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Interactive comment on "Storm-triggered landslides in the Peruvian Andes and implications for topography, carbon cycles, and biodiversity" *by* K. E. Clark et al.

Anonymous Referee #1

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In their paper "Storm-triggered landslides in the Peruvian Andes and implications for topography, carbon cycles, and biodiversity", Clark et al. present a largely remote sensing-based investigation of landslide distribution in space and time. They draw on field-derived measurements of soil properties and carbon content to derive carbon yields. They draw a number of conclusions about the degree, timing, and distribution of erosion and carbon export in the Kosnipata valley. This is a well-conceived and well supported study that has incremental, but important, implications for geomorphic studies. In particular, the authors make some very good points about landslide inventory biases by spatial-temporal variability, and the possible control on biomarkers.

General comments: More information is needed around the calculation of soil organic

carbon. A table in the supplemental data would be appreciated here. Is a single density used for each pit? Seeing the depth intervals and carbon/density values would help the reader to understand why the calculated carbon stocks here are 2x (give or take, according to Figure 7a) the previous estimates. I would like to see an explanation of why these values are higher, not just that this dataset is more complete.

The mapped landslides include both scars and deposits. Please discuss the implications beyond the inclusion of low slopes. This would make some, but possibly not all, landslide areas too large. Might there be a topographic bias associated with this?

The relationship between erosion and topography is not clear. Based on the landslide inventories, the authors suggest that erosion rats are highest at low elevations and decrease with elevation. They also state that the low elevation plateau may be a result of high erosion rates not yet propagating onto the plateau. Based on their mapping, the knickpoints in the streams representing this boundary occur at $\sim$1400-1600 m a.s.l. The landslide-derived erosion rates peak at least 1000 meters higher. I understand that part of the paper shows the importance of the single event, but it seems that something is missing from the discussion.

Specific comments: pg. 637 – bottom: More landslides should result in lower concentration of cosmogenic nuclides in quartz. If sediment cosmogenic nuclide-derived erosion rates are lower than the landslide rates, then they have 'higher' concentrations than they should. This implies that there is either significantly storage of material (that does not make it into the river system), or that there is total bypass and poor mixing (i.e. cosmogenic nuclide-derived rates are local and not representative of catchments as a whole). This is not crucial to the paper, but it is an interesting topic.

Pg. 647 – middle: If the landslide inventory also includes depositional areas, then this is not a conservative estimate.

Pg. 647 – bottom: Why New Zealand? Give some justification for this comparison.

Pg. 649 – middle: You might want to distinguish the ‘work done’ by landslides (which is removal of material here) from the geomorphic work done by landslides in the topographic sense (steep lower slopes).

Pg. 650 – lines 8-12: does not make sense.

Pg 653 – top: this is too speculative. You could discuss the potential controls on topography, but should avoid discussions of erosion.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 631, 2015.