

## ***Interactive comment on “Moderate mass loss of Kanchenjunga Glacier in the eastern Nepal Himalaya since 1975 revealed by Hexagon KH-9 and ALOS satellite Imageries” by Damodar Lamsal et al.***

### **Anonymous Referee #2**

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The paper by Lamsal et al. presents an estimate of glacier mass balance and surface velocities of the Kangchenjunga glacier in the E. Himalaya from 1975 to 2010, using Hexagon KH-9 and ALOS imagery. The topic is relevant to the Cryosphere, and this type of study is much needed for the eastern part of the Himalaya, where glacier mass balance is poorly studied. While I appreciate the authors' efforts for creating the DEMs needed for this study, I think the paper needs much improvement before it can be published; certainly it needs to be re-worked and re-submitted in a different form. Geodetic mass balance using remote sensing study that only presents a single glacier, with no validation is not really acceptable for a published paper.

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Some of the more technical comments on the DEM generation, glacier mapping etc were already addressed by reviewer 1, and I agree with those suggestions. In addition to those technical details which I also believe should be addressed, my major concerns are:

1. Extent of study area / representativeness: The main concern here is the focus on one glacier only seems insufficient for this study, especially since the study is based on remote sensing data. I strongly advice extending this to a larger extent, which would allow to better examine spatial tendencies of elevation changes across a larger region Given that the authors already have the two sets of images (ortho-rectified and referenced), this would require minimal effort. The choice of one glacier only would be justified if the authors had field-based mass balance observations to compare the geodetic mass balance estimates with, but this is not the case.
2. Focus on mass balance: This is the second major concern. I find it problematic to expand db/dz from the ablation area only to infer mass balance over the entire surface of the glacie, especially since the DEMs derived are sparse and do not cover the entire glacier surface. Also, the 4 scenarios for estimating mass balance from surface lowering are not very clear- and averaging all these scenarios lead to too much uncertainty. Furthermore, db/dz cannot be directly interpreted as mass balance changes unless the full glacier dynamics component is evaluated.
3. Objective of the paper: Given these concerns above, I suggest revising the objective of the paper, for example focusing on glacier thickness changes based on DEMs as in other studies (Kääb et al. 2012, Gardelle et al. 2012a) over a larger area, and compare with trends reported in these studies. Then potentially refer to mass balance changes, but with a lot of caution because of lack of validation. However, I suggest that these be a secondary (and less emphasised aspect). I believe this would provide a much more valuable contribution.
4. Role of supra glacier lakes/ice cliffs: This section is somewhat vague. The authors map the lakes based on the DEM (this should be revised as well to include spectral signal), and hint at the role of the lakes, but it is not clear whether the elevation changes reported over the surface of the lakes takes into account any

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shifting of the lakes, ie have the lakes stayed in the same place and just lowered (seems not likely so)? 5. Figures and tables: the paper seems quite cluttered with a lot of graphs, some of which are unnecessary (see detailed comments below). It lacks in some illustrations of the imagery, also a figure showing the extent of the images used. 6. English language: Some improvement in the English form is necessary- some paragraphs are hard to follow. Please see detailed comments. For example use of past tense vs. present perfect, and other nuances underlined below.

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/tc-2016-202/tc-2016-202-RC2-supplement.pdf>

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