Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2017-259-AC2, 2017

Natural Hazards and Earth System Sciences Discussions
Open Access

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.

Liesbet Jacobs et al.

liesbet.jacobs@vub.ac.be

Received and published: 16 November 2017

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 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

Printer-friendly version

Discussion paper

supplement.

(1) General evaluation by referee

RC: General comments: Paper summarized objectives, methodology and results are already well commented by the first reviewer, therefore I will skip this part in my comments. Likewise, the manuscript is well structured and written, and figures and tables are quite illustrative. The manuscript is considered valuable for publication with minor revision.

AC: We are happy to learn that Referee #2 agrees with the positive evaluation by referee #1.

(2) Specific comments by the referee

1. RC: 1. In the paper title, the term "Fieldbased.. ." does not appear to properly reflect the contents of the paper. In addition field investigations are mainly dealt with in previous work by the main author. Therefore I suggest to change this somehow misleading title.

AC: It is correct that the manuscript contains limited information on the data collection itself as this is described in other publications (indicated in lines 20-21 pag. 3). However, we were asked in the initial submission stage to redirect the focus of the manuscript more towards the site application in a data-scarce environment. This includes a large component of field work, hence the choice for this title. The susceptibility analysis is in this sense field-based: the landslide data driving the analysis was collected mostly through field surveys.

2. RC: Page 2, lines 12-13: Do you mean reliable landslide susceptibility "models" or rather "data" or "assessments"?

AC: Indeed we mean "reliable susceptibility assessments": this is corrected in the revised manuscript: pag. 2 line 13.

3. RC: Page 5, line 10: A reference should be provided for TanDEM-X DEM as done for the other DEMs

AC: The reference ‘Deo et al., 2013’ is now included here: pag. 5 line 14.

4. RC: Page 5, lines 6-18: Although four DEM resolutions are said to be used, the last sentence appears to indicate six resolutions. Please clarify. Also, adding a table would improve the readability of this part.

AC: Indeed four different DEM resolutions are tested, but at 30 m resolution, three different DEMs are compared as described in that paragraph. This results in a total of six unique combinations of DEM type and resolution (pag. 5 lines 23-24). These combinations are described in lines 20-21 and thus an additional table seems of limited added value.

5. RC: Page 9, lines 20-21: Even though the focus of this part in placed on risk hotspots not on risk assessment, the census data used (from 2002) appears quite outdated considering that a more recent census of 2014 provisionally reports an outstanding 43% increase of total population in the country in that period.

AC: Indeed, the 2014 census data would be a better reflection of the current population density, however, the data on parish level has not been made available. Therefore, at this scale we only have the 2002 census data at our disposal.

6. RC: Page 12, line 27 – Page. 13, line 2: Can the findings from DEMs quality comparison be extrapolated to non-tropical regions, i.e. to areas where more cloud-free ASTER imagery can be acquired and thus better ASTER-based DEM model can be constructed (e.g. to Europe)? Please explain.

AC: In this manuscript, the suitability of different DEMs was evaluated for one region only. Therefore, a blind extrapolation of these findings to other regions should be avoided as DEM quality, regardless of the type of DEM, is inherently dependent on the quality of the acquired data used to construct these DEMs which can differ from region to region. However, similar findings through direct comparison of e.g. the SRTM DEM and ASTER DEM are available in the literature (e.g. Guth, 2010 , Li et al., 2013). These references are now added to the manuscript: pag 13, lines 3-4.

7. RC: Page 15, line 24 (and page 9, line 23): Within-parish distribution of population densities could be somehow determined through mapping built-up areas using optical satellite imagery, thus providing a better identification of landslide risk hotspots when combined with landslide susceptibility levels. Please consider this in the revision.

AC: Population estimates based on optical satellite data in the tropics, although being a promising technique, is rare, especially at regional scales for rural areas. This requires very high resolution satellite data. Automatic mapping is complex in densely vegetated area with scattered houses. The perspective for a more detailed and reliable population density estimation is now included in the discussion: pag 15 lines 32-34.

8. RC: Page 18, lines 8-9: It would be helpful to explain possible DEM mosaicking-derived issues (e.g. boundary effects) and the solutions implemented if any.

AC: Indeed, seaming issues can arise when combining several images. However, mosaicking was performed while avoiding seams in the study areas used for the model calibration and validation. This is now specified in the aforementioned section: pag. 18 lines 22-23.

9. RC: Page 26: In Figure 3 caption it would be helpful to refer to table 2.

AC: This figure caption is now adjusted accordingly

10. RC: Page 30: Table 3: Please clarify why there is no lithological variable called gneiss, despite it is reported in the text to be the reference lithology in the region. Additionally, do you mean by “quarts” “quartzites”? Please clarify or correct.

AC: Gneiss is not mentioned in table 3 and 4 because it is the reference lithology: i.e. this lithology is coded by a 0-value combination of all other lithological dummy variables relevant to the investigated study area. If in one study area, n lithological classes occur, among which gneiss, these n lithological classes will be coded by n-1 variables indicating the presence/absence of the n-1 lithological classes that are not gneiss. The presence of gneiss is then coded by a 0-value for all the n-1 other lithological classes. By including all n variables indicating the binary presence/absence of all lithological classes, including gneiss, a 'dummy trap' would occur, i.e. there is one redundant variable. As a result of these n-1 dummy transformations of the n lithological classes, the reference lithology does not have its own beta coefficient in the regression, i.e. the beta value, and the corresponding Odds Ratio (OR) should be interpreted relative to the reference value. Examples of such approaches can be found in Dai and Lee (2002) and Goverski et al. (2006) . These references are now added to the manuscript: pag; 7 line 6. A negative beta-coefficient for variables of the n-1 lithological classes indicate a OR below 1, i.e. a lower probability of landslides compared to the reference lithology and vice versa. The word "quarts" has been replace by the correct term "quartzites".

11. RC: Page 31: Table 4: Same as previous comment AC: Idem as 10.

(3) Technical comments by the referee 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 (Li and Goodchild, 2016). 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

Printer-friendly version

Discussion paper

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

Liesbet.jacobs@vub.be PhD researcher – VUB and RMCA AfReSlide project: http://afreslide.africamuseum.be/ Vrije Universiteit Brussel Pleinlaan 2, 1050 Brussels, Belgium +3226293556

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-259/nhess-2017-259-AC2-supplement.pdf

---

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-259, 2017.