Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2020-131-RC2, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Landslides distribution at tributaries with different evolution stages in Jiangjia Gully, southwestern China" *by* Xia Fei Tian et al.

Anonymous Referee #2

Received and published: 18 October 2020

Overview and general comments

The manuscript deals with the evaluation of relationships between landslides distribution and the morphologic evolution stage of watersheds, as described through an index (EI) obtained by analysing the hypsometric curves. The topic may be considered in principle interesting and suitable for publication in the NHESS journal. Nevertheless, the manuscript suffers from some relevant weakness points.The Introduction doesn't clearly state the state of the art about the relationships between EI and the landslide distribution/processes and the new contributions the paper is aimed to provide for supporting the relevance of the index. Input data, methods and results sections are not

well developed and split each other, so often it's tricky to recognize where results of the paper are reported, and discussion is somehow developed also outside the relevant section. Figures and captions are quite poor and in some cases lack of relevant information (sources, symbols, . . .). In the Discussion, a robust assessment of the quality of all the different kinds of results is not provided, and links to the existing literature should be improved. As for the significance of results and conclusions, only factors related to the hypsometric curve are considered, while distribution of factors like lithology, state of activity of geological processes, other geomorphological factors, rainfall, land cover are not taken into account (quantitatively or qualitatively). So, Authors do not investigate how much spatial heterogeneity of these factors (which may be linked to landslide distribution) may affect the results of the research.

For the above reasons, I think the research needs to be further developed for some issues and the manuscript would undergo an extensive reorganization, including a substantial editing to improve text fluency.

Minor comments

Abstract

The Abstract is not self-supporting and it's probably useful only to those researchers with expertise on the hypsometric curve and its analysis.

Line 14-16: unclear sentence.

Line 17-18 and 21: both the surface erosion index and the evolution index (EI) are reported to be the integral of the hypsometric curve.

Introduction

Line 41: the dissection index is used without introducing it and without adding the relevant literature.

Line 44-45 and 46: unclear sentence.

Line 49-51: older literature on these topics could be taken into account. Moreover, also much literature focusing on regional scale analyses may be found.

Line 51 and 57: it seems that Authors are using here and all over the manuscript, the term gully as a synonymous of watershed or catchment. I do not agree with this assumption, as the term gully refers, for example, to "...permanent erosional forms that develop when water concentrates in narrow runoff paths and channels and cuts into the soil to depths that cannot be smoothed over by tillage any more..." (https://doi.org/10.1016/B978-0-444-53260-2.10013-4), i.e. smaller features (one or more orders smaller). I suggest to carefully check this point.

Line 60: Authors should demonstrate that, in the considered watersheds with different landslide frequency, EI is the only parameter (related to landslide development) which is not constant.

Line 64-68: unclear sentence.

2 Study area

Line 80-86: "active neotectonic movement, faults, and folds". Please add relevant literature to support this sentence, because different spatial characters of neotectonics activity could influence slope instability in the different watersheds. Moreover, the geological description is too poor and "mixed" together with vegetation and landslide information.

Line 88: are the statistical data obtained from either this work or the literature? In one case this information should be moved to the results, or relevant literature should be added otherwise.

Line 96-108: concepts regarding processing of the DEM and analysis on tributaries are described together and this make the sentences unclear. Moreover, these are contents dealing with the methods section, not the "Data collection" section.

Lines 112, 121: these paragraphs deal with methods, not with data collection.

Line 119-120: these symbols are reported in Fig. 3. A link to this figure should be added.

Line 122-128: panchromatic, multispectral or pan-sharpened imagery (see also Line 132)? What about the accuracy of spatial co-registration to topographic maps? May Authors explain whether the equal area projection issue is relevant in respect to the dimensions of the study area? Which is the objective of atmospheric and radiometric correction? This description is too qualitative, and it should be improved.

Line 129: what is the meaning of "processing"? Do Authors refer to image pre-processing?

Line 143: how the accuracy was evaluated? What about the false/true positives/negatives matrix?

3 Evolution division of JJG

Line 147: this section includes both results and discussion contents, so these should be split and rearranged. Moreover, the meaning of the title is unclear. Could Authors state what is an "evolution division"? Please check also the English meaning of this term.

Line 156: these results should be highlighted within a table.

Fig. 3: the caption should clearly state the meaning of axes label and numbers within the right diagram.

Line147 and 157: same title for both sections.

Line 171-176: results and discussion are mixed, so the section should be rearranged as a whole.

Line 176: where do Authors quantify the distribution of debris flow frequency in the different tributaries?

Line 180-183: this refer to methods, so it should be introduced in the former part of the manuscript. Moreover, the parameter “Aip” is not described, and relevant literature is lacking.

Line 184-186: Authors have to highlight whether this result is already known and expected from the literature (to be added) or it’s an outcome of their work.

Line 189: unclear/incomplete figure caption.

Line 207: “evolutionary periods”. What is the meaning of this term? Are Authors dealing with time? Please clarify.

Line 207-212: this is a visual qualitative analysis between EI and landslide distribution, which corresponds to the title of the next section. Why this paragraph is located here?

Line 214: unclear figure caption.

4 Landslides distribution in relation to EI

Line 218: this is a result of the work, but it is presented after discussion of other results.

Line 223: what is the meaning of the term “vulnerable”?

Line 232: the sum of % in column 2 is greater than 100. Why? Isn’t the total area equal to the sum of values in column 1? Please check.

Line 233: unclear title.

Line 242-243 and Figures 11, 12: Authors do not consider the role of other factors (like lithology and structural framework, hillslope aspect, land cover, . . .) which may influence the landslides frequency – extension relationships within different watersheds characterized by similar EI values. May Authors exclude these further factors are in some measure the reason of “fluctuation” of the EI-Lap results? Why? Is this fluctuation a random distribution, instead?

Line 243-244: “smaller than other evolution stages in active evolution stage”. Unclear

sentence.

Line 263-265: Authors talk about historical landslides, but in the previous sections no information were provided about the state of activity of landslides.

5 Discussion

Line 269-270: this is most probably questionable. See for example, https://doi.org/10.1016/j.enggeo.2011.03.016, https://doi.org/10.1016/j.ancene.2014.08.001, DOI: 10.15551/prgs.2017.28.

Line 280-281: Authors do not provide any information about the friction angle of soils, so how this parameter may be related to slope distribution in the study area? Moreover, what about the role of cohesion (especially for landslides involving bedrock) and pore pressure? These concepts should be analysed and discussed. The sentence should be removed otherwise.

Line 285-290: see comment Line 242-243. May Authors demonstrate that the observations here reported depend on EI only?

Line 294-295: channel density is introduced here in the discussion section. This parameter should be introduced in the methods sections along with the description of the procedure implemented for calculation and the corresponding literature.

Line 307-308: please add references.

Line 312: more information should be added to the caption. If this information is taken from the literature, add references.

Line 314-317: this is a very generic sentence and more detailed information should be given in order to support relationships between the results of this work and the data known from the literature. Again, any different condition/behaviour (in this case variation of debris flows composition) is assumed to depend on the “spatial heterogeneity of evolution”. Instead, other factors (i.e.: geological, morphological, rainfall distribution,

. . .) could also be relevant.

Line 342-345: unclear sentence.

Line 352: please add references.

Conclusions

Line 358-359: unclear sentence. Are Author stating that intermittency of debris flows is related to spatial heterogeneity of landslides distribution? Does it mean that the temporal behaviour of the former is related with the spatial behaviour of the latter? Moreover, either new data or data from the literature are not analysed within the previous sections of the manuscript, so why the above “relation” is reported within the conclusions? Please check and explain.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-131, 2020.