Reply to some general comments:

1. Some readers may not familiar with the local climate and its variability. Why is Gurenhekou Glacier expected to exhibit a climatic mass balance distribution similar to Xibu Glacier?

Xibu Glacier is close to Gurenhekou Glacier (about 25km away, Elevation: 5072~7162 m), and both of them are locate in Nyainqentanglha range. This region is influenced by both the continental climate of Central Asia and the Indian monsoon system, and is situated at the transition zone between temperate and sub-continental glaciers (Bolch et al., 2010; Kang et al., 2009). The northern slope and the southern slope of Nyainqentanglha range have different local climate in temperature lapse rate, relative humidity and radiation (You et al., 2007; Shangguan et al., 2008). Both Xibu Glacier and Gurenhekou Glacier are on southern slope and face southeast. So we expect the two glaciers are subject to similar local climate, which may result in similar climatic mass balance distribution.

2. As a test the parameterization is compared to observed surface elevation changes and stake measurements. From Figure 3 one expects a large uncertainty in the climatic mass balance, but this uncertainty, and potential consequences on projected glacier retreat, are not addressed in this paper.

In the revision, we investigate the uncertainties of surface mass balance (SMB), and translate them into uncertainties in projections of glacier mass (volume) changes. Besides the previous model-based SMB resulting from energy balance model of Xibu Glacier, we also introduce a new formulation for estimating SMB of Gurenhekou Glacier, which we call observation-based SMB. We digitized climatic mass balance for six years from a published figure in (Yao et al., 2012) and use our DEM data to get the SMB-altitude profile for six years. For each altitude, we linearly regress SMB on the JJA mean temperature in Lhasa station for the six years, and get a best fit 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 obtain the uncertainties of SMB for each year and the whole altitude range.

For each warming scenario (either 0.02 K a^{-1} or 0.05 K a^{-1}), we run 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 uncertainties 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 bars.

3. Surface elevation changes are the result of climatic mass balance and flux divergence. Given relatively small ice thicknesses and surface speeds, it is conceivable that the uncertainty in climatic mass balance is larger than the flux divergence term. So, is the glacier just down-wasting, and ice dynamics plays an insignificant role? I would test the hypothesis that a simple melt model is sufficient to project changes in the evolution of Gurenhekou over the next 50 years within the uncertainties arising from the climatic mass balance forcing.

In order to quantify the role that ice dynamics plays, we 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% contribution to 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.

4. About the nameing convention: "climatic mass balance", "full Stokes".

Thank you for pointing out the difference in terminology between climate and surface mass balance. However in the ice modeling community surface mass balance is routinely used, and firn processes are not usually treated at all. Firn has important influence on glaciers which consist of large parts of firn rather than pure ice (e.g. Zwinger et al., 2007), but here the firn is thin and neglected in dynamics, and we use ice density for the whole glacier in the model. The surface mass balance we use is ice equivalent. So we still use "surface mass balance" in the revision.

Regarding "full" stokes, we agree with the referee - but feel that we still should use the name "full Stokes" in the revision, since it is widely used in glaciology modeling community.

Reply to Specific comments:

(1) p. 146, l. 6; p. 147, l. 29; and p. 155, l. 7 "dynamical evolution". Please remove "dynamical". "Dynamic evolution" refers to changes in ice dynamics (i.e. flow patterns, distribution of stresses, etc). From the context I infer that you mean the glacier's response to environmental forcing, hence "The evolution of the glacier. . . " seems both sufficient and more adequate.

We replace "dynamical evolution" with "evolution".

(2) p. 146, l. 16 It might be an oversight on my side, but I cannot find statement or reference within the main text that supports "Although Tibetan glaciers are not particularly sensitive to climate warming"

We delete "Although Tibetan glaciers are not particularly sensitive to climate warming", since its not that relevant to the abstract. However it is suggested (Oerlemans and Fortuin, 1992) that maritime glaciers are more sensitive to climate change than continental ones (such as Tibet).

(3) p. 147, l. 14-21 In a flow-line study comparing the Stokes stress balance and the shallow Ice Approximation, Leysinger Vieli and Gudmundsson (2004) conclude that length and ice thickness changes are well reproduced with the Shallow Ice Approximation, at least in the case of insignificant basal sliding. Maybe the above reference could be added here.

Thanks. We cite this reference in p. 147, line 19.

(4) p. 149, l. 4 What are the errors in ice thickness estimates along the radar lines, and how does the sparsity of radar measurements and gridding-related errors translate into uncertainties in the DEM (i.e. ice volume). It is conceivable that the sensitivity of the prognostic simulations to uncertainties in initial ice volume estimates is small compared to the uncertainties in the climatic mass balance. If so, this might be worth a statement.

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 changes on all crossovers between different years are no more than 4 m, which is mainly due to climate 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 p.148, l.21. Therefore, the sensitivity of the prognostic simulations to uncertainties in initial ice volume estimates is smaller compared to the uncertainties in the climatic mass balance for 50 years.

(5) p. 153, l. 12

We correct “change” to “changes”.

(6) p. 153, l. 15-16 This is a repetition of p. 147, l. 22 and p. 152, l. 3. I suggest to remove it, and add the website of ELMER to p. 147, l. 22.

We remove “Elmer/Ice uses the Finite Element Method (FEM) to solve the partial differential equations.” in p. 152, l. 3. and “The full-Stokes model is solved by using an open source FEM package Elmer/Ice (http://elmerice.elmerfem.org).” in p. 153, l. 15-16, and add “Finite Element” and the website of Elmer (http://elmerice.elmerfem.org) to p. 147, l. 22.

(7) p. 153, l. 19-20 I am not sure I understand what is meant with “. . . , which then defines a mass balance pattern that is constant over time”. Could you clarify? It defines the surface mass balance that balances, both locally and globally, the flux divergence such that the surface elevation change is zero everywhere, or something like that, right?

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.

(8) p. 155, l. 12-13 remove “every year" unless it implies a yearly time step in the surface kinematical equation. If so, please clarify.

We delete the word “every year”.

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