: The region is of special interest : Please provide citations.
We provide the citations, Bolch et al. (2010) and Kang et al., (2009).

page 148, line 9: strikingly regular in shape. What do you mean?
We mean the shape of this glacier area is relatively simple and regular. We change it to “ The glacier area is rectangular, with a length of 2.5 km, a width of 0.6 km and an area of 1.40 km^2 ”.

page 148, line 20: 140 m along a traverse line and … What do you mean?
The traverse line means the line which is perpendicular to the central flow line. We delete these word to avoid confusion. People can see the location on the ice thickness contour map (Fig. 6).

page 148, line 24: surface mass balance (SMB). Please use the abbreviation in the rest of the paper once you define it.
Done.

page 150, line 8–9: We can test this hypothesis. I wonder how you test the mass balance model with surface elevation change. Do you neglect the ice dynamics?
Yes, we neglect the ice dynamics.

page 151, line 1–4:This is very important part of the modeling. Please consider to show the temperature and mass balance data and plot the temperature dependence of the mass balance.
We do not have temperature measurements on the glacier. The JJA mean temperature in Lhasa station for six-year period 2004-2010 was used along with data from another glacier in the range to compute the lapse rate (as explained in for the derivation of Equation 1). As we said before, we do not have our own mass balance data. Observational ELA dependence on JJA mean temperature at Lhasa is also plotted in the revision.

page 151, line 11-13: Surface and bed geometry. Please provide contour maps of the surface and bed. Because surface and bed elevation data are sparse, discussion is needed on the quality of the DEMs derived by interpolations.
We add the contour maps of the surface and bed in the revision (see Fig. 4). We compare the surface and bed elevation data at crossovers between radar lines. The quality of measured surface and bedrock elevation is good. Surface elevation differences on crossovers between all the tracks measured in different years are no more than 4 m, which is mainly due to surface mass balance. The bedrock elevation on all crossovers between radar lines taken in different years agrees well, with the difference of no more than 4 m. We add the above discussion in the page p.148, l.21.

The surface relaxation that we did also shows that there is no obvious error in the ice thickness that is revealed by anomalous ice velocities in the model.

page 151, line 13-20: Finite element mesh. Please show the mesh. What are the element type and resolutions? Because Elmer/Ice is widely used for glacier modeling, please introduce previous works carried out with the same code.
We add the mesh plot in the revision. We add the element type and resolution in Page 151, Line 20 and Line 13 (Section 3.1). The three-dimensional mesh consists of 59600 nodes and 60228 bulk elements and 17938 surface boundary elements. Three different element types are used in the mesh: 14298 bilinear quadrilaterals, 16380 linear wedges and 43848 trilinear hexahedrons. The resolution of two-dimensional footprint mesh is 30 m.

page 152, line 12: gravity acceleration » gravitational acceleration?
we corrected it to “gravitational acceleration”.

page 152, line 12: ice density. What is the influence of firn in the accumulation zone?
In Tibetan glaciers the firn/ice transition is shallow, typically 15 m depth (Thompson et al., 1990). Firn has important influence on glacier flow when the firn depth is a large fraction of total depth, (e.g. Zwinger et al., 2007). But for Gurenhekou Glacier, the steep bedrock means that the firn is a small fraction of ice thickness except for the very uppermost glacier, where velocities are very slow (fig. 8 in the revision). Hence we do not think the influence of firn will make big difference to our results of retreat rate and area reduction rate.

page 152, line 17: effective strain tensor » effective strain rate?
It is “effective strain rate”, we correct it.

page 152, line 20: heat capacity and conductivity. Please provide citation.
The heat capacity and conductivity values we use come from Seddik et al. (2011); Cuffey and

Paterson (2010). We provide the citation in the revision.

page 153, line 3: the upper interface is: : :» the upper interface are : : :
We change the “upper interface” into “surface”.

page 153, line 18-20: The first is diagnostic simulation : I do not understand the latter half of this sentence. Isn’t it computation of velocity and temperature for a given glacier geometry? Is mass balance relevant, here?
Sorry, this sentence is confusing. We explain the meaning of steady state, in which case the surface elevation does not change with time, which means the surface mass balance balances the flux divergence. In fact, there is no need to explain it and we delete the latter half sentence.

page 154, line 4: Gao et al., 2012 » 2010?
It is 2010. We correct it.

page 154, line 15: Lhasa station Please consider to present the data.
We do not have the observed temperature data of Lhasa station for the period 1955-2005. In the reference by Caidong and Sorteberg (2010), they plot the temperature rise trend of Lhasa station for the period 1955-2005 (see Fig. 2 (b) in their paper) and conclude that the long-term warming is 0.23 C /(10 a) for wet season. Since the glacier is a summer accumulation type, we only concern the temperature in summer (wet season). We add this reference here in the revision.

page 154, line 23: fixed point iteration scheme. What is this iteration for? Please clarify.
We use the fixed point iteration scheme to solver the equations (5) and (7) in steady state (assuming all time derivatives to vanish), so we can get a steady state solution of velocity and temperature. We add the purpose in Page 154, Line 23.

page 155, line 4-5: We find : Please provide some statistical numbers, e.g. maximum deviation, standard deviation.
We add the maximum deviation of surface velocities in this sentence.

page 155, line 8: steady state solution. I’m still wondering what is done in the diagnostic experiment. Did you evolve the glacier surface until it reaches a steady state?
No. In diagnostic experiment, we fixed the surface and solve the steady state equations (5)-(7) (vanishing the term with time derivative in (7)) with boundary conditions (10)-(11).

page 155, line 23-26: Using topographic maps…**Please consider to show the data on the glacier retreat in the past.** ***Explain more details if you analyzed the topographic maps and aerial photos.***
We do not have the topographic maps and aerial photos. This observational retreat rate is from the reference (Pu et al., 2006). Page 155, Line 23-26 are all from (Pu et al., 2006). To clarify, we add (Pu et al., 2006) again in Line 24.

page 156, line 1: acceleration » retreat?
We change it to “retreat”.

page 156, line 5: area reduction rate. Reduction rate should be positive when the area decreases.
Yes. We use positive value when we say “area reduction rate” in the revision.

page 157, line 3-19: I agree that glacier growth above the level of the surrounding mountain

ridge is unlikely. Even if this sensitivity test shows its effect is insignificant, such a result gives me an impression that something is wrong with the mass balance model.
There is no obvious reason why the glacier could not expand to fill the head of its valley. However we agree that significant growth indicates an unphysical mass balance model, that is one with too much accumulation. That is why we limit growth to 15 m. We can reject some SMB scenarios that may be statistically reasonable, but unphysical.

In the revision, we delete the text and figure about the sensitivity test of 20 years surface elevation change.

page 158, line 5-13:I found that this discussion is very weak. Because of the parameterization of the mass balance to the temperature, one can easily expect a greater ice loss under a greater warming rate. This paragraph shows nothing more than this. To extrapolate the results to 100 years in the future, more careful analyses are needed on the glacier area and volume changes. For example, on which bases you assume a constant volume reduction rate for the next 100 years?
We agree that more careful analyses are needed on the glacier area and volume changes for 100 years. This question is included in "(5) Main conclusions" (page 4), and we answer it in the last paragraph before "Other Comments" in this text.

page 158, line 20-21: Hence if our results are representative : : :This is an important assumption that modeling results on this relatively large Gurenhekou Glacier (1.4 km2) represent all other smaller glaciers in the region. This assumption has to be supported by data. Otherwise, the statement "95% of glaciers in the range would disappear" is misleading.
We answer it in the reply to "(5) Main conclusions" (page 4) in this text.

page 158, line 28: : : : increased rate : : :» increased area loss rate?
Yes. We modify it to "increased area loss rate".

page 159, line 12-13: it is likely that southerly facing glaciers will accelerate. Please explain why.
We explain more here. As we said in page 159, line 8-10, the southeast side receives more snow fall than the northwest side and also more direct radiation, at present the two sides have similar average areal loss rates (Bolch et al., 2010; Shangguan et al., 2008). In future, since regional climate models suggest only modest increases in precipitation, but large temperature rises, therefore, the impact of temperature rise by radiation will be likely stronger than the snow fall. Since the southeast side receives more radiation, the retreat of glacier on this side will be likely faster.

page 159, line 3-26: These are interesting and important discussions. The climate variability has to be analyzed to assess the representability of Gurenhekou Glacier. It would be interesting if this model is used to investigate the effect of these changes expected in the future, i.e. changes in temperature and/or precipitation, albedo, debris cover.
We will study in the future the effect of albedo, debris cover change on glacier change. But it is outreach the scope of this paper.

Other modifications:
The Figures are renumbered since we add new plots.

We delete page 157, line 9-19 and Figure 10 of 15 m surface elevation change constrain test for 20 years, since this constraint is reasonable and we do not think we need to do this 20 years test.

We delete the references Blatter et. al., (2010), Greve and Blatter, (2009) in Page 147, Line 16, since we feel we do not need list many references there.

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