In the reply, the referee's comments are in *italics*, our response is in normal text, and quotes from the manuscript are in blue.

Anonymous Referee #2

In this study, the authors propose to drive a minimal glacier model with GCM projections in the HMA region. The innovative part of the study is that they assess the impact of geoengineering on glacier changes, which is (as far as I am aware of) not discussed very often. However, the study suffers from the over-simplification of the glacier processes and from poor uncertainty assessments, two points which have to be addressed before considering publication.

General comments

Glacier model

The glacier model used in this study is quite far behind today's standards (e.g.Marzeion et al., 2012, Huss and Hock, 2015). I list here the major issues that need to be addressed:

• the model only considers changes in ELA with respect to summer temperature. They justify their choice by saying that most glaciers in the region are of the summer accumulation type (which is not proven) and that precipitation varies little over the entire HMA (which is a qualitative statement, and also probably not true for the sub-regions, as shown in Fig. 3). Precipitation has to be considered by the model, and not only summer precipitation: winter precipitation and the differenciation between liquid and solid precipitation has to be taken into account (in particular for the whole western and northern part of the study region, where precipitation is not falling in summer)

Reply: Yes the model is relatively simple, but we note that data are limited in HMA so providing verification and calibration for more sophisticated models is problematic (See the new Section 5).

Specifically addressing the issues raised:

We considered the annual precipitation, and the differentiation between liquid and solid precipitation in the revision.

• the response time of glaciers has to be taken into account. This has to be parameterised in the volume-area scaling relation, as discussed by Marzeion et al., (2012) and Bahr et al., (2015).

Reply: We take into account the response time of glaciers. We add response time in the volume-area scaling relation as in Marzeion et al., (2012) and present it in section 3.1.

• it is not clear to me how glaciers are supposed to grow in this model. Many glaciers in the HMA are currently growing or at least stagnating (without mentioning debris-covered glaciers), ad point which is not discussed in the study.

Reply: We add in the method section 3.1 a description of how we deal with glaciers growing. Integrating the SMB over each glacier gives the mass balance, which is also the volume change rate, which is converted to an area change rate using volume–area scaling.

The set of glacier surface grid points is updated every year --- the number of the grid points that need to be removed or added is calculated using the area change rate while the elevation of the grid points is updated using SMB.

For advancing glaciers, we add grid points to the glacier surface grid, whose elevations are all supposed to be the glacier elevation minimum in the n+1th year, $z_{min}(n+1)$, which is obtained as follows by assuming a constant glacier surface slope,

$$z_{min}(n+1)=z_{max}(n+1)+\frac{L(n+1)}{L(n)}\bullet(z_{min}(n)-z_{max}(n)), \qquad (5)$$

where $z_{max}(n+1)$ denotes the glacier elevation maximum in the n+1th year. We also limited the maximal surface increase at any point on the glacier to 15 m above the initial elevation at the starting year. We chose to do this because the valley glacier is physically constrained from growing above the level of the surrounding mountain ridge and side-walls.

• *the calibration of the mass-balance (MB) gradients is extremely loose. If I understand well, the MB gradients are defined for one glacier with observations and then applied to the entire sub-region. By looking at Table 1 (where the MB gradients are described), it looks very unlikely that there is any reason for the local MG gradients (which contain arbitrary altitude thresholds and other local properties) to be representative for the region. Here I suggest to use either data-driven gradients (i.e. based on climate data) or even much simpler statistical gradients models which would be easier to cross-validate (see validation section below).*

Reply: The SMB gradients are data-based and come from the sparse dataset available, as described in Zhao et al. (2014), with some additional glaciers in this study. We add information about ELA and altitude ranges for each glacier in Table 1. We have only one glacier with SMB measurements in most sub-regions, so we cannot do cross-validate everywhere. However, interestingly, in a few sub-region where there are two or three glaciers, we found that the SMB gradients of these glaciers is very similar in their common altitude range. For inner Tibet, there are 3 glaciers (Zhadang, Gurenhekou and Xiao Dongkemadi Glacier) with SMB observations, and they have almost the same SMB-altitude gradients, 0.0041 m m^{-1}, over their common elevation range (5515~5750 m, Table 1); two glaciers (Naimona'nyi and Kangwure) in central Himalaya have SMB gradients of 0.0038 m m^{-1} in their common altitude range of 5700~6100 m. These similarities suggest that the measured glaciers share some important characteristics with the vast majority which are not surveyed.

Validation and uncertainty assessment

The current approach to uncertainty assessment is not robust enough. Validation (i.e. comparison against observations) is quasi non-existent. I agree that given the few *number of observations, the task is not trivial. But especially in this case, it is recommended to make full use of all available data:*

• the authors could make use of cross-validation to assess the impact of interpolating the gradients on mass-balance (see e.g. Michaelsen, 1987)

Reply: We add a section 3.3 that discusses validation for the glacier model. We show that the model produces significant correlations on decadal scales with observations, and also how the benchmark glaciers agree well on MB gradients where they can be compared. We also show in Table 2 how the elevation changes simulated compare with satellite altimetry estimates at a marginally significant level, but which is of course limited in accuracy by the few regions and gross averaging from the satellite data.

Section 5 also discusses in depth how climate forcing and the glacier model affect the simulations.

• several recent publications made use of satellite observations to assess geodetic MB (e.g. volume changes) in HMA. This could serve as basis for a region-wide validation during the last decade, if only qualitative. See e.g. Huss and Hock (2015) who made use of the region-wide estimates of Gardner et al. (2013)

Reply: We have a section about validation of glacier model (section 3.3) in the revision. We also estimated elevation changes for individual glaciers directly from simulated volume and area changes, then calculated the average rate of elevation change for all the glaciers in each sub-region and compared them with remote-sensing estimates from 2003 to 2009 from Gardner and others (2013), Table 2. The correlation coefficient between the Gardner et al. (2013) estimates for the 6 RGI 5.0 sub-regions with data regional and our modeled regional averages is 0.7 which is marginally significant, ($p<0.1$).

Also note ELA evolution is a key parameter in the method. As a validation of the method, Zhao et al. (2016) calculated the ELA for nine glaciers in China, India and Kyrgyzstan, and compared them with the observed ELA time series by similarities of decadal trends and also annual variability. The ELA parameterization produced reasonable fits to observed ELA decadal trends on 9 glaciers, with a correlation coefficient of 0.6 which is significant ($p<0.05$, the values we give for p are single tailed Pearson correlation tests).

• the spread between the GCM ensemble members should also be discussed, as it probably impacts the results a lot.

Reply: We add the simulation results using GCM ensemble members, and discuss the spread between them in the revision.

Specific comments

Add uncertainty ranges to numbers in the abstract

Reply: done.

L50: add references to the summer-accumulation type statement (e.g. Fujita, 2008). Besides, it is highly speculative (and probably wrong) to say that all glaciers in HMA

are "mainly" of this type. See the classifications by Rupper and Roe (2008) or the classification by Maussion et al., (2014), which shows that large parts of HMA are not of the summer accumulation type.
Reply: Yes. In contrast to glaciers in higher latitudes, many on the Tibetan Plateau are summer accumulation type (e.g. Fujita et al., 2000), that is both surface snow fall and melting occur overwhelmingly in the 3 summer months of June, July and August, with little mass gain or loss throughout the remaining 9 months of the year. However some glaciers, especially in the northwestern parts of HMA are winter accumulation type (Maussion et al., 2014).

L85: I don't understand the need to use different inventories in this study. It seems much more consistent to stick to one, and give all the figures for the one judged more adapted.
Reply: We only use Randolph Glacier Inventory (RGI) 5.0 for glacier outlines. But different parts of the region uses different sources. The RGI 5.0 data inside China are based on the Second Chinese Glacier Inventory (Guo et al., 2015), which provides glacier outlines from 2006–2010, except for some older outlines from the First Chinese Glacier Inventory where suitable imagery could not be found - mainly in southern and eastern Tibet (the S and E Tibet RGI 5.0 sub-region), most of which were made in the 1970s. The RGI 5.0 data outside China are from the "Glacier Area Mapping for Discharge from the Asian Mountains" (GAMDAM) inventory (Nuimura et al., 2015) and nearly all come from 1999–2003 with images selected as close to the year 2000 as possible.

L90: please justify your choice of the median for the ELA proxy. What consequences does this choice have in the case of glaciers which are far from equilibrium, as it is the case in Eastern Himalaya?
Reply: In Section 3.3 In choosing the initial ELAs for each glacier, there are several reasonable alternatives (Zhao et al., 2016): i) using ELAs interpolated from the first Chinese glacier inventory, ii) median elevations from RGI dataset, iii) the elevation of the 60th percentile of the cumulative area above the glacier terminus. These three choices lead to a range of about 2.5 mm of global sea level in glacier volume loss at 2050. In this study, we use median elevations from RGI dataset, which corresponds to the median result
Table 2 in the revision shows that our model indicates E. Himalaya is in the largest negative mass balance of the sub-regions, in agreement with Gardner et al., 2013.

Sub-regions	Gardner and others (2013)	Modelled
E Himalaya	-0.89±0.18	-1.51±0.59

L99-100: rephrase
Reply: done.

Table 1: explain the gradients column in the legend, specify units

Reply: The unit of SMB gradients is m m^{-1}. We add it in the legend.

L120: reformulate "to calculate two or three SMB gradients with altitude", which is unclear to me
Reply: We change it to "We calculate no more than three SMB gradients using in-situ SMB measurements for every glacier in Fig.1 and Table 1. Following Zhao et al (2014), the SMB–altitude profile is constructed for every glacier by using its own ELA and these SMB gradients."

L125: volume area scaling must be extended with a relaxation time scale! See Marzeion et al., (2012) and Bahr et al., (2015).
Reply: We add relaxation time scale in the volume-area scaling, the same as in Marzeion et al., (2012).

L127: "by assuming all the decrease in area takes place in the lowest parts of the glacier": but how do you deal with growing glaciers?
Reply: We add how to deal with growing glaciers in the revision in Section 3.1 and see the answer to the first main point of the referee.

L143: "relatively small (<10%).": I wonder as to which percentage the authors would consider that the preciptiation changes aren't "relatively small" anymore. I personally find that 10% is quite a big deal.
Reply: We considered precipitation in the revision and removed these words.

L150: why not considering CRU (https://cr udata.uea.ac.uk/cru/data/hrg/), which has a resolution of 0.5deg?
Reply: We used CRU temperature data instead of Berkeley Earth Project in the revision, but compare the two results together in Section 3.3 That simulation was done using temperature alone as the glacier driver, so precipitation for each glacier was constant over time. The simulated climate ensemble mean forced volume losses in the period 2010-2069 were +4% (G3), -9% (G4), -11% (RCP4.5) and -13% (RCP8.5) different from the results using the CRU dataset.

L166: how are they different?
Reply: Yu et al. (2015), noted that was no significant change in surface temperatures after sulphate was injected in the GISS-E2-R model possibly due to the efficacy of SO2 forcing being relatively small as compared to CO2 forcing in the model. Neither do we also find a termination effect in GISS-E2-R under G3. Therefore, we not use any results from GISS-E2-R.

At the end of the methods section the reader is left with many questions about how the calibration of the α parameter is done, and how the uncertainties are handled in the study.
Reply: In Section 3.3 we discuss the calibration. For the ELA sensitivity to summer

mean temperature and annual precipitation, we use the zonal mean values from energy-balance modelling of glaciers in HMA by Rupper and Roe (2008). Alternatively, it can be estimated using an empirical formula for ablation and a degree-day method (Zhao et al., 2016). Zhao et al. (2016) calculated the ELA for nine glaciers in China, India and Kyrgyzstan, and compared them with the observed ELA time series by similarities of decadal trends and also annual variability. The Rupper and Roe ELA parameterization produced the best fits to observed ELA decadal trends on 9 glaciers, with a correlation coefficient of 0.6 which is significant ($p<0.05$, the values we give for p are single tailed Pearson correlation tests).

Fig 2 Fig 3: please make a figure following today's standards. Add country borders or topography (or anything that helps for orientation). Consider using discrete levels instead of continuous colors. Are the anomalies for the entire year or just the summer season?
Reply: We add country borders in Figure 1. As suggested by the other referee, we replaced Fig 2 and 3 with sub-regional line plot plots (the new Fig. 3 in the revision).

Fig 5: add the spread between the ensemble members
Reply: done.

Fig 6: the uncertainty associated with the various ensemble members should also appear in the spread.
Reply: done.

L317: deep convection
Reply: Changed.

Conclusions: part of the conclusions should be extended and moved to the discussion (in particular the comparison with other studies).
Reply: done.

L368: specify what "close" means
Reply: We delete "close" and write "The results projected by our method have higher means but smaller uncertainties than theirs, but do not differ significantly."

References
Bahr, D. B., Pfeffer, W. T. and Kaser, G.: A review of volume-area scaling of glaciers, Rev. Geophys., 95–140, doi:10.1002/2014RG000470, 2015.
Fujita, K. and Ageta, Y.: Effect of summer accumulation on glacier mass balance on the Tibetan Plateau revealed by mass-balance model, J. Glaciol., 46(153), 244–252, doi:10.3189/172756500781832945, 2000.
Gardner, A. S., Moholdt, G., Cogley, J. G., Wouters, B., Arendt, A. a, Wahr, J., Berthier, E., Hock, R., Pfeffer, W. T., Kaser, G., Ligtenberg, S. R. M., Bolch, T., Sharp, M. J., Hagen, J. O., van den Broeke, M. R. and Paul, F.: A Reconciled Estimate of Glacier

Contributions to Sea Level Rise: 2003 to 2009, Science., 340(6134), 852–857, doi:10.1126/science.1234532, 2013.
Marzeion, B., Jarosch, a. H. and Hofer, M.: Past and future sea-level change from the surface mass balance of glaciers, Cr yosph., 6(6), 1295–1322, doi:10.5194/tc-6-1295-2012, 2012
Maussion, F., Scherer, D., Mölg, T., Collier, E., Cur io, J. and Finkelnburg, R.: Precipitation Seasonality and Variability over the Tibetan Plateau as Resolved by the High Asia Reanalysis*, J. Clim., 27(5), 1910–1927, doi:10.1175/JCLI-D-13-00282.1, 2014.
Michaelsen, J.: Cross-validation in statistical climate forecast models, J. Clim. Appl. Meteorol.,26(11),1589–1600,doi:10.1175/1520-0450(1987)026 <1589:CVISCF> 2.0. CO; 2, 1987.
Rupper, S. and Roe, G.: Glacier Changes and Regional Climate: A Mass and Energy Balance Approach, J. Clim., 21(20), 5384–5401, doi:10.1175/2008JCLI2219.1, 2008.
Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-830, 2016.