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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Which data for quantitative landslide susceptibility mapping at operational scale? Case study of the Pays d'Auge plateau hillslopes (Normandy, France)" *by* M. Fressard et al.

Anonymous Referee #3

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General comments

This contribution focusses on the evaluation of the effect of using data of different quality (resolution or scale) for landslide susceptibility modelling on the final susceptibility maps. These maps should be suitable in a spatial planning and or hazard reduction framework (RAM) of the French government. This analysis is very valuable, as many stakeholders or practitioners will face the exact same question during the preparation of landslide susceptibility maps with statistical methods. The combined evaluation of

the resulting quality of the landslide susceptibility maps with the involved costs of producing the input data for the analysis is of scientific and practical significance. As the application of this method is proposed to be applied within the RAM methods for each municipality, the usage of a well analysed and broadly applied method, the logistic regression, seems reasonable. However, some aspects of the study design and details of the methodology remain open and unclear for the reviewer. Therefore, some more detailed clarification of some methods and decision criteria to decide on which combination of the parameters is the best suitable for the study area should be added in major revisions to provide a more transparent and comprehensible presentation (please refer to the specific comments for more details on that). The source or mapping method of the landslide inventories (API and field mapping) is not specified in the methods section. Therefore, possible uncertainties arising from the quality of the orthophoto or the mapping method are not comprehensible for the reviewer. The section on the selection of the appropriate cell size was found very interesting with good scientific reasoning. This is also of practical and scientific interest. The discussion of the results could be enhanced by providing a broader view on other (comparable) work done and issues raised in this field of susceptibility modelling and the quality of susceptibility maps (e.g. sampling and sample size, necessity to model run out). The conclusions are substantial and supported by the results and interpretations. The references are appropriate and up to date. The manuscript was for most parts easy to follow, although some minor corrections in spelling and sentence structure are necessary. Moreover, some confusions due to incorrect references to tables and unexplained abbreviations occurred. However, the structure of the manuscript is clear for most parts and the length is appropriate. The figures and tables substantially maintain the understanding of the presented research.

Specific comments

The following comments are given without any specific order or indication of relative importance. These are mainly addressing missing information in the description of the

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methods (data preparation and modelling). Adding these would enhance the traceability of the research presented in the manuscript.

1. As this work seems to be in a close connection with the French RAM some more sentences on these guidelines would be very interesting for the reader. Some more background in a short comment on who is planned to do the (susceptibility) analysis in future would be very informative. Is the proposed method of susceptibility modelling planned to be applied by stakeholders? If yes, it might also give additional justification for selecting the logistic regression method as it is stated to be “simple to apply”. Otherwise simplicity in the application is no scientific reason for using this method.

2. Connected to this guidelines it is not clear if the possible run out of landslides will be considered (according to the guidelines but also in general) to facilitate the preparation of the hazard, or subsequently risk maps. Probably this is planned to be done in the step of producing a hazard map and is therefore not part of this study. According to the manuscript so far the possible spatial occurrence of triggering areas of landslides is modelled by logistic regression not taking into account the possible run out. This results from using samples in the triggering zone cells only, in the modelling. This is generally good and done very often but has to be openly addressed or discussed in the manuscript, as this has restrictive effects on the allowed interpretation of the susceptibility maps.

3. In the introduction a test study area of 24 km^2 is introduced. However, the description of the study area is provided for a much bigger area of 2500 km^2. Additionally in the section 2.2 about the landslides an area with a size of 130 km^2 is mentioned. These different sizes of different study areas are confusing and it is not clear to the reviewer why the description of the study area was done on very different extents. Maybe it would be appropriate, or enough to describe the small study area (24km^2) only, or please specify where the different areas are located.

4. In section 2.2 a landslide inventory is described which was mapped during the

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winters of 2010 and 2011. However, the source of this inventory is unclear for the reviewer. Please provide a reference if this was prepared in an earlier study or refer to the methods section for details on the preparation of this inventory if it was done within this study. If the inventory was prepared within the presented study it actually describes methods of this study. Therefore, this section (2.2) and the Figure 2 would rather belong to the results section, and the methods have to be described in the methods section. Furthermore, it is unclear if this is the same inventory of which a part was used later for the modelling. Please also give some details on the size of the landslides in the study area, as there might be differences in the landslide size captured with the different inventories or landslide types.

5. In section 3.1.1 point 3 an inventory is presented, which contains 12 solifluction processes. However, on page 963 Line 6 solifluction processes were considered as quaternary and stable. Please specify if these are the same processes or if these are arising from different inventories and respective mapping times. Maybe this is only a misunderstanding by the reviewer. Please clarify if the solifluction processes were included into the modelling with the "field mapping" inventory and why the different landslide types were not modelled separately as usually recommended. Are the predisposing processes for all the landslide types the same? This might be a reason or possibility to still model all of them at once. However, if the solifluction is considered to be quaternary the past conditions of the environment might not fit today's conditions of predisposing factors and therefore introduce a bias into the modelling.

6. Within the data preparation (3.1) some description of the used methods is missing. As stated in Table 1 the API and Field mapping inventory are originating from work done specifically for this study. If this is understood right by the reviewer, a detailed description of the mapping method and data involved in the mapping (resolution/scale of the orthophotos/aerial photographs) is missing. Furthermore some confusion of reading section 3.1.1 arose comparing the described data sets with the legend in Figure 4. It is not understood by the reviewer if the API inventory results from different aerial

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photographs (with different scale) as the “field mapping” inventory. Furthermore, in the following the third inventory is described often as resulting from field mapping only, but in this section it seems that also air-photo interpretation was used. Please clarify that in this section. Furthermore, the surficial formations map was specifically prepared for this study. This was not clear to the reviewer in the first place. Similarly, the land cover mapping was done specifically for this study, as indicated in Table 1. Please give some more details on the criteria used to determine the different land cover classes (e.g. forest, grassland,..) and specify the data and its specifics used as a mapping basis. Based on this need for clarification a change in the headline “Database available for the landslide susceptibility mapping” might be helpful to avoid misunderstanding in the data sources. The way it is worded at the moment implies for the reviewer that all data was available before the study started and not prepared within the study. A possible title could be something similar to “Data sources and data preparation”.

7. In line 7 of page 965 the inventory is considered to be exhaustive. Probably it is only a matter of the reviewers misunderstanding of the word exhaustive. Please clarify if the inventory is considered to be a complete inventory (including all landslides of the area) or a substantially complete inventory as stated by Malamud et al. 2004 (“Landslide inventories and their statistical properties”). Please consider the discussion of the mentioned paper to enhance the wording in this sentence.

8. Regarding the landslide mapping: it is understood that the mapping was not the main topic of this study, however the question arises if the Radar data and possible derivatives (DEM, hillshade maps, contour lines) were considered as a possible mapping basis for the mapping of landslides as well? As stated in the conclusion the availability of LiDAR images is restricted due to the costs but Radar Data is available. Depending on the size of the landslides the 5m resolution of the Radar DEM might be suitable to map landslides. Maybe it would be a good addition to the mapping on the basis of aerial photographs only. This would be also very interesting from the cost point of view, as the Radar DEM could be used twice and the mapping on it might be faster

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as field work only. Research in a similar topography was performed earlier with the usage of LiDAR data (Van den Eeckhaut et al. 2007: “Use of LIDAR-derived images for mapping old landslides under forest”), and maybe the Radar DEM would be just as good. However, it is understood by the reviewer that this might be beyond the scope of the presented study.

9. Please give some more details on the sampling method of the partitioning into calibration and validation sample. From the details given in section 3.4 it is not clear whether 80% of the landslides or 80% of all grid cells but of any landslide are used in the calibration set. If the reviewer understood this correctly the samples were derived in the second way. However, this might lead to the case that the same landslide is with 80% of the triggering cells in the calibration set and with 20% of the triggering cells in the validation set. Therefore, the independency of the sample, which is important in the validation step, would be questionable, as it is not consisting of landslides at different locations. Furthermore it is not clear to the reviewer how large this trigger areas are, and if or why all cells of the triggering zones were used within the model? As mentioned by other authors, spatial autocorrelation might be a problem within logistic regression (Van den Eeckhaut et al 2006 (“Prediction of landslide susceptibility using rare events logistic regression: a case-study in the Flemish Ardennes (Belgium)”)). Furthermore, by using all cells larger landslides are considered more in the model. (Atkinson and Massari 2011 (“Autologistic modelling of susceptibility to landsliding in the central apennines, Italy”)). Both effects were often avoided by sampling only one cell/point within the landslide trigger or initiation area. Please consider clarifying this in the methods section.

10. As one of the landslide inventories contains a very small number of landslides compared to the other inventories questions regarding the general influence of different sample sizes on the modelling performance and map representation arise to the reviewer. Please consider this in the discussion by referencing relevant literature (for example in recent publications of NHESSD some of these aspects were addressed

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by Heckmann et al. (for logistic regression) and by Petschko et al. (for generalized additive models)).

11. The assembly of the five different parameter combinations is found very good. However, the reviewer would find it very interesting to add one more data set. This additional parameter combination could be used to show if including a more detailed DEM (IFSAAR-DEM) alone can also enhance the maps or the model performance. Therefore, the analysis and comparison of a set DS6 with the landslides from field mapping, the IFSAR-DEM, the BRGM Map and the API land cover is proposed as a very interesting additional test within this study. However, maybe this was already tested but not presented. Additionally, it is understood by the reviewer, that this might be beyond the scope of this study.

12. Regarding the classification method CAPP in section 3.4 several aspects which are described in the following are not clear to the reviewer. Please give a detailed list or table at which threshold values of percentage of the study area the classes were formed. For an appropriate "visual" comparison of the susceptibility map it is necessary, that the classes always cover the same percentage of the study area. This might have been done in this study, but it is not comprehensible for the reviewer from the methods or results section.

13. Please define the possible value range of AUC values in the methods section. Furthermore, in the results section a specific classification of AUC in good, fair (etc.) is used. Please provide the general threshold values for these quality classes in the methods section, as this helps the reader to see, if the values were rather at the upper or lower boundary of the class.

14. Please give more details to the relative error of the AUC value, its calculation and interpretation (what is a high error, what is a low error, and why?). The calculation is presented in Table 3 but should please be presented more obviously but briefly in the methods section.

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15. Often it is referred to “the best” result. However, the criteria of how this “best” result for each data set (DS) was identified, is not clear from the methods section but is somehow indirectly mentioned in the results. Please clarify this in the methods section only and make sure that in the results section no methods are described (and vice versa). Furthermore, please keep in mind the general advantages and drawbacks of interpreting the AUC from the calibration and/or the independent validation set. The necessity of doing this is not directly addressed in the manuscript.

16. The correlation tests of the maps were found very useful and a good idea to do for the comparison of the landslide susceptibility maps. Also the expert opinion and the avoidance of “isolated pixel effects” is found of high practical value.

17. Please present the final threshold values for the classification (if different for each map, please see point 11.) and the actual correlation values in the text of the results section.

18. Please make sure, that the discussion does not contain new results, which have not been described in the results section yet. Furthermore, the discussion should put the research done more in the view of other analysis or research done by other authors and discuss similar or contrary findings. Furthermore, the general framework of where this study is located in relation to other studies is interesting to point out here.

19. Some content of the conclusions is rather considered to be part of the discussion (or maybe results) by the reviewer. Line 24 to line 28 on page 976 are discussing the quality of the inventory, which might fit better in the discussion section. Please also consider moving the sentence starting in line 18 to line 23 on page 977 in the discussion section.

Technical corrections

In the following some minor technical corrections of spelling and wording are proposed. Although my English is okay I am still not a native speaker. Therefore, please consider

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a check of the final manuscript by a native English speaker. Please consider the general difference between land cover and landuse maps. Land cover maps are maps derived from satellite data or from aerial photographs, where the details on the exact land use (e.g. type of crop) is not known. Please decide on one term which applies for this study and use it consistently throughout the manuscript. Furthermore, please explain abbreviations used in the manuscript at their first usage, e.g. digital elevation model (DEM), also in Tables or Figure captions. This will not be indicated in the detailed list of the technical corrections.

Page 659, Line: 05: Guzzetti, one "z" was missing 13: These methods use (no "s") GIS . . . 16: but still few of these were used in official RAMs (please add words in bold)

Page 960, Line: 09: . . .few scientists have studied (?) them. . . 13: but remain (without s) 14:the stakeholders (one word) . . .. . . landslide hazard management (each without s) Page 961, Line 08: What does shapes of the map describe? Maybe shapes of the polygons of the susceptibility classes? Please consider rewording.

Page 962, Line: 19: is extremely variable and is a function . . . (please add a) 21: to landsliding in the study area

Page 963, Line: 04: were identified in the study area 16/17: Please transfer "respectively" to the end of the sentence

Page 964, Line: 06: Figs. 5 and 6. Table 2 shows the . . .. (delete The in the beginning of this sentence, and add the s at shows) 16: . . .BRGM website. Only landslides. . .. (delete the The in the beginning of this sentence) 19: . . .slides are identified in the area 21: This inventory contains a low number of landslides (i.e. 15. . .): please specify the exact number of landslides used in the modelling

Page 965, Line: 03: The landslide boundaries. . . (please delete the s at the end of landslides) 23: . . . and is then more in accordance with. . . (the usage and necessity of the word then is not understood)

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Page 966, Line: 01: in order to avoid artefacts related to ... (please leave out the "the" before artefacts) 07: ... susceptibility mapping the surficial formations map ... (please insert the before surficial) 11: These boreholes were interpreted ... (I am not sure if interpreted is the right word either instead of interpreting as the sentence is not very clear to me) 14: Particular attention was paid to... (Please delete the "A" at the beginning of the sentence)

Page 967, Line: 07: pixel size – Please consider using the word grid cell, or cell instead of pixel throughout the manuscript as both terms are used later on as well and the word grid cell or cell or more commonly used than pixel. This will not be commented on in the following. 14/15: ...available thematic maps selecting a detailed grid cell size was considered not very realistic. (Please consider changing the sentence structure to shorten it and make it clearer)

Page 968, Line: In section 3.4 the methods for the modelling and validation are described. However, there are references to figures which already show results and which have not been as well referenced in the results section. Please delete the referring to the Figures of results in the methods section and include this at the description of the specific result in the respective sentences. 04: ... a set of predictor variables – Please consider using the word predictor variable or explanatory variable as a conceptual pair of response variable throughout the manuscript. Hosmer and Lemeshow use the pair response and explanatory variable or dependent and independent variable. The term predictive variables could not be found in this citation. Please consider this throughout the manuscript consistently as this will not be commented on in the following. 20: detailed in the Tables 2 (this is not shown in Table 3). .... A set of landslide data .. (please delete the s in landslides) 22: Fabbri (Please insert a b)

Page 969, Line: 02: ... raw probability maps was obtained... 19: The table which is referred to (Table 2) is wrong. It is Table 3. Page 970, Line: 12: Please consider deleting "presented on the Sect. 3 of this paper" as this is only a repetition of already known things which is not necessary. 17: presented in Table 3 and Figure 9. (Please

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delete the “the” before Table and insert Figure 9) 21: Description of the modelling results (Please consider using the word modelling instead of simulation, as modelling refers better to the wording used in the methods section) 22: Please consider using the word model with DS 1 (or DS5..) instead of simulation. Please consider this throughout the manuscript consistently as this will not be commented on in the following. 23: Please refer to table 3 and Figure 9 here.

Page 971, Line: 04: Please put the word respectively at the end of the sentence. 13: . . . expert opinion and does not indicate high susceptibility levels . . .

Page 974, Line: 02: . . . by the lack of accuracy of the DEMs. Are these maps also affected by the quality of the land cover data? Please add this here. 12/13: The use of the detailed surficial formations map simplifies the zoning of the high susceptibility class. Areas within the highest class are often the most . . . (Please consider splitting the sentence according to the proposed sentences) 15: . . . using surficial deposits maps to model areas prone to landslides. (Please consider changing the sentence accordingly) 17: Please insert a comma after the “etc.)” 18: . . . maps, but are not always sufficient. . . 20: . . .remains an essential step. . . 23: . . . variables have an important cost. . .. 25: . . . that are necessary for the geomorphological. . .

Page 975, Line: 02: the acquisition cost . . . 10: From the engineering perspective, . . . 13: please clarify or reword “conservative by the scientists”

Page 977, Line: 10: . . . to obtain a DEM, but necessitates time-consuming procedures of . . . 16: . . . Corine land cover obviously does not fit the . . . 20: the last 70 yr which constitutes an important limitation in mapping the landslide susceptibility. (Please consider changing the sentence according to the suggestion)

Figures and Tables:

Table 3: Please define the abbreviations “cal” and “val” in the Table caption. Table 6: Please explain the meaning of (+) next to the data accuracy and estimated cost. As it is

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next to every number, maybe it can be considered to delete it, and explain the general meaning in the results section? Furthermore, it remains unclear to the reviewer how these values (4-16) were assigned? What is the minimum and maximum reachable value?

Figure 1: The legend has one line assigning the Pays d'Auge limits. However, there is a black line also around the bigger study area and/or the selected study area. It is not clear to where this black line refers to. Please add another line of a different colour, to outline the limits of the selected study area. Figure 3: (A) Shallow landslide – it seems to be one landslide only, as indicated in the picture. Figure 4: Please add some tick marks showing the latitude and longitude of the location of the map. Furthermore, the terms in the legend do not fit to the data described in 3.1.1. Please update this accordingly. Figure 8: Please indicate the threshold values of the shown example in order to make the classification method easier understandable.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 957, 2013.