

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/nhess-2022-43-RC1>, 2022
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Review for paper NHESS-2022-43

Anonymous Referee #1

Referee comment on "Hazard Assessment of Earthquake-Induced Landslides Based on a Mechanical Slope Unit Extraction Method, A Case in Ghana" by Peter Antwi Buah et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-43-RC1>, 2022

Review for paper NHESS-2022-43: "Hazard Assessment of Earthquake-Induced Landslides Based on a Mechanical Slope Unit Extraction Method, A Case in Ghana" by Antwi Buah et alii

Dear Authors,

I have evaluated the manuscript "Hazard Assessment of Earthquake-Induced Landslides Based on a Mechanical Slope Unit Extraction Method, A Case in Ghana".

The authors analyse as a case study the whole territory of Ghana proposing a consolidated workflow that goes through a definition of safety factors for each analysis unit from which a coseismic displacement is computed by means of the Newmark rigid block method through the quantification critical acceleration K . The authors systematically analyse the critical aspect in the definitions of coseismic displacement, introducing factors that account for non-infinite slope failure or non-rigid block displacement. Nevertheless, several critical aspects were found. Given the title of the work, I would have expected a detailed analysis of the proposed slope unit extraction method, also in relation to the other methods listed in the manuscript. The authors stated that "Ghana (West Africa) is selected to test the proposed slope unit extraction method", however, most of the manuscript deals with an overview of coseismic displacement analysis methods. The mechanical slope unit extraction method is poorly explained, and the strength and weaknesses of the method are not explored. I missed what the attribute "mechanical" is referred to in the proposed method, and which of the considered mechanical parameters are included in the slope unit's definition.

In addition, in the summarised approach, it is not clear if slope units or grid cells were adopted as analysis units. In my opinion, this study seems to have been conducted on grid cell analysis units and not on SU. In the workflow in Fig. 2 the analysis adopted the slope angle H and α and β angles of Fig.1,

however, the images reported in Fig. 7 and the following variables (e.g., slope angle) seem to contradict it. According to the Flowchart reported in Fig. 2, the proposed framework aims to provide a "Displacement prediction for slope units.", however, what is understood from the method is that the slope units are used only as an area in which calculating the Prediction rate rather than a unit of analysis for evaluating the coseismic displacement. If so, how the improvement of SU delineation implemented in the proposed approach can improve the prediction rate should be better discussed. Are the different slope unit extraction methods more important than the adopted SU area? A quantitative analysis of the differences introduced by the three approaches is missing, and there is a limited quantitative analysis except for the assessment of prediction rates. Only a general comment on the number and extent of the resulting slope units (lines 306-312) is reported. The parameters considered for the calculation of the slope units are not clear (e.g., threshold of the accumulation surface of the initial flow, minimum and maximum surface for the slope units). Noticeably the manuscript poorly discussed the validation of the computed displacement with respect to the landslide catalogues. What about the existing or available inventories? I suppose that "scars" reported in the prediction rate table refers to some landslide catalogue? Are the ones reported in Fig. 10 from an event-based inventory or generic landslides catalogue? The authors report a generic comparison with the susceptibility map of Ghana which seems to express no more than the spatial relationship between steep slopes and landslides. If these catalogues are available, are the prediction rate and failure rates compared to seismically induced landslides? Regarding the adopted seismic input, how the seismic loading in terms of PGA was selected to 0.13g? Is it representative of Ghana's seismicity? Are they referred to as a seismic hazard map? Is it comparable to some of the earthquakes available in the record? Is the seismic input assumed in the analysis the same in the whole Ghana territory? The authors state that Eq. (20) is "used to determine the extent of earthquake vibration that can trigger slope displacement in Ghana": Does the analysis consider a specific seismic event? Please, clarify. Map of seismic distribution of historical earthquakes, as well as the inventory of ground failure, would help the reader understand. The manuscript style is not adequately organized in styles and formatting. Figure and not sequentially cited in the text and often placed in the wrong chapters. Many of them are not useful or simplistic (e.g., Fig. 4 or 5). Reference to figures and tables in the text are not consecutive, captions not informative and not self-standing. Tables 2, 3 and 5 can be combined or eliminated. Some of the reported equations are pleonastic

(e.g., Eq. 1, Eq. 7, Eq. 19) and can be substituted by reference. Despite the adopted techniques being appropriate, the knowledge gap is not clarified or mentioned, and the expected scientific progress is not clarified by analyses and conclusions. I do not understand the improvement posed by the proposed SU method for the definition of scenarios or analysis of seismic induction. Regardless of the specific interest in the case study here analyzed, the work conducts a systematic analysis of the sensitivity of one or more models and of different mechanical parameters, which has already been the subject of numerous studies in the literature, therefore, this does not add relevant scientific elements for the engineeringâgeological community.

I think the subject of the article is within the scope of the journal, but the main objective stated in the abstract and introduction was not deeply addressed in the manuscript. Major limitations regarding the SU delineation and the consequent coseismic displacement analysis were found. The mechanical slope unit extraction method is poorly detailed, as well is not clear the use of SU for displacement analysis.

Given these aspects, the paper would benefit from a deep systematic revision of the presentation of results and proposed approach with respect to the claimed aim before consideration for publication in Natural Hazard and Earth System Science. Please note the specific comments included. I hope that you will find the comments to be of use to you.

Specific Comments

1) The Introduction chapter is unbalanced on the literature empirical relations available for the calculation of the coseismic displacement and of the different solutions for landslides stability analysis, with respect to slope units' delineation approaches.

2) The authors stated from line 80 that the main aspect of the model used and proposed has three distinct features compared to the others and include: i) the SU delineation, ii) the consideration of poreâwater distribution and iii) the GIS computation of F_s and the K_y to avoid iterative errors:

The first important aspect is the SU definition approach that should solve slope heterogeneity defects is not adequately presented and is not clear and easy to understand by the general audience. Secondarily, the role

and the areal constraints of pore pressure (hydrostatic?) lacks in the whole manuscript. Is it considered parametrically? Does the SF of Fig. 7 account for the r_u pore pressure ratio reported in the methods

section? Finally, the areal GISâbased quantification of SF and K_y appears to be a commonly used approach in the scientific community.

3) The rigid and flexible block effect is considered important and treated analytically, however, the contribution of cohesion in earth shallow landslides is considered irrelevant, which are mainly governed by the effect of apparent and mechanical cohesion of unsaturated media. A critical comment

about this topic

would improve the manuscript. In addition, eq. (8) by Saygili & Rathje, 2009 is dependent on cohesion.

Which values have been adopted?

4) Are the landslides plotted in Figures seismically induced? In some figures are reported locations of

"failure areas and catalogue" that are not explained and/or not fully considered in the validation of results.

5) Regarding the Prediction rate I'd spend more effort in discussing the general concept and the meaning of

the two curves for the validation of the results. With a few landslides' observations, I supposed the Pr is

overbalanced by a large number of negatives. I'll express the success rate as the ratio between true positive

rate and false-positive rate. Would the success rate for true positive be more informative? What about the

effect of strength parameters on the lone True Positives (S1)?

6) Concepts expressed in methods are often repeated in the results section. Technical language is often less

precise (see Detailed comments). The introduction of the chapter "Seismic activity of Ghana" is mainly

focused on African landslides, however, a detailed analysis of the available landslide catalogue lacks. I would

suggest changing the title or rephrasing the introduction to the chapter.

7) The manuscript is not adequately cared for in styles and formatting. Figure and not sequentially cited in the text

and often placed in the wrong sections. Many of them are not useful or simplistic.

Reference to figures and

table are not consecutive, captions not informative and not self-standing. Please consider a detailed review

according to Journal standards.

Detailed Comments

Line 20: Please change the typos sentence that is supposed to be "in order to"

Line 36: Newmark instead of Newark

Line 60: Please deeply explain the limitation in reflecting morphological features

Line 101: Do the authors referred to river thalweg with the term "crevasses"?

Line 116: It is not tensile. It corresponds to the shear stress component parallel to the failure surface

Line 121: "m" should be formatted in Italic

Line 139: References are cited twice

Line 143: Please rephrase the definition of $K_y(g)$

Line 190: The list of coordinates can be omitted in the text since are included in the figures.

Lines 192-194: Please use the International System of Units

Line 148: Reference of DCF is missing. The factor is reported sometimes in subscript, please uniform it.

Line 205: The Romanche Transform fault and earthquake epicentres should be located on Map.

Line 217: Reference to figure missing

Line 227: Unit weight cannot be considered a strength parameter

Line 258: Annum

Line 323: "Ninety-nine per cent correlation is obtained". I missed the presentation on this correlation analysis.

Line 337: T_s/T_m areal or lithological distribution is not discussed.

Table 2: In the percentage column 1/3 has been changed to 3/Jan by the corrector

Comment on References and Figures

• Most of the figures are not informative (Es. Fig. 4 or Fig. 5) and can be deleted.

• Fig. 7a the red-green colour bar looks counterintuitive to express the SF of slopes.

• Fig. 14 caption is wrong, looks like a repetition of fig 13.

• Table 2 can be merged with Table 3 since it is not informative. Areal percentage distribution of the two

major complexes is not indicative. I'd report it for the lithological unit in Table 3.

• Fig. 5 can be merged with fig. 6.

• Fig. 6 can be improved including the relief map reported in Appendix Fig.A1.

• Fig. 9 It is difficult to appreciate the difference between the three adopted models.

• Fig. 12 Change "Hydrological"

• Bu et alii 2019 is not listed in the reference