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Discussion paper

Interactive comment on "Geomorphological evolution of landslides near an active normal fault in Northern Taiwan, as revealed by LiDAR and unmanned aircraft system data" *by* Kuo-Jen Chang et al.

Anonymous Referee #2

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The study by Chang et al. investigates two large landslides developed along an active normal fault in a volcanic environment. Starting from previous knowledge about two large landslides in the area, the authors build their study on mapping the two landslides from visual interpretation of UAS imagery, as well as the interpretation of high-resolution digital topography (1 x 1 m LiDAR DEM). Based on their mapping, they estimate the volume of the two landslides by subtracting the present day topography from a reconstructed pre-failure topography. They conclude that the volume obtained is six times higher than the reported largest landslide volume in Taiwan. They further postulate that an active normal fault controlled the morphological evolution of the two landslides, and that ongoing faulting is responsible for maintaining landslide hazard condition in the study area. While it is interesting the attempt of the authors to relate landslide evolution directly to fault activity, I'm not fully convinced by the story they want to tell. I identified many issues and problems with the data (1), methods (2), and interpretations (3) that preclude this from being a convincing study. These include lack of clarity in data and methods and what was actually measured, issues with the interpretations and what the data mean, and a lack of depth in the interpretations and implications that are drawn from the data.

1) I have reservations about some of the assumptions that the authors have gone into their dataset. In particular, I don't know where their slip surfaces position estimates have come from. These are critical, because it is the postulated spatial coincidence between the slip surfaces and the present-day topography that provides the condition to calculate the landslides volume according to the method presented in the paper. The authors are not clear at this point: only short and general shrift are done at lines 15-20 page 8, but without any geological evidence or examples, it's hard to know what, exactly, they have considered for their assumption. Geology of the area is presented in figure 1, but the figure is not informative enough to support the assumption of the authors. Clearly, the present day topography is somehow related to the movement along the slip surfaces, but I think the authors need to be a lot more careful about what they say, and do a better job of documenting why the present day topography can be considered the slip surface of an old landslide. I also have reservation about the landslide detection, mapping and classification. Figure 5 illustrate the detection of zones affected by mass movements highlighted by ridges and scarps, which are commonly interpreted as the topographic response to movements along the slip surfaces at depth. However, the evidences strongly contrast with the assumption done by the authors about the coincidence between the slip surface and the present-day topography. This is a main issue that the authors should address to be their contribution convincing. In addition, I have reservations about the mapping itself. Landslide map-

ping should include the definition of the scarp area, deposit area, and both the flanks (see for instance Santangelo et al. 2015 NHESS, 15, 2111–2126; Guzzetti et al. 2012, Earth Science Reviews, 112, 42-66; Ambrosi and Crosta, 2006, Engineering Geology, 83, 183-200). Looking Figure 5, I really don't know where the limits (even supposed) of the two landslides are positioned. The circumstance undermine the possibility to visually appreciate and to quantitatively measure landslide area in map. Furthermore, the paper is not informative enough about the landslide type, landslide age (even relative age) and different generation of landslides recognized inside the old landslides. The information is necessary to characterize the landslide morphology, evolution and hazard, which are specific purposes of the paper. I think a more detailed mapping using the high quality materials (UAS imagery and LiDAR DEM) available to the authors should be add to the paper.

2) Although the method seems to be reasonable in theory, too many issues remain unexplained. For instance: I disagree with the assumption that detailed UAV imagery are better than aerial photographs and/or satellite images to detect and characterized large landslides. My own experience suggest quite the opposite. Indeed, UAV imagery and detailed LiDAR DEM are very useful to perform detailed studies. As a matter of fact, one of the more interesting piece of work in the paper is related to the characterization of the micro-topography of the landslides and the discussion about the possibility to apply the method to the study of gully erosion. However, gully erosion appear to be as a minor complication compared to the estimation of the landslide volume of a giant landslide. Complication is irrelevant here if the authors focus their paper on the calculation of the total landslide volume.

3) The final interpretation is not convincing and rise many question: Why just such two landslides developed along a regional normal fault? What about other places along the fault? There is somethings peculiar in the specific location of the two landslides? (i.e. relative relief higher respect to other places along the fault?) geo-structural setting different respect to other places along the fault and prone to landslides? cluster of

strong earthquakes? evidence of high vertical deformation rates? what else?) In the scheme proposed by the authors the fault is the main factor controlling both the onset and the disruption of the landslides, but no analysis support their conclusion. I have also reservation about the idea that normal fault activity has the effect of cancel the landslide signature (third diagram in the final scheme). I think quite the opposite; fault activity sustain relief formation, maintaining the condition for landslide development (see Bucci et al. 2016, ESPL, 41, 711-720; and Densmore et al. 1997, Science 275, 369-72). The authors conclude somethings similar at lines 27-29 page 12, but their statement conflict with the idea illustrated in the scheme. Finally, the authors never explicitly address time scales of the considered landslides and fault, as well as the probable mismatch in timescale of the landsliding and faulting processes.

Finally, I have reservation about the general organization of the paper.

The chapter Introduction is a blend (sometime confused) of general issues about landslide identification and characterization. I suggest to restructure the text, developing a sharper motivation with some clearer objectives. Also, quote the pertinent literature addressing the mapping and analysis of large landslides. Pertinent local literature help understanding the state of the art at local scale. The authors are not clear enough at this point. For instance at line 25 page 2 the authors acknowledge that the two landslides were already recognized. So why the authors define the two landslides as “obscure” if they were already recognized? I think additional information should be provided, and a comparison of previous and new results should be done. Similarly, the manuscript lacks of references to international literature addressing mapping and analysis of large landslide in active regions. Pertinent international literature help defining the framework of the study and it should be quoted along the paper (see for instance Bucci et al. 2016, ESPL, 41, 711-720; Scheingross et al. 2013, Geological Society of America Bulletin, 125, 473-489; Bucci et al. 2013, Physics and Chemistry of the Earth, 63, 12–24; Strecker M.R. and Marret R. 1999, Geology, 27, 307-310)

The chapter geological background (lines 14-23 page 3) is confused: it is hard to follow and to understand the polygenic history of the faults of the area. The chapter contain information negligible for the aim of the paper. At the same time, the chapter lack of potentially useful information about the age and deformation rate of active structures, seismicity, landslide events. Finally, lines 3-11 page 4 belong to method, not to geological background.

The chapters 3 and 4 mix up methods, results and discussion, which is also included in the following chapter: Discussion. This writing setting makes reading hard to follow and to understand. Please change the text of the manuscript including the following chapters: Methods (include here technical issues regarding UAS imagery, digital topography (1 x 1 m LiDAR DEM), how you define landslides, what do you map using conventional approach (i.e. stereoscopic aerial photo-interpretation), what new using UAS imagery and LiDAR DEM (would be good to see in map the differences), how you estimate the landslide dimension, how you carried out the morphological reconstruction); Results (includes the new data and maps); and then Discussion (what can we learn from the new data and what is the meaning also comparing to other works) and Conclusions (take home messages in short).

The chapters Discussion and Conclusion focus on the evolution of the two landslides, stressing the role of tectonics. However, the paper do not contain any new information/analysis/result related to tectonics. The evolution scheme drawn by the authors remain poorly constrained also by the lacks of geological evidences supporting the supposed coincidence of the slip surfaces and the present day topography. I suggest to reconsider in depth (or to drop) the part of the analysis related to the volume calculation of the two landslide, because it simply raises too many questions.

Apart the many issues and problems, the figures are good and the geomorphic application related to gully incision and related erosion of old landslides seems interesting, and I would like to eventually see it in print. I think the authors need to be more careful about what they claim, and more explicit about how they explain and relate their various data sets. If the authors could do a better job of documenting it, then the contribution could be considered for publication after careful major revision.

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