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Interactive comment on "Topographic changes due to the 2004 Chuetsu thrusting earthquake in low mountain region" *by* Zhikun Ren et al.

Anonymous Referee #1

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Review of Ren et al., on the mass balance of the Niigata earthquake

The authors present a comparison of a pre earthquake (10m, from pairs of stereo images) and post-earthquake (2m LIDAR) digital elevation models. By making the difference after co-registrating them they claim to obtain landslide volume and thus to be able to better constrain the relation between coseismic uplift and coseismic erosion in a low relief region. Although this may be a relevant topic, the study currently contains major errors that plague almost all results.

Major comments The study is poorly written and many sentences are unclear, but the key issues are methodological errors. Almost at the end of the manuscript the authors admit that the pre DEM contain vegetation and not the post-DEM. In effect this means that where a landslide occur, its estimated depth will be D*=D+Ht , with D its real depth

and Ht the tree height. Of course for landslide on grass land (Ht«1) or very deep slides (say D∼20-30) Ht (typically 3-10m) may not matter. But in places with forest yes. The authors dismiss it because they find a difference of about 0 in a sub area of their study. They simply forgot to say that this zone is an alluvial plain covered mainly by towns and field, thus with Ht∼0 in most places. Any satellite image demonstrate this see the 3 figures of this review.

In contrast they "surprisingly" report (in Fig 5 absolutely no comment in the text) that almost all small landslides (>1000m2) are 5 to 10m deep when they should be around 0.5-2m typically (Larsen et al 2010). This strongly suggest a vast majority of measurement is tracking Ht not D. As a result almost all result and consequent discussion are flawed and not worth further consideration until the author make an in-depth analysis of where canopy effect may play, how much, and what are the resulting uncertainties on individual landslide and estimated erosion.

Some additional Line by Line comments Abstract: Several unclear sentences

L41: This sentence is very vague and confusing. The idea that earthquake can contribute to mean topographic base level increase, as to the formation of relief is pretty old (see King et al., 1988, Avouac 2007). We also know other tectonic processes than earthquake redistribute mass and affetc topography (e.g., interseismic processes). These facts are not very well introduced by the authors overall in the whole introduction.

L46 – 48 : Several of this reference are erroneous : some work do not relate earthquake to topography : e.g.; Montgomery and Larsen 2012 Hovius 2011 is about Taiwan , not the LongMenShan

L49: confusing wording: demonstrated that Ldsl are tought to ?

L51 I would suggest to specify first the location : Recent study in the arid foothills of Peru

L63 end of the sentence unclear

L64 what mean totally different volumes ? Which different methods ? Again wrong reference Marc 2015 does not present anything about coseismic landslide volume.

L112 : Change the wording

L118 : If pre eartquake DEM is from stereo pairs, the height of vegetation will be included. So how do you account for it? Is there a correction on the pre-DEM ? Then it need to be explain and its uncertainties described. Or is the Lidar giving you the post-DEM with elevation including tree height ? But in this case many landslide “depth” will be driven by tree height. For me this is a likely explanation of why most of the small landslides (10-1000m2) depth is between 5-10m in Fig 5. If you look at the global database of landslide (Larsen et al., 2010) in ths size range the mean depth should be around 1m (with significant scatter).

L207 : Where can we see that ?The authors need to support this claim with a supplementary figure at least.

L208: I disagree with its claim. Where there is no landslides the precision of the difference DEM may be high (low noise level) but this noise level may very well change between the landslide zone (with steep topography even if not very tall) and the valley to the North. Additionally the author compare the biggest landslides to the mean noise. . .They should compare to mean landslide depth.

L212 : How were shallow and deep-seated landslide classified ? Clearly I would not call deep seated a slide with a 1-2 m depth, while some of them have 10cm. . . And 10m is not shallow and except in some place most likely much deeper than the soil layer.

L234-238 : Here the author acknowledge, very late, the problem of canopy (this should be done in the method !). And then dismiss it on the argument that no systematic error are observed and that low elevation difference exist in the “blank area”. Well the reason is simple enough : in the carefully selected area of fig 4b, there is only city,

agricultural fields and river flood plain. SO very limited high ranging vegetation, and if the images were taken when fields were denuded it would make sense to obtain overall no elevation change.

Authors figures Fig 1-4 Overall the author show lithological maps everywhere that are almost not discussed. IN contrast a map differentiating agricultural lands, forest , grass land, and shrub or medium height vegetation would be much more useful and a potential place to start to evauate methodological error related to canopy.

Fig 1: could be an inset.

Fig 3C : The color scale need to be re adjusted to something like +10 / -10 Same for Fig 4, this stretch hides all details and just show the biggest slides.

Fig 5: We need to see the uncertainties on the parameters of the V-A relationships and the associated confidence interval on the plot (along the fitted lines).

In any case the biggest issue is that the trend of size with depth do not exist for “shallow” landslides. They tend to be randomly distributed around 10m, that is most likely a methodological error, not acknowledged nor discussed by the authors.

Fig 8B : No idea what the points are or the shade line and how they are drawn...

References used in the review: Avouac, J.âĂŘP. (2007), 6.09 Dynamic processes in extensional and compressional settingsâĂŤMountain building: From earthquakes to geological deformation, in Treatise on Geophysics, edited by E.âĂŘC. G. Schubert, pp. 377–439, Elsevier, Amsterdam

King, G. C. P., R. S. Stein, and J. B. Rundle (1988), The growth of geological structures by repeated earthquakes 1. Conceptual framework, J. Geophys. Res., 93(B11), 13,307–13,318, doi:10.1029/JB093iB11p13307.

Larsen, I. J., Montgomery, D. R., & Korup, O. (2010). Landslide erosion controlled by hillslope material. Nature Geoscience, 3(4), 247.

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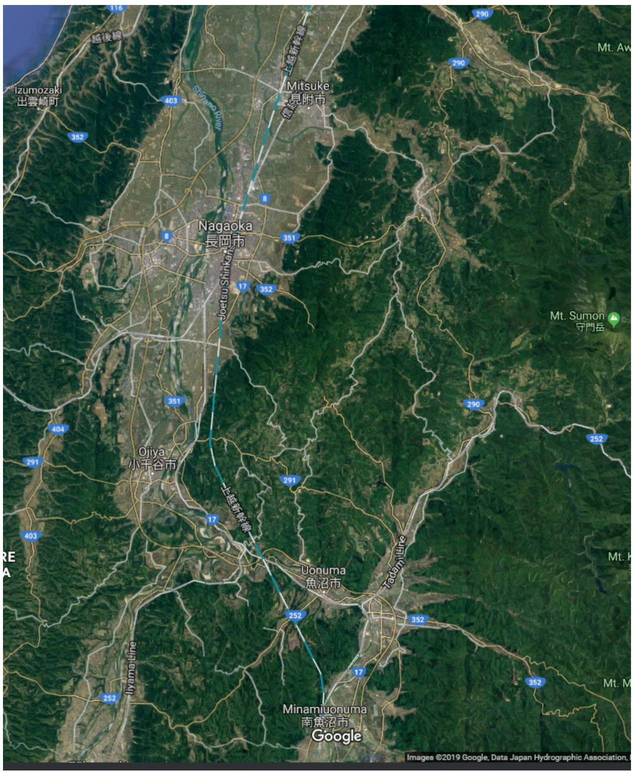

Fig. 1. Satellite view from the author study zone. The Autors Fig 4 is centered on the 3 valley juction west of Uonuma. Most coseismic landslides were in the valley north of Uonuma, near road 291.

Fig. 2. Zoom out of this review Fig1, north of Nagaoka (where the author “check” that canopy is not biasing their results)

Fig. 3. Zoom out of this review Fig 1 in the zone north of Uonuma where landslide were located and were the DEM difference is used to constrain them. Substantial patches of forest are visible on many slopes.