Authors have now well-taken into account most comments from my first and second reviews.

I still fully disagree with the statement that the ELA corresponds to 0 elevation change. This is not a key point/finding of this paper but it is not glaciologically acceptable. The publications by Huss & Farinotti (cited by the authors to back up their assumption) do not agree with this statement. I reproduce below the Figure 1 of their 2012 JGR paper. Dh/dt does not cross the axis! I recommend that the authors use the much more conventional hypothesis that the ELA = median altitude of the glacier (see for example http://www.the-cryosphere.net/9/2135/2015/ or Figure 3 in https://www-cambridge-org.biblioplanets.gate.inist.fr/core/services/aop-cambridge-core/content/view/7A6999E78A3C73FDF340119462BFF313/S0260305500000239a.pdf/div-class-title-the-high-mountain-asia-glacier-contribution-to-sea-level-rise-from-2000-to-2050-div.pdf).

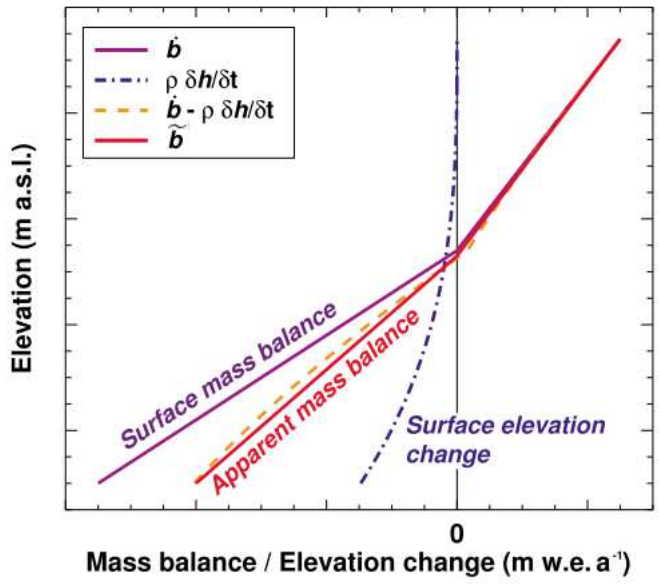

Figure 1. Schematic representation of the altitudinal distribution of surface mass balance $\dot{b}$, hypothetical elevation change $\rho\partial h/\partial t$, and apparent mass balance $\tilde{\dot{b}}$ over the glacier using linear elevation gradients (approximating $\dot{b} - \rho \cdot \partial h/\partial t$, see equation (1)).

In the event where the authors would decide to keep this wrong statement in their paper (I really hope not and do not see what they would stick to it), authors need to explain how they tackle the glaciers for which the dh/dt curves do not cross the 0 elevation change line (this is the case for some of the glaciers in Figure 5 and will be observed for many glaciers in a retreat state due to simple principles of glacier dynamics). More specifically, how do the authors quantitatively define the "altitude where the curve closes 0"? It is a bit too vague for a scientific paper and lack reproducibility. Do they use a specific minimum absolute value for dh/dt to define where dh/dt become undistinguishable from 0 based on the uncertainty of their measurements?

Further, is this definition of the ELA used to apply different SRTM penetration bias correction for clean ice and snow/firn? This is currently not clearly specified.

P13.25, authors state "Using those ELAs the accumulation area ratio (AAR) (Dyurgerov et al., 2009) can be estimated for each glacier and this is a parameter strongly related to long-term mass balance (König et al., 2014)". Significance of the ELA/AAR for debris covered glaciers is different than for clean ice glaciers. When the proportion of debris cover is large and the accumulation is partly made by avalanches the significance of the AAR and its relationship to mass balance / climate is dramatically

change. See for example discussion this in (Iturrizaga, 2011), P211) for Karakoram glaciers. Authors need to acknowledge this additional complexity.

I will now trust the editor in its ability to check carefully that this last round of comments is well-taken into account by the authors.

Good finalization of the paper.

P8 L19. Eastern

P3 L28. New paragraph before "There"

Some readers may wonder whether the WV DEM are available to others. Were they not restricted to 2015 post-earthquake studies as originally announced on the cryolist if I recall correctly (maybe to mention P5.10 or in the acknwoledgments) ?

P12 L13 fix spacing between words