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.

Response to anonymous referee 3

The authors wish to thank the careful revisions of the manuscript and the valuable suggestions. Most of the corrections and suggestions were included in the paper to submit for publication in NHESS. The reviewer comments (in italic) are followed by the authors reply (in bold).

Specific comments

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.

Some more descriptions related to the background of the French RAM were added in the introduction section. The methods of susceptibility modelling are not yet planned to be added to the official RAM procedures and this work is not performed in association with officials that will possibly use it into a RAM. It is just questioning more generally this possibility.
The justification of selecting the logistic regression was modified in this way, now only referring to its popularity in the scientific literature.

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.

This is definitely true. The question of the run out assessment is very interesting and obviously essential the framework of hazard zoning. Nevertheless, we chose not to discuss of it in this paper as it implies the use of an independent modelling step that would maybe necessitate the same kind of stepwise comparison of the effect of the data quality. We considered that the question of the "spatial source area probability" was sufficient to assess in a first step. We have integrated a sentence discussing this point in the revisited manuscript.

3. In the introduction a test study area of 24 km2 is introduced. However, the description of the study area is provided for a much bigger area of 2500 km2. Additionally in the section 2.2 about the landslides an area with a size of 130 km2 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 (24km2) only, or please specify where the different areas are located.

We are conscious of this issue. Then we simplified according to the reviewer suggestions. The description, including the landslide typology description were simplified and only focused on the 24 km² study area, and just mentioning that this area is included in a more global regional context. (i.e. all references to the intermediate study area were deleted and reported to the small, 24 km² area).

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

We referred to a previous study (published in French, Fressard et al., 2011) for the landslide inventory.
Since there are no big differences on the size of the captured landslides according to the different methods, we referred to Fressard, 2013 (phd thesis) that will be published by the end of December.

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.

Only the shallow landslides susceptibility is mapped for this study. The solifluction and deep seated landslides processes are mostly inactive or dormant processes that arise from different climatic conditions (e.g. Pleistocene). More details were added on this point at the end of the section 3.1.1.

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.

Some more details on the scale/resolution and date of the orthophotos were added in the text. Considering the field mapping method, this is a classical systematic field survey geomorphological analysis. This was clarified in the text.

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 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.

The API was performed using two sets of recent orthophotos (2006 and 2009). The third landslides inventory was obviously constituted using at first the data of the API, completed by field survey. This point was clarified in the text.

Furthermore, the surficial formations map was specifically prepared for this study. This was not clear to the reviewer in the first place.

This point was clarified in the text.

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.

The landcover API was performed using the 2009 orthophotos. We don't understand the first part of the reviewer comment about the "criteria used to determine the different land cover classes" since the details about each class is already given in the text.

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".

This point was modified according to the reviewer suggestion.

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.

This point is a translation error from French to English. We consider that the landslide inventory is "substantially complete inventory as stated by Malamud et al. 2004". This reference was added in the manuscript.

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 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.

This point was actually tested, but the resolution and accuracy of the radar DEM remains a little too coarse for this type of landslide mapping. The main old deep seated landslides can be identified using the radar dem, as well as on the 1/25.000 scale topographic maps. Moreover, for the areas covered by forest, the provided dem is smoothed and also do not permit a detailed view of the topography.

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.

A specific section called "landslide data preparation" (3.2) was added in the text, presenting and discussing all points mentioned by the reviewer.

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

This issue is now mentioned in the discussion section.

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.

This combination (BRGM geological map + IFASAR DEM) was actually tested (as several other combinations) but did not finally enhance the result much. I was then decided to present only the models characterized significant improvement of the results quality. The aim was to limit the number of "DS" presented, and we have considered than 5 was a good compromise in this stance.

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.

Since the results are provided by different landslide inventories and different datasets the CAPP curves are characterized by different shapes. It is then impossible to define the exact same proportion of the study area for each of the susceptibility classes on each map. We nevertheless tried to use roughly equal proportions of the study area to classify the landslide susceptibility maps. Some more details about the classifications problems are provided in the text (see section 3.5.2). We added as well the CAPP curves of each of the models to illustrate the impossibility of defining common criteria of classification. Finally a table summarizing the proportion of each class was added to the paper.

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.

This point was modified according to the reviewer suggestion.

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.

A short description and the calculation formula were added in the text.

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.

These points were clarified in the method section according to the reviewer suggestion.

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.

Thanks!

17. Please present the final threshold values for the classification (if different for each map, please see point 12.) and the actual correlation values in the text of the results section.

Since we modified the classification method (and add the following description in the method section) this point is no longer presented 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.

A paragraph was added in the discussion section, putting the paper in a more general context of landslide susceptibility mapping.

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.

This point was modified according to the reviewer suggestion.

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 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.

All language technical corrections were taken into account and a very good English speaker reviewed the paper.
The term landuse was selected and all references to "landcover" were deleted except for "corine land cover".
The abbreviations were explained in the text.

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)

Corrected

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)

Corrected

Page 961, Line 08: What does shapes of the map describe? Maybe shapes of the polygons of the susceptibility classes? Please consider rewording.

Corrected

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

Corrected

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

Corrected

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 modeling.

Corrected. A table summarizing the number of landslides used in the modeling for each of the inventories was added.

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)

Corrected

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)

Corrected

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)

Corrected

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)

Corrected

Page 969, Line: 02: : : : raw probability maps was obtained: : : 19: The table which is referred to (Table 2) is wrong. It is Table 3.

Corrected

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 delete the "the" before Table and insert Figure 9) 21: Description of the modeling results (Please consider using the word modelling instead of simulation, as modeling 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.

Corrected

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 : : :

Corrected

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: : :

Corrected

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

Corrected

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)

Corrected

Figures and Tables:

Table 3: Please define the abbreviations "cal" and "val" in the Table caption.

Corrected

Table 6: Please explain the meaning of (+) next to the data accuracy and estimated cost. As it is 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?

The table capitation was modified and describes more clearly the way the values were obtained and the minimum and maximum reachable values. The (+) symbols were deleted in the table.

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.

Corrected

Figure 3: (A) Shallow landslide – it seems to be one landslide only, as indicated in the picture.

Corrected

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.

Corrected

Figure 8: Please indicate the threshold values of the shown example in order to make the classification method easier understandable.

The figure was modified and now shows all calculated CAPP curves and thresholds values.