This is an assessment of the revised version manuscript (MS) by King et al.

The revised MS is improved in several aspects but unfortunately one of the major issues that I raised in my first review has NOT been corrected. Authors are still believing/pretending that they measured the pattern of mass balance (for example with altitude, see the legend and axis title in Figure 5, 6 and 8) although they only measured the rate of elevation change (dh/dt, see comment 9.17 below). I think many of their conclusions (e.g., role played by glacial lakes in controlling glacier mass loss) would still hold without this basic error but the paper cannot be published in its current form. If the paper is publish like this, there would be a risk that many glaciologists would confuse dh/dt (readily available from DEM comparison) and mass balance in the future and this would be dramatic because these are two very different quantities with profoundly different meaning/interpretation. Only the glacier-wide averages of these two quantities are equal (with a proper density assumption).

Another major issue is the error estimate that is different from the one presented in the submitted MS. Authors have followed a non standard approach (not a problem per se) but not very clearly described/justified. In my view some errors are double-counted which lead to overestimated uncertainties.

My line by line comments are provided below (Page.Line).

Abstract and elsewhere in the paper. Space sometime missing between “w.e.” and “ a-1”

3.1-5 paragraph not well linked with the rest of the introduction

5.4 Two versions (X and C-Band) of the SRTM DEMs are presented. Authors need to clarify which one of the two was used. See comment from my initial review on this. By the way, why presenting the SRTM X-Band DEM if it is not used at all?

6.26 Authors used EGM2008 for the WV DEM geoid correction. But SRTM C-Band uses a different geoid, i.e. EGM96. It would have been best to use the same geoid. Further, if the DEMs are both registered to the geoid I do not understand why a 30 m systematic elevation difference (see Table 2) remain between them. I would have expected a few meters of bias, no more (orbital WV errors + differences between EGM96 and EGM2008). These unexpected systematic elevation differences make the whole DEM processing suspicious.

7.8 can the authors confirms (and write in the MS) that no penetration correction was applied for debris-covered areas? Was still unclear to me. Authors use an average value for the ablation/accumulation area because these are the one available from Kaab et al. However, they need to state that there is a potentially strong spatial/altitudinal variability in the SRTM penetration depth (depending on firn temperature and water content) and thus that this is source of error when examining the spatial pattern of elevation change.

7.23 Error estimate. Equation (1): First I do not understand why authors do this calculation, taken from Wang & Kaab. They are not really interested in the error of individual DEMs but rather in the error on elevation changes which is directly provided by Sigma_i. Or did I miss something?

Further, following standard error propagation, I would have expected that
$Sigma\_dh^2=Error\_dem1^2+Error\_dem2^2$
so the sign does not appear to be OK here. This error was also present in the Wang & Kaab paper unfortunately and authors should avoid propagating it in the literature. See http://ipl.physics.harvard.edu/wp-uploads/2013/03/PS3_Error_Propagation_sp13.pdf for example.

8.1. Write maybe “standard elevation differences over stable terrain (σ_{stable})”.

8.15 in their σ_{season}, the authors include the σ_{stable} that is the std deviation of the DEM differencing on the stable terrain. But this source of error has already been accounted for earlier in Eq 2? Rather the mean bias on glaciers between two WV2 DEMs acquired a few weeks/months apart could maybe be a better estimate for the (systematic) seasonal error.

9.17-19 “We do not quantify emergence velocity”. Of course, it is very difficult to infer mass balance (MB) from elevation change (velocity, ice thickness data are needed and not available, true). Then, locally, the values that the authors show are not mass balances but elevation changes. Despite this statement, in the rest of the MS, authors ignored totally that they did not observe mass balance. Maybe the authors believe that Himalayan glaciers are special and that in their case MB and dh/dt are equal? If this is so, they are wrong. It has been demonstrated for one of their study glaciers (Khumbu).
Authors can refer to Nuimura et al. 2011 to check that the magnitude of the emergence velocity (even in Himalaya) can match or be even much larger than the value of the surface mass balance. In Nuimura’s Table 4, the dh/dt are 0.7 m/a and the emergence velocity needed to retrieve the surface mass balance for some portions of the ablation area of Khumbu Glacier is... 5 to 6 m/yr, i.e. one order of magnitude larger!!! Clearly it illustrates how dh/dt and MB cannot be mixed/confused.
Nuimura, T., Fujita, K., Fukui, K., Asahi, K., Aryal, R. and Ageta, Y.: Temporal changes in elevation of the debris-covered ablation area of Khumbu Glacier in the Nepal Himalaya since 1978, Arctic, Antarctic, and Alpine Research, 43(2), 246–255, 2011.

9.21 Authors did not calculate mass balance but elevation changes! See comment (9.17). Consequently, the altitude where dh/dt approaches 0 is NOT the ELA. If this was true then the ELA would be above most Alaskan glaciers for example (See dh/dt vs altitude curve in Arendt et al., 2006 approaching 0 close to the glacier head, just one example among many studies showing that thinning can indeed affect the entire accumulation zone of some glaciers) but we all know that the AAR of most of these Alaskan glaciers is not 0! They still have an accumulation area. The same would hold in the Alps and many other mountain ranges. Altitude of zero elevation change and ELA have nothing in common.

10.4 "in the" repeated

10.5 Here I want to recall my above comment (9.17) to the authors that their dh/dt can only be interpreted as (glacier-wide) mass balance after averaging over the whole glacier area. This is not true for point, individual altitude band and considering ablation/accumulation areas separately because of the divergence of the ice fluxes. See text books.

10.29 authors did not measure the ablation gradient. They measured the gradient of dh/dt with altitude. See comment 9.17.

11.26 what do the authors mean by "here"?

13.19 I would expect the rise in mean temperature to be between the minimum and the maximum. But maybe I wrong? Maybe authors can double check the reference cited?

14.5 This is a conclusion inherited from the previous version of the paper that does not hold anymore. Or is it for a specific altitude range?

14.17 omit parenthesis

15.7 “m a-1” or “m/a”, author need to be consistent in their notations

15.27 what do the authors mean by "here" in this context?

26.5-7 This is already stated in the Method, no need to repeat I believe.

16.28 again, authors did not measure mass balance curves. This whole comparison is then problematic. See comment 9.17

17.33 does it mean that water is stored in the englacial hydrological network? Or in ponds? Maybe authors could clarify what is the “distributed water storage”.

17.7 no "s" for glacier margin

Figure2. Authors do not only show “surface lowering” as written in the legend and caption. There are some areas of thickening (!) in their map. Rather they show the “rate of elevation change”. Same for Figure 3.

Figure 5. How normalization was done should be described in the method section of the text (maybe with a reference to justify/explain it?). This normalization is really useful to compare the different basins and glacier type. Thanks for following my advice.

Figure 5-6 and 8. Caption and axis title are wrong. These are NOT mass balance curves. But curve of dh/dt

Supplementary. Figure 2. With this color scale one does not see much. Supplementary figure 1 (which by the way is very convincing) suggests that a color scale between -15 to +15 (with a step of 5 m) would be more appropriate.