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Discussions

Interactive comment on "Assessing the quality of landslide susceptibility maps – case study Lower Austria" *by* H. Petschko et al.

Anonymous Referee #2

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The paper deals with a major issue in the study of landslide and debris flows: assessing the quality and the accuracy of landslide susceptibility maps created by statistical methods. The model performance is analyzed by a k-fold cross validation with spatial and random subsampling. The paper emphasizes the importance of assessing the uncertainties of the method for a forward prediction. As a study site, "Lower Austria" was selected which contains a high variability of lithological factors and a very heterogenic nature. Even though the paper addresses a main issue in the research field related to gravitational flows, there are significant weaknesses in the content of the manuscript and many unjustified assumptions which could have been avoided. On the other hand, the manuscript is written in a clear form and well structured. The paper must undergo moderate-major revisions for publication in NHESS.

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Here are general and more focused comments:

General comments.

A general comment about the content and title: the authors must and should make a clear distinction about statistical and deterministic susceptibility models. In the paper it is assumed that all the susceptibility maps created uses a statistical approach which is clearly not the case. More and more deterministic models are being used to give a physical aspect to the assessments.

The authors have tried to report and refer to the literature in a thorough manner but some interesting work than can contribute to this document can be added. For example:

- van Westen, C.J., van Asch, Th.W.J. and Soeters, R. (2006) Landslide hazard and risk zonation : why is it still so difficult?. In: Bulletin of engineering geology and the environment IAEG, 65 (2006)2 pp. 167-184.

- van Westen, C.J., Castellanos Abella, E.A. and Sekhar, L.K. (2008) Spatial data for landslide susceptibility, hazards and vulnerability assessment : an overview. In: Engineering geology, 102 (2008)3-4 pp. 112-131.

Regarding also references, the paper is lacking of an analysis of the important results and issues raised by other studies, in particular in context of the submitted paper. Discussion of the results obtained in the submitted manuscript should be made by comparing qualitatively and if possible quantitatively with the results obtained in referenced studies.

A very important part of this document is the inventory of past events. An extra figure should be added where the inventory is represented and its attributes are reflected in a proper manner.

Concerning the GAM model, the authors should refer to the recent work done with the model and it applicability. Actually at the moment, the presented model makes it

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difficult to foresee its usage in other cases and locations.

What is really the added value of a k-fold cross validation in comparison to the mentioned assessment? The authors should make this clear in a concise way. Is it because of the ranges of possible AUROC? How would this help a decision maker?

The methodology at some points gets confusing and difficult to follow. I suggest that a graphic and detailed workflow-framework should be added in order for the reader to understand the entire process.

The authors should revise carefully the numbering of the equations, tables numbers and to mention them correctly inside the text. In addition the editing and the English should be revised in some sections.

Detailed comments.

- P1003 line 20 – 25: I suggest that the most used and applied models should be mentioned or briefly described. Then it would be easier to understand why they tend to overfit or why are they not flexible. This will enhance the manuscript and the methodology used.

- P1004 line 10 – 15: It should be clear why the k-fold validation is chosen and in which grounds. The authors should quantify how much more reliable is this than traditional methods. In line 15 the authors mentioned about uncertainties and ways to communicate them. This is very important but I can not see how this document assists on this, neither in the methodology nor inside the manuscript. The authors should be quite clear in showing how they tackle this problem.

- P1004 line 26: I suggest rephrasing this sentence and to be more specific regarding the terms of quality and its analysis.

-P1005 line 5 – 8: I disagree with this statement. In fact, I suggest that the authors rephrase the complete paragraph. The idea of making a susceptibility map which can lead to a risk assessment (quantitative) is to assess the uncertainties that are either

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Discussion Paper

inherent or non inherent. There are ways and procedures to assess these uncertainties and quantify them. The authors should carefully look into this.

- P1005 and P1006: It is a nice effort to try to summarize the aspects that can influence the quality of a susceptibility maps. But at the same time, I would suggest including the uncertainties derived from them and divide them in epistemic and aleatory.

- P1005 and P1006: In the course of the manuscript the term "susceptibility map" is very general. The authors assume that these types of assessments are done in a regional scale with a statistical approach. This is not the case in reality. I suggest that the authors make a clear definition of the type of susceptibility map they are analyzing and if their proposed method can be applied to other type of susceptibility maps.

- P1005 line 27: I suggest changing the subtitle "Quality of the statistical model". It is not the quality of the models that should be the focus in this section (since they have been applied soundly in many other cases) but how the performance of the models and its applicability to landslide cases. This performance should be linked to the quality of the data and the uncertainties from the application.

- P1006 line 15: This statement should be referenced or if not explained thoroughly. For this reason, it is suggested to define the uncertainties involved in the mapping and to classify them. Once this is done, it can be referred to this classification.

- P1006 line 26 – 28: I suggest rephrasing the sentence. This is dependent on the amount of susceptibility classes and the choice of classification.

- P1007 line 1 -13: The authors attempt to mention the quality achieved by communication and presentation of the uncertainties. However, the paragraph is very poor and does not present something valuable for the manuscript. This is a very complex issue that the authors should discuss in more detail (if not, this should be removed or added to other section.

P 1007 Title section 3: The subtitle is very generic and does not portray the area as

such. Many places in the world are heterogeneous too. Is this heterogeneity based only on the lithological factors? Then this should be included and a more concise title is suggested.

P1007 section 3: The authors should include a map and a table regarding the inventory. This will help the reader to understand the attributes and the spatial distribution of the inventory in the heterogeneous area.

P1007 line 27: The authors should explain why and what is the importance of featuring the median slope angle. What influence this have in the result and what is the significance of this value in a scale like this?

P1008 line 4: Land use as mentioned in the manuscript differs among topography and lithology. Why not consider this factor? The authors mention the temporal aspect of land use, but this is not an excuse to exclude this of the analysis. Besides this, land use has a very important influence in the infiltration process. Why instead of using proxies for infiltration, not link the land use to these factors too? I suggest that the authors make their assumptions understandable regarding this and make a clear reasoning about this.

P1008 line 17: The choice of earth and debris slides is clear and the authors try to include hydrological factors inside the analysis which is a good point. However, one of the main triggers of this type of slides is the intensity-duration of the rainfall. How is this taken into account in this analysis?

P1009 line 10-12: It is not clear why the authors had a LIDAR data with a resolution of 1x1, they resample it to have a final resolution of 10x10. I am certain that this loss of resolution will affect the slopes and derivatives factors. What type of resampling was done?

P1009 4.1 Response variable: Choosing to work with a point inventory has a great influence on the final susceptibility map results, mostly when assessing fast moving

landslides like debris flows. I disagree with the authors that there are small differences when choosing the main scarp or the entire landslide, this is a clear problem of scale (check also reference listed). The authors should dig into this deeper and come with the right assumption for using a point inventory. Another difficulty regarding this is the slide mechanism and its retrogressive failure. Is the point inventory enough then?

P1010 4.2 Explanatory variables: How is it taken into account a debris flow that happens several times in the same channel?

P1011 line 8: Infiltration and run-off are closely related to land use also. Why only use the lithological factors? Is this accurate enough?

P1011 line 15-28: Is this relevant for debris flows? How do the tectonic arrange affects this flows in reality? Mention some references regarding this.

P1012 line 1: Why not used the generated lithological map to make this portioning?? Why used a map that is not use the assessment?

P1012 Modeling heterogeneous areas: If the area is so heterogeneous and the area is already subdivided in homogenous areas, why not make the analysis in the single homogenous divisions. This will make the assessment more accurate according to Blahut 2011, also the GAM model will fit better the data. Can the authors explain this?

P1013 Generalized additive model: I am not sure if I missed it inside the manuscript but it is still not clear to me how was the overfitting of the model approached in this analysis. Can the authors mention the model's functions on a table?

P1016 line 6: What is the point of this? The results are already predisposed.

P1016 line 7: The authors should give relevant references in this part of the manuscript.

P1017 Section 5.4: The authors should describe the meaning of the transferability and consistency indexes in a way that the reader is familiar with these terms.

P1019 line 13 to 18: I suggest including a figure regarding this.

P1022 line 10-14: Explain the reason why these reductions give better values but decrease the transferability of the model. P1022 Section 6.3: This is clear since most of the topographic influence of the factors. How can this be overcome in this particular analysis?

P1024 line 1-5: I suggest including a table of errors.

P1025 line 4-15 and P1026 line 5-10: The authors should clarify how this influences the final susceptibility map and how to avoid this.

P1026 Section Discussion: The discussion section should be more critical to the work and the difficulties found on it. So far it seems to replicate Section 2 with the assumptions made, the methodology used and some results. I suggest that this section be modified to include how the work can improve the quality of susceptibility maps and its transferability to other cases.

P1031 Section Conclusions: The conclusions are poor and do not reflect the results. This arises the question about the credibility of the created susceptibility map. Land use and the triggering effect are not considered in a correct manner. Meanwhile, the other factors chosen are mainly dominated by the slope. Is it surprising that the slope has the biggest influence on the results? How to include the propagation of uncertainties during the analysis? The papers makes the susceptibility assessment feels like is not worth to use it for a decision making process. It arises the question...is it worth to make models more complicated or to simplify them (since mostly the same results will be obtained) in order to make them understandable and easier to communicate.

Figures and table comments.

Table 1: What is the point of adding a median slope angle? Is this relevant to the final assessment? As mentioned in the paper, these are geotechnical parameters but I can not see any type of geotechnical parameters (i.e. strength parameters) but just a lithological description. Besides this the table is confusing and not easy to grasp.

For example, there are more landslides in a 12.6 degree median slope than a 20.2 degree median slope and the one with 16.6 degrees has very few. Problem arises when looking at the density, there are 4.27 (density) slides in a slope of 4.8 degrees which is a fine sand silty marl but there are 2.08 (density) in 20.2 degree limestone, marl and sandstone. I suggest adding more relevant information to the table.

Table 2: Once again, this seems to be a topographic susceptibility map only influences by the topography (according to the table values). Hydrological factors can be almost discriminated while in reality this is a very important factor in terms of triggering and soil conditions. I suggest revising this carefully.

Table 3: It is a nice table but can be linked to table 1 in order to give space to other figures. This can be the authors decision of they think the table is relevant enough.

Figure 7: Nice figure but it would be good to see it displayed on a map to have a better grasp of the concept.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 1001, 2013.

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