Review on Tan et al.,

Landslides distribution at tributaries with different evolution stages in Jiangjia Gully, southwestern China

General summary : The author study a debris flow prone catchment. They performed some landslide mapping and some topographic classification of sub-catchments and attempt to link the two to propose that hypsometric analysis may be useful for landslide studies. The topic may be of interest for NHESS readers. Hypsometric analysis in relations to landslide are relatively rare, so it may be novel, but therefore the utility of such new index also need to be thoroughly demonstrated, that is not the case in the current state of the paper.

Major concerns:

1/ The quality of the writing is very variable and many sentences are hard to understand and/or highly ambiguous. Some English support and clarification would be needed before the author resubmit. The two last sentences of the conclusions (L325-329) are of these sorts.

2/ The quality of the figure caption is extremely low. A number of figures (10B, 11B, 12B and 13) contain lines (presumably fitted) without any explanation on the functional form and the fit criterium.

3/ Overall the author present various analysis, more or less standard, eg, EI ofHypsometry and inflection point in the Hypsometry, and often relate it to landslide or debris flow through bold and totally unsupported/unreferenced statement, as the few examples below:

L183: "For example, inflection points in EI between 0.45~0.55 are generally higher than those in EI below 0.45, indicating that such tributaries are more prone to landslides activities"

L186-187 : "the more concave the [hypsometric] curve is presented, and the smaller of the elevation value corresponding to the inflexion point is, which indicates that the elevation changing in unit area is small, such a tributary is not conducive to the occurrence of landslides."
(Note that these two statements are even done before presenting the result of the landslide maps...)

L223: "The major branches of JJG, the Gully of Menqian and Duozhao, are distinctive in debris flow and landslides activities."
L241: "Meanwhile, lower LD and larger LA_p is in the downstream, which is at the old evolution stage, which means that with the occurrence of historical landslides or large landslides in slope surface, the tributary has reached a stable state."

All these statements make it sound as if the conclusions are known before hand (what drive landslides in which part of the study area), and the methods of the authors (EI for example) have already been largely demonstrated to be relevant to landsliding. This is not the case to my knowledge, and likely why no references are given by the authors in this type of statements. Several of these sentence are also good example of the ambiguous and unclear writing in themselves.

4/ The landslide mapping is not defined clearly enough for the readers to understand what is mapped and to what it can relate. This means again that many of the interpretations are ambiguous. I give suggestion to strengthen and clarify the mapping description in the line by line comments.

As a result my general feeling is that most of the author claim are unsupported, and that although the topic and approach may bring something of interest to the community, the paper is at the moment quite far from being at publications standard: I think that many new analysis should be performed (typically a probability ratio or relative hazard approach (cf Milledge 2019, Rault et al 2019) with associated uncertainty level should be performed to demonstrate the utility of EI for landsliding, and a comparison with simpler classic index such as slope) and most of the paper should be rewritten/updated accordingly. This is not so far from rewriting the paper from scratch.

Below I provide a number of other criticism and suggestions:
Odin Marc

Line by Line comments

Introduction: It seems strange to me to put emphasis on the method of the paper rather than on its object (landslide and debris flow hazard).

L44-48 : The fact that EI merges several topograhic effect (slope, curvature , others) in an integrated index, is more likely to blurry rather than reveal underlying, physical landslide control…
However, I agree that it may contains information about disequilibrium state of subcatchment, river incision state or other long-term geomorphological effects that may have influence landsliding. But choosing a complex index the author must demonstrate that any potential link between EI and landslide is not simply due to a correlation between EI and well-know landslide

L62 63: The question of the link between EI and material supply (not clear what this is) and debris flow frequency is stated and will be repeated in conclusions but almost without any other mention in the result or discussion section (except in 5.4 under the form of a general discussion on debris flow where EI is not even mentioned). This is a problem and the paper in its current state should focus in Introduction and conclusions only on the link between EI and landslide, not debris flows.

Landslide mapping methodology : It is overall too long but also lacking important elements :
L114: is slightly ambiguous, because no article was used. If only one image was used I would write “A or One Quickbird image was purchased to create an inventory...”

L121-122: This sentence is subjective and not bringing any information on the method. Remove.

L125: From the Fig 8 it does not seem that polygons are initiation zones of landslides but the whole affected area, including runout and deposit.

L128: This individual delineation does not seem to have been performed judging from the map in Fig 8.
To show that, only landslide outline should be shown, and preferably 2-3 landslide complex should be shown in a supplementary figure where the outline can be compared to the imagery.

L134-135 : The author should state here (or at the start of the 4.1 section) what are the landslide they mapped : recent soil or rock avalanches ? Debris flow ? Large, old deposits ? Large slope instabilities visible through deformation indicators ?
Typically small shallow disruption of the soil and vegetation may have been caused by recent storm activity, and be recovered by new vegetation in a few years, while large, undated deposits may be 100-1000 of years old, and have a very different origin. Knowing what contains the inventory is essentila

for any interpretation in terms of the landslide triggering and susceptibility, as well as for their geomorphological impact.

L136: The accuracy of what was checked by GPS ? Currently this sentence is cryptic and must be precised.

L154-160 : this refined classification is not clear in Fig 5 and does not seem to bring anything as you go back to Strahler youthful/mature distinction a few lines below (L165).

L161-163 : The preference for the Weibull distribution is not demonstrated. Anyway it is not needed for the conclusion of the author, that is that most catchments are between 0.4-0.6.

L165 : Unsupported and unexplained statement.

Section 3.3 : Very poorly written section with unclear message and no obvious support from the figure 7. Nowhere it is explain why and how EI is useful for this studies. The author just claim it indicates high or low landslide activity, without explanation or support.

L207: "It is clear that major of landslides are distributed in subregions of III and IV, with EI between 0.55 ~ 0.75."
No it is not clear. I see many landslides in II. The only way to show that EI has a substantial impact on landslide probability would be to compute the ratio between the fraction of landslide in different EI class and the fraction of the study area taken by each of this class. Thus a relative Hazard could be defined and analyzed, see the methods in Rault et al., 2019 and Milledge et al., 2019 for examples on the link between landslide and other topographic metrics. IN any case Fig 9 should be replaced by such an analysis as it will never allow to make any quantitative deduction.

Section 4.2.2 : I have the impression the author should spend more time thinking to why LD seems to increase monotonically with EI while LA does not. If this is not noise, this would suggest a very different behavior between small landslides (driving LD) and large landslides (driving LA).
The meaning of the lines in the bottom subplot of Fig 10-12 is not explained.
For me Fig 11 brings nothing : upper subplot is equal to Fig 10 and Lower subplot look just like noise.

L246 : I am quite sure Malamud 2010 does not exist. The reference in the bibliography has correct title but wrong date, should be 2004. Further, all the study you cite also clearly observed and analyzed the existence of a roll-over, that is a decay of frequency below a modal area. This does not seem to exist in your data.

L251 : "The verification of power law confirms that the landslides area interpreted is reliable."
The sentence is logically fallacious, and additionally the lack of a rollover and the very high exponent (4.32, while exponent above 3 are already rare) may suggest that the mapping is wrong or problematic. Further absolutely no information on how the exponent was obtained is given. I would suggest reading Clauset 2009 and using their method.

Section 5.2 and 5.3 : The author align a succesion of poorly explain and poorly supported statement. As a striking fact these two sections (about half the discussion) are devoid of any reference from the literature. Nothing allows the reader to evaluate /understand the discussion relative to the Wenchuan earthquake.

5.3 could have been an important section, trying to demonstrate why EI may be a better index than more classical index such as slope or flow convergence, but it is not reaching this goal yet. The method/figure are extremely naive and again I would recommend the author to go and inspire themselves from work that uses probability ratio to test whether some areas of the landscape are more or less prone to landslides (Rault et al., 2019 and Milledge et al., 2019)

L254-258 : Landslide intensity (LD and LA) satisfy the Weibull distribution (but the author do not say how they tested and validated this claim). In any case what do the conclude from it ? I do not unerstand the link between this distribution and the claim from the author that "the landslides of most tributaries are distributed on small scales."
I am not even sure to understand what they mean.

L272 : This sentence as nothing to do here, it seems like a method comment on why you chose this study area.

The conclusion start with a summary of results, but I am unconvinced by many of them (see comments above). Then it ends with cryptic, unsupported statement. So it shouldlikely be rewritten fully after the results and method have been updated thoroughly.

Figures and Tables:
Table 1: What is the use of this table ? I do not see much use of having all the number/name of sub catchment… Clarify the utility of these numbers of remove.

Fig 2 : a lot of missing info in the caption: Where is the picture ? What are we supposed to understand from it ? Put the mean density rather than inequalities in the right plot. Does it make sense to show the sequence ? Or a mean and standard deviation (or interquartile range) would be enough ? The mean velocity does increase but scatter and overlap remains large.

Figure 4 : Are the number in parenthesis the n and k values ? Again a Caption/Figure that cannot be understood without ambiguity.

Fig 5: What is below the catchment EI color ? Something from blue to red that is confusing and useless… remove it. I have the impression category V is not in any catchment. I do not see a xlear spatial pattern of EI within the study area

Figure 6 : Why a Weibull distribution ? What is the fit quality relative to your data ? A Normal distribution may be just as fine given your histogram.

Fig 8 : What is the background ? A satellite image ? It is very poorly visible, with half the study area covered by solid landslide polygons.

Fig 9 : Useless, both info (EI map and Landslide map) exist in other figures. Should be removed.

Also the landslide maps is problematic to me : We cannot know what the author have mapped, without images from the satellite of from the ground, and given that the mapping method is imprecise.
In any case, it seems clear to me that some polygons are likely amalgamated landslide complex rather than individual landslide, with all the problem that amalgamation can bring (see Marc and Hovius 2015).

Figure 13 : This figure is absolutely useless. As it should obviously be in Log-Log coordinate to see anything…

Fig 16 and 17 : What is in these figures ? The mean the median ? What is the error bar meaning ? Without a statistical test, and/or some measure of uncertainty it is not clear that the difference in the different bins of EI are meaningful…

References :

Marc, O. and Hovius, N.: Amalgamation in landslide maps: effects and automatic detection, Nat. Hazards Earth Syst. Sci., 15(4), 723–733, doi:10.5194/nhess-15-723-2015, 2015.

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Rault, C., Robert, A., Marc, O., Hovius, N. and Meunier, P.: Seismic and geologic controls on spatial clustering of landslides in three large earthquakes, Earth Surface Dynamics, 7(3), 829–839, doi:https://doi.org/10.5194/esurf-7-829-2019, 2019.