Review TC-2016-48

General Comments

The potential of declassified optical stereo satellite imagery for glacier change detection is undisputed. Several studies quantified glacier volume changes using declassified Corona and Hexagon data, mainly focusing on small basins or individual glaciers only. For the processing nearly all of the studies used commercial software packages. From this point of view the study of Maurer is highly welcomed as it proposed an independent workflow for DEM generation. Moreover, the study of Maurer closes a gap of knowledge in terms of region-wide glacier mass balance investigations in Bhutan using declassified imagery.

The workflow of DEM generation has already been published in the ISPRS Journal of Photogrammetry and Remote Sensing (Maurer and Rupper, 2015). This paper builds upon this previous paper and presents an application in the field of glacier volume change assessment. All in all it is a nice paper and worth of prompt publishing.

Specific Comments

P 3, L 29. How were the blocks selected? Were they defined with respect to the glacier extent?

P 4, L2. “Points located on unstable terrain were excluded” – Based on ICIMOD glacier outlines?

P 4, L 21. Hexigon -> Hexagon

P 4, L 29. I am not familiar with glacier elevation changes in Bhutan. Comparing with the Everest Region 100 m seems to be suitable. However, the strong elevation change for glacier "k" and "i" might also justify a threshold of 150 or 200 m.

P 5, L 2. “Greatest accuracy” – What does it mean? Did you use a slope threshold to correct the outlines with regard to steep parts in the accumulation regions which have erroneously been delineated as part of the glacier? The delineation of debris-covered glacier tongues is often ambiguous. How did you cope with uncertainties in these regions (e.g. glacier “f” in Figure 3 shows a strong glacier thinning at the end of the tongue outside the glacier outline)?

P 5, L9-11. Data gaps in the accumulation regions are characteristic for DEMs generated using optical satellite imagery. Closing these gaps is indispensable for glacier mass balance calculations. Several approaches can be found in the literature, e.g. based on TINs (Surazakov and Aizen, 2006) or using the regional mean/median for individual elevation bands. Other studies assumed no change in the accumulation regions replacing missing data values by zero (Pieczonka and Bolch, 2013). Using the regional mean the authors assume a similar behavior for all glaciers of the same type. However, this must not necessarily be true. Taking this into account the proposed method allows the calculation of regional glacier volume changes but might not be suitable to calculate glacier volume changes for individual glaciers (e.g. P 10, L 4). Can you provide a difference image after extrapolation to judge the meaningfulness of the approach? You could also add a figure showing all three stages for an example glacier (difference image before hole interpolation, after hole interpolation, after extrapolation).

P 8, L12. The results from this study are not comparable to the results of Kääb et al. (2012) and Gardelle et al. (2013) due to different time periods. The authors should add other references when

writing “is comparable to other estimates derived from remote sensing […]”. Compared to the Everest Region (Bolch et al., 2011) the regional mass budget is less negative. Zemp at al. (2009) give annual mass balances based on in-situ observations on a broader scale. For Southeast Asia they found a moderate mass loss until 1995 with a subsequent acceleration since 1996, reflecting the higher mass loss found by Kääb et al. (2009) and Gardelle et al. (2012) for the last decade.

P 9, L 20. What is the source for the information regarding melt ponds? Did the authors use Google Earth for a visual inspection of the glaciers?

P 10, L 15 and L 20. In my opinion it is a decrease from -35 to -10 m in line 15 and an increase from 0 to -10 m in line 20 as the values are related to glacier thinning.

P 11, L 7. “Low slope and low surface velocity” – This is imprecise and some further information is needed.

P 11, L 9. “Near stagnant flow velocities” – This statement is not supported by any figure or by a reference.

P 11, L 14. “heavy debris” and “lighter debris” – What does it mean in terms of absolute debris thickness?

Figure 2. 8 different Hexagon DEMs are mentioned. Figure 2 shows only 3 polygons. This needs to be changed to 8 polygons to get the link between Table S1 and Figure 2.

Figure 3. The chosen color for the investigated glaciers is unfavorable in comparison to the bright background color. A different color for the outlines would improve the readability of the figure significantly (in particular for the glaciers “q”-“u”).

Figure 3. The current visualization using a continuous color coding shows an almost perfect fit between both models but conceals uncertainties in the data. The authors should use classes instead of a continuous color coding in order to allow a better distinction between areas of higher and lower deviations from zero, in particular for the stable terrain.

Figure 3. Glacier “a” shows a strong surface lowering in the middle part of the tongue followed by sudden elevation uplift at the end of the tongue. At the same time there is a strong thinning right next to the outline not mapped as a glacier.

Figure 3. Considering glacier “k” and “i” I would expect a surface lowering of more than 100 m. The threshold of 100 m used to exclude outliers might be too low. A value of 150 or 200 m might be more suitable, in particular for the ablation regions. Can the author provide a difference image before gap interpolation in the supplements?

Table S2. Area uncertainties for 1974 are missing.