Review of Zhang

The work by Zhang et al., presents two landslide inventories obtained with satellite and aerial imagery over a large catchment (Koshi River) in the central Himalaya. One inventory contains rainfall triggered landslides (RTL) as observed in 1992 and in 2015, while the other contains earthquake-triggered landslide (ETL) from the 2015 Gorkha earthquake. They compare both dataset relative to landscape properties (altitude, slope gradient and aspect, soil cover, etc) and derive a susceptibility model from each inventory to assess whether both susceptibility model agree and can be inter-changed or not. The authors also propose a size cutoff and performs various analysis for landslide smaller or larger than this cutoff.

Overall I think some of the question discussed in the paper are worth to be investigated and published within NHESS, however the current manuscript is unclear or lacking details in many places, and for me the 2 main results stated by the authors (size and trigger matter) are poorly supported by their current analysis. In depth revisions are clearly needed in my opinion, and I propose below several directions to clarify and improve the analyses, whether or not the authors claims will stand after these re-analysis is unclear as of now.

Major comments

1/ The authors present new inventories but there is a lack of description of mapping : what about amalgamation of landslides (cf Marc and Hovius 2015)? What about the mapping of debris flow ? etc What about the mapping resolution effects on the size distribution roll-over? With airphotos and Google Earth what was the highest altitude where comprehensive mapping could be performed ?
Also I think a brief comparison of the ETL mapped by the authors with the public dataset of Roback et al., (2017) would be useful to validate mapping.

2/ In the introduction the authors state that susceptibility comes from Internal and External factor, but later you use no external factor for Rainfall. This is a problem I would say because your susceptibility maps for EQIL and RIL have all internal parameters in common, so it is a bit as if you assumed rainfall forcing was homogeneous across the study area, while it is not. I thnik it would be worth to try to constrain your RIL with a long term average pattern of Rainfall (i.e. climatologic mean summer rainfall ?). This can exactly be done with a TRMM climatology, as presented by Bookhagen and Burbank 2006. Other option may also be possible. This would be a great improvement for the paper, and should be at least mention and discussed.

In any case, the comparison of the two susceptibility model does not necessarily depends on the different trigger but very possibly on the relevant landscape properties, as the coverage zone for the two model are very different. I strongly think that this possibility needs to be quantitatively assessed before possible publication.

3/ The author spend quite some time discussing size-effects in the introduction and in their analysis, but their is almost no explanation on how they choose/find their threshold for small or large landslide size. Second : In Fig 5, 6 and 7 (and maybe 8 at least for ETL) there is nothing that strongly suggest any significative difference between small and large landslize. The statement that "size matters" in the title, abstract and conclusions is for me completely unsupported. Further, I do not see really any place where the authors summarize in what size would matter (in the result section) and why it could (at least in discussion).

4/ I think the purpose of the paper and its relation to the state of the art literature is not very clearly presented, and would suggest that the authors try to clarify several parts of the introduction (cf. Minor comments).

5/ The discussion and conclusions section is using vague or inaccurate formulations and is missing a lot of references (there is only 1 on the rainfall pattern !!) on the importance of the seismic shaking pattern for example, on the elevated landslide susceptibility caused by loose landslide deposits or by slopes damaged by the shaking but unfailed. Potential model bias or difference in the mechanics of small or large landslides are also not discussed. Significant improvement are possible and needed (cf. Minor comments).

Detailed comments

L47 "To investigate whether earthquake- and rainfall-triggered landslides inventories have similar area-frequency distributions, area-volume relations and spatially controlling factors, it is important to collect event-based landslide inventories. The difficulty is to collect complete inventories that are independent for earthquakes and rainfalls. Many studies that compare the characteristics of earthquake- and rainfall-triggered landslide inventories focus on mapping landslides triggered by rainfall after major earthquakes."
>> The question underlying this study is unclear. The literature overview seems biased and inexact. Since decades they are indepedent rainfall inventories : New Zealand, Taiwan, Guatemala (Hovius 1997, 2000, Malamud, 2004) and others...
The study cited on L51-60 presumably looked at rainfall associated to EQ on purpose, to study whether or not an earthquake affected the properties of subsequent rainfall induced behavior.

L68 "There are fewer studies that compare the two triggering mechanisms in an independent manner."
Fewer ? Then cite them or say No studies. Malamud 2004 did. Meunier too. Again it is unclear in the introduction what the authr want to compare ? I recognize that there is a value into comparing rainfall and EQ induced landslide in the same area, to normalize for landscape properties. But if this is the aim of the authors this is not clearly stated.
I also do not see the problem of the study of Lin 2006 and Chang 2007 in Taiwan : They mapped rainfall landslide before the EQ exactly has the author are doing here.

L71-72: I am not sure "potential causal factor" are appropriate terms, given the trigger could also be considered a necessary term to "cause" the landslide. In-situ properties maybe although this is almost identical to internal factors...
I also note that from a physical point of view I would say that landslide occurrence is the convolution of a susceptibility term (due to in-situ/internal factor) and a forcing or triggering term. This may be the most adequate view point for a landslide event analysis (e.g. Meunier 2013, Barlow 2016).
From a probabilistic point of view, used for hazard analysis, the landslide susceptibility does not design the intensity of the response of a slope to a given forcing, but the long-term probability of landslide occurrence, including both in-situ properties, and the probability of various trigger. This is most suited for historical landslide inventories, where individual triggers are not or poorly constrained.
The authors do not really stick to one frame that makes the term susceptibility ambiguous in their study. Indeed in they state in essence in L70-71: Susceptibility (probabilistic sense) depends on internal factor (that makes area susceptible (physical sense)) and triggering factors.
This sentence and probably couple of others could be rephrased to avoid this ambivalent and possibly confusing uses.

L83 : "There is no clear evidence shows the difference on morphology between rainfall-triggered landslide and earthquake- triggered landslide"
>> Unclear statement. Could the authors specify what they mean with morphology ?
Also incorrect grammar :"that shows" or "showing"

L84 : also unclear. Rephrasing needed. Which statistics?

L92: huge slides ? Give a size range maybe.

L95: "whether it is possible to utilize inventories of earthquake triggered landslides (ETL) as inputs for analyzing the susceptibility of rainfall-triggered landslides (RTL)."
Depending on what authors means by the "susceptibility" here (cf comment above), the problem can be ill-posed given that obviously Rtl and ETL depends on a different trigger and thus will likely show different patterns (as shown by other studies: Meunier et al., 2008, Marc et al., 2018)

L151: It is unclear what you did with Landsat and ASTER DEM. ? Map or only adjust locations of landslides mapped with Google Earth or topo maps? The use of "therefore" is confusing.
The author should precise (in Fig 1?) where Topo maps where used and where Google Earth. With overlap or not ? Is the mapping style in topo maps consistent with Google Earth ?

L155 : resolution of satellite ?

L159 : Confusing sentence, clarify or rewrite.

L160 : consider replacing "rainfall impact to landslide" by something clearer, like : new or reactivated landslide due to subsequent rainfall.

L161 : which pre EQ image ? Google Erth or other... Estimation of the areas where pre or post EQ imagery did not allow mapping (because of clouds or shadows)

L164 : You said above you did not separate different zones of the andslides. How did you choose where was the initiation point? Is it the hghest point? Taking a single pixel as source or scar zone may bias your statistics. Why not considering a scar surface in the upper part of the polygon ?

L166 : Line 151 you said you use ASTER GDEM (30m). Be consistent. There is absolutely no reason to use a 90m dem while SRTM 30m is available. For quantitative slope assessment it will make a difference and analysis should be re performed with the highest possible resolution.

L167 : Explain how you determine where the river network start, as this is not done by arc GIS.

L172: you mean it is from Shakemap ? At which resolution ? In any case a few sentences on how shakemaps are derived and on what are their limitations (no topographic amplification, no constraints on site effects within mountainous area, interpolation with heavy weight given to station measurements even in areas with very different setting) is needed, together with a couple of references. I also think a map of the shaking in the Koshi, with landslides indicated, should be shown at least in supplement.

L183: Did you use distance to river (as suggested above) or not ? What is relative relief , computed at which scale ? Same drainage density ? Distance to fault, which faults ? I think a supplementary figure with the different (relevant) susceptibility factor would be useful.

L207-210: which method did you use to determine the Beta exponent and the threshold size ? Clauset et al. 2009 is the recommended approach (and they provide script to reproduce their analysis). Are the different estimates significatively different (i.e., what are the uncertainty on them)? ETL-All and the two RTL dataset have very close exponents.

P9 214: landslide size definition : is there a mistake or this classification is discontinuous ? small <1000 ; 1000< medium <10,000 ; <100,000 large...
What about landslide between 10,000 and 100,000??

L216: why 6000? you say it is based on FAD but without explanation... The sentence above is meaning less, which FAD analysis ? Which field exp ?6000 i the power-law cutofffor ETL but is in the roll over of RTL....
Also a few sentence on the meaning of the roll-over (and its sensitivity toresolution censoring) and of the Beta exponent and how it may be linked to physical properties is needed ! Cf Pelletier 1997, Stark and Hovius 2001, Stark and Guzzetti 2009, Frattini and Crosta 2013,

L224: For this initial correlation did you use ETL or only RTL ? If ETL was used what about PGA ?

L229-231: I am not sure this comparative analysis in terms of altitude or other parameters make any sense : because the difference will not have any thing to do with EQ or Rain , just to the fact that one dataset (RIL) covers 10-20 times more area, with a vast area at low elevation. Instead the ETL are limited, because of the fault location, to a small zone with high elevation.
I think all this analysis should be redone : ETL and RTL should be comapred to the landscape within which they occur, so that it i not absolute elevation or slope or aspect that is analyzd but fraction of the lanscape (percentile of landscape elevation for exemple, or analysis of oversampling or undersampling of given slopes or aspect. Cf Meunier 2008, Barlow 2016 etc). Fig 5 should also be updated.

L234: Is this based on the land cover maps ? Or is this from the imagery ?

L244: Missing word...to the?? direction ?

L264: gully density ? Or drainage density ? Be consistent !

L267-269: Could you comment on the values given for the different model ? It reaches 24 / 22 for ETL against 7 /6 for RTL. The methods sequence could include some more details to allow the author to have an intuition about the relative importance of different parameters.

L284: Obviously landslide susceptibility of ETL is giving only high suscptibility where you had data... As mentionned above you should also show the Shaking map ...

L289 : EQ without effect on large landslides ? The argument that large landslides occur only close the fault may be true for very large landslides but seems unikely for landslides down to 6000m2 that is not so large.

L335 : I thnk this conclusion is erroneous, or at least not demonstrated by the authors. Because the ETL model includes PGA, and also because it is based on a much smaller part of the landscape, a subarea where landslide are located in a different environent copared to the zone affected by RTL.

I think only by limiting the model development in an area where both RTL and ETL are widespread could the authors try to test this hypothesis.

L340: You repeat this result that is completely obscure in the main text. There was no reason given to this threshold value.

L342 : You never demonstrated the correlation in altitude and aspect was due to precipitation... The following sentences are interesting but a bit weak. The use of some rainfall climatology (as existing with TRMM for example, would be an actual demonstration).

L349: Should be rephrased. The epicenter is extremely far from your study area and seismic waves propagates in all directions. Second part may refer to seismic directivity that relates to wave interference. I think a discussion in terms of the ground motion pattern is what you mean. (and it is difficult to discuss without showing the shaking in a figure...)

L365: The forcing extent are different within this catchment. You need to discuss it, and for that you need to show shaking and rainfall pattern, both essential information that are missing !

L368: "Some more detail information could be included in large scale research"
>> Like what ? why would it help? and why didn't you include it ? As of now this sentence does not bring anything to the reader.

L376: "Whereas, the use of rainfall-triggered landslide maps can be of some use for predicting the occurrence of earthquake-triggered landslides, one should be careful, as the specific location of the earthquake plays a dominant role."
>> Not sure Whereas is the proper word. Anyway I do not think there is anything new for the community in a sentence like that.

Fig 5 : Why show Altitude vs other param? This display does show nicely the difference in altitude between datasets but not really with the other parameters.
Further it is hard to interpret anything when the distribution of landscape parameters is not shown... I think the authors must show the distribution of landscape properties (as classically done in the literature) slope gradient, aspect, altitude; stratum etc in the study area in black and then the ones of landslide RTL/ ETL in red / yellow on top for comparison.

Fig 6 : I suggest that you put all RTL in the left column and ETL in the right. It will make the figure less confusing and subplots easier to compare.

Fig 8 : large RTL are better predicted. Do you think this is physical or it may be a bias due to the higher difficulty to map small landslides? Also is there any ROC difference between RTL at any size and the 1992 or 2015 inventories ?

Fig 9 : Comparison is not ideal : ETL susceptibility is likely driven by the fact landslides are limited to a very small subset of the Koshi.

Technical comments:
L85/86 : "are" missing between volume smaller/larger

L97 -> hazard and risk assessment (i.e. remove 1 assessment)

L214: From the Biblio it should be Tong et al. 2013. Given that this is a book in Chinese I doubt that this references will be accessible by much reader... and not sure it is essential .

L 385 : Weather > Whether

Fig 7 caption : "Statistics" ; susceptiblity x2 > missing "i"

Overall the phrasing is a bit awkward or erroneous in many more places than reported here, and I invite the authors to carefully read again the manuscript.

References used in the review and not in the study:

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