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Interactive comment on "Glacier evolution in high mountain Asia under stratospheric sulfate aerosol injection geoengineering" *by* L. Zhao et al.

Anonymous Referee #2

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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)
- 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).
- 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.
- the calibration of the mass-balance (MB) gradients is extremely loose. If I understand well, the MB gradients are defined for one glacier wih 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).

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

Specific comments

Add uncertainty ranges to numbers in the abstract

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.

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.

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?

L99-100: rephrase

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

L120: reformulate "to calculate two or three SMB gradients with altitude", which is unclear to me

L125: volume area scaling must be extended with a relaxation time scale! See Marzeion et al., (2012) and Bahr et al., (2015).

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?

L143: "relatively small (<10%).": I wonder as to which percentage the authors would consider that the preciptiation changes aren't "relatively small" anymore. I personnally find that 10% is quite a big deal.

L150: why not considering CRU (https://crudata.uea.ac.uk/cru/data/hrg/), which has a resolution of 0.5deg?

L166: how are they different?

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.

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?

Fig 5: add the spread between the ensemble members

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

L317: deep convection

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

L368: specify what "close" means

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