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Discussions

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.

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Thank you for the helpful comments on our manuscript. Please find below our response and modifications that we have revised in the manuscript following the referee's comments and suggestions.

Anonymous Referee #2 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.

Referee #2-1. 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 mapping 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.

Response #2-1: We have divided the above section into 5 separate questions (a to e), and responded these questions accordingly.

a) 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, but without any geological evidence or examples.

Response #2-1a: Indeed, to estimate the landslide volume, the original topography and slip surface are the key issues. However, regarding to an old landslide, the original surface is unknown. On the other hand, slip surface is usually covered by the slid mass, and is not easily exposed. Therefore, in this study we try to propose one of the methods to reasonably construct the original ground surface and assume the slip surface that likely located at the interface between the volcanic cover and the underlying

sedimentary rocks. The original ground surface is constructed from ideal volcano cone edifice. The sedimentary rock basement and the volcanic rock cover have been well mapped both on the geologic maps (Fig. 1 and the new Fig. 5) and in field survey in the region. Based on the distribution of rock types, it is supposed that the contact between the volcanic cover and the underneath sedimentary rocks may serve as a weak plane for the slip surface. The slip surface consists from the difference of material and the exposed different lithology. We have revised and improved the paragraph in the manuscript.

b) Geology of the area is not informative enough to support the assumption of the authors, and do a better job of documenting why the present day topography can be considered the slip surface of an old landslide.

Response #2-1b: We have now added a figure to show more detailed local geologic conditions. In the new geological map, many landslides that occurred in the study area and in the Tatun Volcano region were attached to demonstrate the distribution of landslides. Comparing the size, distribution and classification, the two largest landslide (XSL and CSL) were thus chosen as the target for this study. We have revised and improved the paragraph in the manuscript. On the other hand, the 2D hillshade map (the original Fig. 5, now Fig. 6) has been modified with the azimuth of shade illumination being 315° to better illustrate the landslide geomorphologic features.

c) 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.

Response #2-1c: The original Fig. 5 is now modified as Fig. 6. Indeed, ridges and scarps of a landslide are commonly interpreted as the topographic response of the movements along the slip surfaces at depth. However, the topographic feature responses reflect only the ground subsidence actually. Thus if the slid mass glides with a long run out distance or the displaced mass has been eroded away, both processes will preserved topographic relicts by distinct shutter ridges and scarps. In consequence, we interpret that most of the material has been eroded away from the perspectives of normal faulting and tectonic setting of the study area. We have newly improved the manuscript to better illustrate the overall framework of the study.

d) 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.

Response #2-1d: The original Fig. 5 (now Fig. 6) has been modified with the azimuth of shade illumination being 315° to better illustrate the landslide geomorphologic features. This new hillshade image shall improve the identification of landslide region visually, because not all readers are familiar with the landslide morphology.

e) 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.

Response #2-1e: The normal faulting in the region started from 400Ka and is activated continuously ever since. The faulting was identified in the Taipei basin area and northeastern offshore Taiwan, with the fault line situated on both sides of the study area. And the fault line was recently identified and linked together as only one normal fault in Tatun Volcano region (near and surrounding the study area) by the authors. In conclusion, for the relative age of the landslide, we interpret that the landslide has been triggered since right after normal faulting started and the formation of Tatun Volcano, which is far later than 200 Ka. Regarding to the different generation of landslide, the geomorphologic components show different degrees of preservation within the two observed landslides. Furthermore, the CSL is interpreted to have occurred from a combination of multiple landslide events. We have newly revised the manuscript to denote

the relative age of the landslide and the different generation of landslides.

Referee #2-2. 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.

Response #2-2: We have divided the above section into 2 questions (a to b), and responded the questions accordingly:

a) I disagree with the assumption that detailed UAV imagery are better than aerial photographs and/or satellite images to detect and characterized large landslides.

Response #2-2a: In Taiwan, heavy precipitation induced by the annual northeast monsoon modifies easily the landslide topography. On the other hand, the study region is situated within a national park and preserves dense forest very well. Both effects conceal detaiedl topography and nearly impossible to study directly from aerial photographs and/or satellite images. The same situation can be found in the two giant landslides (namely, Tsaoling and Jiufengershan) triggered by the Chi-Chi earthquake, where the vegetation colonization concealed almost all the topographic details, especially for the zone of accumulation in just ten years after the landslides occurred. That is why we employed high-resolution and high-precision datasets/methods, the UAV and the airborne LiDAR, to decipher the landslide features of the study area. And that is why we assert the quality levels of the datasets, and illustrated them in Figs. 2 and 4.

We have newly revised and clarified the documentation in the manuscript.

b) Gully erosion appears 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.

Response #2-2b: Yes, the gully incision is a minor factor to estimate the overall landslide volume. The method is used only to assess the landslide morphology and evolution. We have clarified the documentation in the manuscript.

Referee #2-3. 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.

Response #2-3: We have divided the paragraph into 3 questions (a to c) and responded accordingly:

a) 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?)

Response #2-3a: In northern Taiwan, the tectonic activity is in extensional regime, thus dominated by normal faulting in the study area nowadays. The Jinshan fault (JSF), and Shanchiao fault (SCF, also known as the Jinshan Fault with normal faulting mechanism), both of the faulting were being identified longtime ago in Taipei Basin area (Southwest to the study area) and in northeastern offshore Taiwan (northeast of the study area). And recently these two faults were identified to have linked together as only one normal fault in the Tatun Volcano region around and across the study area by the authors. The result was published in the Central Geological Survey project report written in Chinese, and the paper for international journal is now in preparation. On the other hand, there are many landslides within the study area and in the Tatun Volcano region, as shown in the newly added Fig. 5. Comparing the size, distribution and classification, the two largest landslides (XSL and CSL) were thus chosen as the target for this study. We have revised and improved the paragraph in the manuscript.

b) 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. The authors conclude somethings similar at lines 27-29 page 12, but their statement conflict with the idea illustrated in the scheme.

Response #2-3b: In northern Taiwan, the tectonic activity of the region is in extensional regime. The Jinshan normal faulting resulted in the formation of Taipei basin by over one thousand meter throw of the fault separation. The normal faulting has been very well documented recently, e.g. Teng et al., (2001); Shyu et al., (2005); C.T. Chen et al., (2007, 2010); Huang et al., (2007); and K.C. Chen et al., (2010). And this normal faulting may also cause the continuous eruption of the Tatun Volcano. The evidence of normal faulting has been recently identified in Taipei basin area and northeastern offshore Taiwan. And two original normal faults are considered to be linked together as a long stretched normal fault that may provide significant earthquake faulting. Finally, the total length of the Jinshan normal fault is more than 130 Km long. We thus interpret that the normal faulting has led to the formation of the slope daylight, as well as the volcano subsidence in the south of the study area. This process may likely lead to the formation of the landslide. Because the normal faulting activated continuously, the sliding mass may be transporting continuously to the Jinshan Delta. The original Fig. 13 (now modified as Fig. 14) demonstrates the general geomorphologic evolution ideally, so as to explain the wear off of the landslide deposits, especially in XSL.

c) 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.

Response #2-3c: The normal faulting started from 400 Ka and activated continuously ever since. The age of the Tatun volcano is smaller than 200 Ka. So the relative age of the landslide is most probably after the normal faulting and the formation of the Tatun Volcano, which is later than 200 Ka. On the other hand, the CSL and XSL preserve different degrees of landslide geomorphologic components, showing a combination of multiple landslide events. Furthermore, part of the fault branches is identified on the lower slope within the sliding area, prompting the faulting behavior truncates and enhances the erosion process. In conclusion, based on many aspects, the authors thus propose one model to highlight the possible landslide evolution that will be useful for further testing.

Referee #2-4. 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.

Response #2-4: We have divided the above paragraph into 4 questions (a to d), and responded accordingly:

a) 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.

Response #2-4a: Pertinent literatures are now added into the manuscript. The two landslides were already recognized from 40 m DTM by Prof. C. T. Lee of the National Central University from only personal communication. However, due to the lack of available datasets and without distinct features, the landslides were not analyzed in depth till this study. From climatologic point of view, the annual rainfall is more than 2500 mm in this area, thus a vast portion of the study area is covered by vegetation. Dense forest thus partially conceals morphological features and has prevented detailed geomorphic studies in the past. On the other hand, the heavy rainfall also enhances the surface processes, e.g., incision and erosion. As a consequence, the erosion effect also obscures the landslide features. We have newly improved the documentation in the manuscript based on the abovementioned points.

b) 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.

Response #2-4b: To discuss the landslide evolution, especially for an old landslide, the geologic and regional tectonics must be included. The polygenic history of the study area must be taken into account. In the study area, we consider many factors, including, lithology, normal fault, climate, vegetation, erosion and human agriculture activity etc., in order to access the landslide geomorphologic evolution. Regarding the slip rate of Jinshan normal faulting, it is shown between 8.2-1.8 mm/yr subsiding rate at different sites and in time intervals (e.g., Rau et al., 2006; Huang et al., 2007; Chen et al., 2010). This high slip rate creates the Taipei Basin, and may significantly affect the landslide evolution as well. But unfortunately, these slip rate studies were focused only on the Taipei Basin, and not on the study area. The manuscript is now reinforced and improved to clarify the tectonic factor and the interaction.

c) This writing setting makes reading hard to follow and to understand. Please change the text of the manuscript including the following chapters: Methods; Results; and then Discussion and Conclusions.

Response #2-4c: We have newly improved the manuscript according to the comment.

d) 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.

Response #2-4d: One geological map (Fig. 5) has been added to demonstrate the geological background of study area, and to better link the relationship between the regional tectonics and landslide geology and evolution. The manuscript has been improved accordingly.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-227/nhess-2017-227-AC3-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-227, 2017.