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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Field-based landslide susceptibility assessment in a data-scarce environment: the populated areas of the Rwenzori Mountains" *by* Liesbet Jacobs et al.

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Dear members of the editorial board,

Dear referees,

We thank you for providing us with the evaluation on our manuscript. With this reply we hope to provide adequate answers to the comments of the reviewers. This is done in a point-by-point fashion below. First the comment of the referee (RC) is given first, after which our response (AC) is given in normal font. We strongly appreciate the insight and feedback provided by both referees and are convinced these have helped

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in further improving the manuscript. All changes made are indicated through a page and line number of a revised version of the manuscript that can be consulted in the supplement.

Response to the comments of the Referee 1: Dr. Marta-Cristina Jurchescu:

(1) General evaluation by referee

RC: The paper brings about an interesting discussion on a subject that landslide literature could consider as being exhausted, namely on the landslide susceptibility assessment. The study successfully responds to two major objectives, as clearly stated in the Introduction section: i) providing new and important information for understanding regional-scale landslide susceptibility patterns and its conditioning factors in a landslide-threatened area of equatorial Africa (Rwenzori Mountains' Footslopes, Uganda), characterized by very limited data and hence necessitating extensive field investigations, and ii) achieving solid statistical modeling results by thoroughly discussing several methodological choices (scale, topographic data source, spatial resolution, selecting landslide type, landslide sampling, variable selection, deciding on calibration and validation data) and grounding these on previous theoretical knowledge or on running simulations and exploring their effects. The authors propose to address landslide susceptibility in the context of a data-scarce environment by: i) constructing a regional scale model in two phases - calibration and validation using landslide inventories in representative case-studies, followed by the extrapolation to the rest of the area -, ii) deriving separate local-scale maps for the individual case-studies and iii) using the regional susceptibility map in combination with population density for a preliminary identification of landslide risk hotspots. Given the in-depth statistical analyses, the obtained maps can be considered valuable tools for directing and implementing policy actions in the region. Results are based on several model variants (varying according to the incorporated topographic data of different sources and spatial resolutions) being run for each scale (local / regional) and subsequently evaluated. Evaluations of obtained models (e.g. in terms of performance or variables' selection) as well as the identification of

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the optimal ones are thoroughly conducted, being based upon statistical significance tests. Additionally, differences induced by random landslide sampling are taken into account by running 20 simulations per variant. The manuscript is well structured and written and illustrations and tables are all necessary. In my opinion, the paper is worth publishing with only minor revisions.

AC: The authors appreciate the positive and in depth evaluation of the manuscript's content by referee #1 both with regard to the thematic content and the methodologies applied.

(2) Specific comments by the referee

1. RC: One of the qualities of the paper consists in testing the effects of several methodological choices on model performances and, especially, in the thorough comparison of these model performances in order to allow for the selection of the appropriate model variants for each level. It is highlighted that tests used to assess statistical significance of differences in performance (in terms of AUC values) highly depend on the assumptions of normality and homogeneity of variance (homoscedasticity) of the examined values. Several steps are therefore involved: i) distributions are tested for normality, ii) homoscedasticity is tested, by also using tests which are robust to large departures from normality (Fligner-Killeen test); iii) only then significance tests are run for assessing performance differences, varying in type according to their suitability to the different categories issued.

AC: The referee supports our evaluation of normality and homoscedasticity prior to implementing significance tests for the variants' performance evaluation.

2. RC: An important insight is brought with respect to the scale issue, a fundamental concept in many sciences, through comparisons among regional and local models. Specifically, it is shown, not only that the regional model, when applied (validated) at the local scale, is generally outperformed by the local models, but also that it lowers its own performance when interpreted in the context of the larger scale (from 0.71 to

0.65-0.70, with one exception, according to Fig. 4). I suggest testing if this lowering from regional to local level is statistically significant and recommend that these aspects be also mentioned in the respective paragraphs (section 3.3. page 9 lines 8-13 and section 4.1. page 11 lines 10-21). This would contribute to reminding us of the scale-dependency of model outcomes, i.e. that model results derived at one scale should not be transferred at another scale, but considered valid and interpreted in the specific context of the scale they were obtained at.

AC: The referee suggests to further improve the discussion related to the applied scale by providing additional testing of the performance decrease of regional models tested on the local scale. We therefore now compare the regional models performance when applied to the individual local scales to the regional model performance when validated on the regional level. On two out of four local levels this decrease in performance is significant indicating that models calibrated and validated at a certain scale cannot readily be interpreted at a larger scale. This is now added to the manuscript in the methodology, results and discussion section: on pag.9 lines 12-13, pag. 11 lines 19-24, pag. 14 lines 15-17.

3. RC: I notice the description of data and methodology used is particularly accurate to allow reproduction. This is enhanced by the appendix describing the processing of TanDEM-X images to construct the respective DEMs as well as by the explicit Data Availability expression at the end of the paper.

AC: We are happy to learn that the reviewer appreciates the description of the TanDEM-X DEM production in the appendix.

4. RC: In my opinion, other previous similar methodological questions on the impacts of varying data (of different sources, resolutions and accuracies: e.g. raised by Lee et al. 2004, Catani et al. 2013, Fressard et al. 2014) or of various modeling settings (e.g. by Poli & Sterlacchini 2007, Yilmaz 2010, Hussin et al. 2016 for landslide sampling strategies; by Heckmann et al. 2014, Petschko et al. 2014 for subdivision in calibration

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and validation datasets) on model performances could complement the ones already mentioned in the Methodology section (e.g. section 3, page 4 lines 15-17; section 3.1. page 4 line 32 – page 5 line 1, sections 3.2.1, 3.2.4.). Recommended would also be the use of "e.g.", since citations here cannot be exhaustive.

AC: We have complemented the references in the manuscript by suggestions made by the referee in order to strengthen the methodological section. We have added references in section 3, e.g. pag. 4 lines 15-17, pag 5 line 4-5. In order to keep the text concise we have, as suggested by the referee, used 'e.g.' wherever other references could be added and we were not exhaustive.

5. RC: Please also refer to motivations brought by previous papers in making methodological decisions. For example, in section 3.2.1. Landslide sampling, page 6 lines 12-13, I suggest citing Van Den Eeckhaut et al. (2006) since these authors give the same motivation for selecting only the central cells of landslide depletion zones (i.e. avoiding spatial autocorrelation and the violation of the assumption of data statistic independence).

AC: This reference is now taken into account: pag. 6 line 13.

6. RC: If and where possible, please also briefly complement the references to outcomes of similar experiments conducted on the effect of using different data sources and spatial resolutions (e.g. Catani et al. 2013, Fressard et al. 2014) when discussing the results achieved in the current paper (section 5.1).

AC: This is taken into account: pag. 12 line 29.

7. RC: Section 3.2.1., page 6 line 6-7: I would not agree that the sentence listing landslide sampling techniques also includes: "or construct a buffer zone around (portions of) the landslide to represent the conditions under which the landslide occurred (Dai and Lee, 2003; Suzen and Doyuran, 2004; Van den Eeckhaut et al., 2006; Che et al., 2012; Hussin et al., 2016)". To my knowledge, buffer zones around landslides are

drawn in order to eliminate those areas from sampling absences and not presences of landslides. Please check this and reword the phrase accordingly.

AC: A buffer area can indeed be used either to exclude those areas from the sampling of non-landslide points, but are likewise used to indicate areas of undisturbed but similar conditions under which the landslide occurs. The latter is often referred to as the 'seed cell' method (Suzen and Doyuran, 2004; Che et al., 2012). This clarification is made in the revised version of the manuscript: pag. 6 line 6.

8. RC: Figure 2 nicely presents the various methodological aspects considered in the research as well as the manner in which they were evaluated. However, it is not clear what does the "(Pre)Selection of Dependent Variables" refer to. Should there not be "(Pre)selection of Independent Variables" i.e. of explanatory variables (since the discussion on the selection of the dependent variable, i.e. landslide occurrences, is made under "Landslide sampling")?

AC: The referee is correct in pointing out this mistake: Fig. 2 is corrected

9. RC: I think the title of figure 4 should also include the explanation that small circles represent the mapped landslides.

AC: This suggestion is now included in the revised version of the manuscript: figure caption of Fig. 4

10. RC: I would suggest modifying the title of section 4.2. from "Separation of landslide types" into "Separation of landslide types and controlling variables", since this section presents results both in terms of the effect of distinguishing landslide categories and in terms of the significant explanatory variables changing according to the landslide type. This would also enable a more obvious linkage between the Results and the Discussions sections.

AC: It indeed will improve the clarity of the results section if the title of section 4.2 is modified. Similar to the first title of the result section 'Influence of model's spatial

resolution, topographic data source and scale', we modified the consecutive title from 'Separation of landslide types' to 'Influence of the separation of landslide types' as to present the results in a straightforward, uniform manner: pag. 11 line 27

(3) Technical comments by the referee AC: The technical corrections suggested by the referee are taken into account in the new version of the manuscript (in track changes).

Other changes to the manuscript: In the previous version of the manuscript we used a SRTM and ASTER DEM that resulted from the resampling to precisely 30*30m^2 resolution based on a warped product in UTM coordinates that carried the original resolution (ca 30.7*30.8 m^2 resolution at the equator). We have now improved this by combining the warping from the WGS product to UTM coordinates and resampling to precisely 30*30m^2 resolution in one step. This in principle entails less manipulation of the original data but de facto results in only minor changes in the final products used. With regard to the manuscript, this adjustment as a result does not imply significant changes to the findings with the exception of a slightly more nuanced interpretation of the ASTER variants' performances, which are now significantly outperformed by InSAR alternatives on 3 levels (compared to 4 levels in the previous version of the manuscript). For clarity, track changes have also been applied to these changes. Tables 2, 3 and 4 were updated as well as figures 3, 4 and 5. With regard of the slope threshold we use to correct our regional susceptibility model, we have decided to deploy a slightly more conservative cut-off value of 3° slope gradient in lieu of 5°. This change is reflected in minor adjustments to Figure 5 b and d. Finally, we've made a few clarifications in the manuscript, all of which are indicated in track changes in the document here in supplement.

Sincerely,

Liesbet Jacobs

On behalf of all the co-authors

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Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-259/nhess-2017-259-AC1-supplement.pdf

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