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Natural Hazards and Earth System Sciences
Discussions

# *Interactive comment on* "Predictive analysis of landslide susceptibility in the Kao-Ping watershed, Taiwan under climate change conditions" *by* K. J. Shou et al.

**Anonymous Referee #2**

Received and published: 25 February 2015

General Comments: This paper applies future rainfall estimates from TCCIP (Taiwan Climate Change Projection and Information Platform Project) in a landslide susceptibility model to assess the landslide potential in near future (2015) and far future (2075-2099) for Kaoping Watershed in southern Taiwan. The topic is apparently related to the interest of NHESS. However I suggest the body of the manuscript is poorly prepared, written and organized, and I suggest the paper does not meet the standard of NHESS. First, the use of English language has many logical problems and grammatical errors. For example, the wrong figure placed in Figure 11, and Figure 16 mentioned in the text does not exist in the submitted paper. Second, the literature review is obviously in



a sallow level. Several published studies have been focused on the same region and event, and can be easily found in many journal article resources, including Chang et al., (2014) who have revealed the relationships between several historical typhoon rainfalls and landslide susceptibility in Koping area, and Mondini and Chang (2014) who have discussed the automatic mapping issue for landslides triggered by Typhoon Morakot (2009) in Koping Watershed. All these articles are directly relevant to this study and worthy of discussion in a deep level. Additionally the motivation of this study is because of the lack of consideration of the future climate effect on landsliding in Taiwan. However, Chiang and Chang (2011) have explored some problematic issues regarding the application of AGCM (Atmospheric General Circulation Model) to the future projection of landsliding scenarios in the mountainous area in Taiwan. Their work is definitely comparable to this paper but didn't get any credits. Third, the methods (section 3) used in the study are not clearly explained, making difficulties for readers to follow. For example, how the authors to link the rainfall frequency analysis to the susceptibility model. Finally, the design of experiment is questionable. Why the authors choose the two models (instability index and logistic regression) instead of applying physically based models or empirical models? And what is the basic assumption behind this study? I personally assume that the statistical properties of the extreme rainfall and the eight adopted variables/control factors will remain the same in the future. However, no further statements or discussions are addressed. Therefore, I doubt outcomes conducted by the study are fully supported. I suggest returning the paper to the authors for rejection.

Specific Comments:

Does the paper address relevant scientific and/or technical questions within the scope of NHESS?

Yes, the title is informative, but the body of the manuscript may not meet the standard of NHESS.

Does the paper present new data and/or novel concepts, ideas, tools, methods or results?

Fair. This study introduces the TCCIP rainfall estimates, based IPCC-AR5-A1B to the landslide study. However, the reliability of the use of TCCIP is questionable. According to Chiang and Chang (2011), it's known that the typhoon rainfall is the major trigger of landsliding in Taiwan, but the AGCMs do not incorporate typhoon in the simulation, which potentially cause incorrect estimation of rainfall patterns in time and spatial domain. I don't know if TCCIP cover this issue in the downscaling and correction scheme, but the authors can easily examine TCCIP data by comparing with historical records from rain stations, since TCCIP provides rainfall estimates from 1979 to 2003 (P583 line 23).

Are these up to international standards?

No.

Are the scientific methods and assumptions valid and outlined clearly?

No. The basic assumption for conducting the rainfall frequency analysis and landslide susceptibility is not mentioned. P576 Line 17-18: "Taiwan has been significantly affected by the concentrated rainfall periods and high rainfall intensities." The authors should provide the background in more detail and address citations that can be investigated by readers. The next sentence P576 Line 18-19: "The frequency of extreme event is increasing. . ."âĂŤthis statement is so critical to the study. As above, the authors should provide evidence/references to support this issue. P578 Line 8 and Table 1: "Slope>40%". The study simply excludes landslides over relatively gentle slopes, which introduces bias of landslide training samples, and this probably causes the (relative) low accuracies of Typhoon Morakot. And, why not just use the landslide inventory provided by the Central Geology Survey of Taiwan to develop the landslide susceptibility model?



Are the results sufficient to support the interpretations and the conclusions?

Not clear. P577 Line 7: "The data of these two typhoons were used to train the susceptibility model", which means no other event(s) is used for validating the landslide susceptibility model. This significantly reduces the reliability of conducted results. P585 Eq. (6)-(9): The four derived equations are important and worthy of a deep discussion, including their physical meanings, similarities and differences. P586 Line 1: Fig. 11 is incorrect. P585 Eq. (6)-(9) & P586 Line 6: The results show that the two applied models are not consistent when a different typhoon rainfall is employed. It's questionable that if the model is applicable to the future rainfall intensity that is expected to become more severe than present, when the stronger event, Typhoon Morakot, obtained a relative low accuracy. P586 Line 22: Fig. 16 and Fig. 17 cannot be found in the paper, and I guess they are shown in Fig. 15. P586 Line 22-23: "The results in Figs. 16 and 17 suggest that the landslide susceptibility is higher in the far future (2075-2099) than in the near future (2015-2039)." By viewing the two maps my feeling is just opposite. It's suggested to list some statistics such as the area of area proportion prone to landsliding for the two periods.

Does the author reach substantial conclusions?

No.

Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)?

Not clear. This study applies two methods: Instability Index and logistic regression. What's the purpose of the trail? P577 Line: 1-2: ". . .the impact of the climatic abnormalities is seldom considered in the landslide analysis, which motivates this study." In fact, few studies have been focused on the application of AGCM rainfall estimates to assess the future landslide scenarios. One early example is the "The temporal stability and activity of landslides in Europe with respect to climatic change," or TESLEC (Dikau

and Schrott, 1999), and for Taiwan, Chiang and Chang (2011) have conducted this issue in Taiwan (as mentioned before). The motivation of the study becomes weak when they don't do their review job appropriately. P579 Line 10-11: "The dip slope index (Ids) is defined as the angle difference between the dip direction of weak planes (bedding and joints) and the dip direction of slope. . ." The authors should explain how they measure/estimate the "dip direction of weak planes (bedding and joints)" for the entire study area. P579 Line: 17-19: . . ."they are highly correlated. . ." and ". . .these two independent variables. . ." The two descriptions are obviously conflictive. P582 Line 10, 14 and Fig. 1: The study applies Kriging to estimate the rainfall distributions, based on 7 rainfall stations shown in Fig. 1. However, no stations in the west side and only two stations are outside the study area, indicating that the rainfall maps shown in the paper are mainly spatially extrapolated instead of interpolated (P583 Line 2). So the estimation probably introduces considerable uncertainties, especially in the western part of the study area. The authors should reveal the estimation error of these rainfall maps.

Does the title clearly and unambiguously reflect the contents of the paper?

Yes.

Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained?

Fair. P576 Line 1: "also" reads redundantly in the very first sentence. P576 Line 8: "Krosa Typhoon" and "Morakot Typhoon" should be "Typhoon Krosa" and "Typhoon Morakot".

Are the title and the abstract pertinent, and easy to understand to a wide and diversified audience?

Fair.

Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or



appendixes listing them?

Not really. P578 Line: 9: "NDVI"; remove italic from "I". P580 Line 11: what's the definition of landslide density Gi? P580 Eq. (2): X should be replace by S. P581 Line 1 and Eq. (4): Is landslide susceptibility index P or Ip? And different representations of Ip are shown in Eq. (4) and Eq. (6) & (7) in P585. P585 Eq. (8) & Eq. (9): F3 is the aspect. Not sure how the authors conduct this variable in the analysis, because the unit is degree ranging from 0-360 (-1 to be flat). For instance, 0 (minimum value) and 360 (maximum value) represent the same physical meaningâĂŤdirection to north, so it's not linearly referenced. Researchers normally use dummy/categorical variable for aspect, otherwise taking sine and cosine to be two independent variables.

Is the size, quality and readability of each figure adequate to the type and quantity of data presented?

Not really. Figure 1: Please denote which river is the Koping River, because the watershed is different from what I have invested from web resources. Please also list the lithology for Pilushan Formation and Chaochou Formation. Figure 2: Are those landslide dots from a single event or the two events, and how to apply this figure to future projection? Figure 3-5: "hr" in figure title should be same as the "h" in the caption. The scale range in each figure should be comparable. Figure 6 and 7: please use "cumulative" instead of "accumulative" for the contour legend. Also please modify the range legend of rainfall to be the same. Figure 8: (b) should be elevation and (c) aspect. Now is incorrect. Figure 9-10: please modify the two range legends to be comparable. Correct "Krosa Typhoon" and "Morakot Typhoon" to be "Typhoon Krosa" and "Typhoon Morakot" in figure caption. Figure 11: this is an incorrect figure here. Figure 12: please correct the figure caption. Figure 13-14: same problems with Figure 3-5. And please modify the "Recurrent Period" to be the same as "return period" in the figure caption. Figure 15: fix the probability legends.

Does the author give proper credit to previous and/or related work, and does he/she

indicate clearly his/her own contribution?

No, as mentioned in the general comment.

Are the number and quality of the references appropriate?

Yes.

Are the references accessible by fellow scientists?

Fair. Some scientific reports and master theses (in Chinese) are not accessible.

Is the overall presentation well structured, clear and easy to understand by a wide and general audience?

No.

Is the length of the paper adequate, too long or too short?

Fair.

Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated?

Yes. Introduction: this part is very weak, not clearly defining the scope, the scientific context and the basic assumption of the study. Section 2: “Basics of the study area: should be changed to “Study area”. For landslide mapping, the study employs NDVI from SPOT imagery. However, the acquired dates (before and after the events?), image quality and image preprocessing are not mentioned. Section 3âĂŤthe methodsâĂŤneed to be clarified with better use of English language. Section 4: the link between rainfall frequency analysis and TCCIP rainfall projection is not clear. And which probability model applied to the frequency analysis is unknown. Section 5âĂŤResults: this part is informative when the authors provide limited explanation and interpretation of the outcomes. Section 6 Conclusions and suggestions: this part is more like



discussion and poorly structured. Please consider combine Figure 6 with Figure 7.

Is the technical language precise and understandable by fellow scientists?

“accumulative rainfall” should be corrected as “cumulative rainfall”.

Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience?

No.

Is the amount and quality of supplementary material (if any) appropriate?

Not applicable.

Reference:

Dikau, R., Schrott, L., 1999. The temporal stability and activity of landslides in Europe with respect to climate change (TESLEC): main objectives and results. Geomorphology 30, pp. 1–12.

Chiang, S. H., Chang, K. T, 2011. The potential impact of climate change on typhoon-triggered landslides in Taiwan, 2010–2099. Geomorphology 133, pp. 143–151.

Mondini, A. C., Chang, K. T., 2014. Combining spectral and geoenvironmental information for probabilistic event landslide mapping. Geomorphology 213, pp. 183–189.

Chang, K. T., Chiang, S. H., Chen, Y. C., Mondini, A. C., 2014. Modeling the spatial occurrence of shallow landslides triggered by typhoons. Geomorphology 208, pp. 137–148.

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