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# ***Interactive comment on “Quantitative spatial analysis of rockfalls from road inventories: a combined statistical and physical susceptibility model” by M. Böhme et al.***

**Anonymous Referee #2**

Received and published: 22 March 2014

The paper deals with a major issue in the study of rockfalls: the effect that an inventory has on a statistical susceptibility analysis. This effect becomes evident due to a common practice when creating rockfalls inventories: the registration of impacts instead of the release source. In addition, the authors found several potential controlling parameters using a Weights of Evidence method in the Norwegian country of Sogn and Fjordane. Success rate and prediction curves were created to assess the best performing model. The authors suggests a combination of statistical susceptibility models with physically based models in order to restrict the areas that are steep enough to represent a potential rockfall source.

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Even though the paper addresses a main issue in the research field related to gravitational movements, there are significant weaknesses in the presentation and in the content of the manuscript. Here are general and more focused comments:

## General comments:

1 - The paper lacks of innovation and new contributions. The authors implement a method that that has been used substantially in the past. To my understanding the author's contributions lies on finding factors that are well known to have an influence in rockfalls. The authors should make clear in which ways and how is the work carried out of scientific significance.

2- Why is advantageous to combine a statistical and physical model instead of using only a physically based model? (as the physically based model can also be validated in terms of past events). It was difficult to understand why this is better from reading the manuscript. Isn't overindulging to use two different methods? How is the combined uncertainty assessed then? I do not believe that the results substantiate this hypothesis in a satisfactory manner.

3- I agree with the author's premise to not include slope as a controlling factor. However they do use other topographical aspects which are controlled by the terrain morphology. How are these influencing the results?

4- Past studies have shown the importance of resolution and scale in this type of analysis. A 10m DEM is available for whole Norway, why is the authors choice to use a 25m DEM? Can the authors discuss this in more detail and how these affect or not the obtained results?

5- The authors should clarify how the parameters classes were obtained and based on what criteria. For example, why is the slope aspect is divided in 5 classes and the relative relief in two?

6- The climate factor is quite important in terms of rockfalls and mostly in this type of

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environment. However, it is included and described very poorly in this analysis. The authors should make a better case regarding the climatic dependency and rock fall initiation. Why do the authors assume that three categories of normal annual precipitation is representative enough?

7 - Even though the authors have tried to report and refer to the literature, the resulting references are very limited. Some examples:

- Begueria S (2006) Validation and evaluation of predictive models in hazard assessment and risk management. *Nat Hazards* 37(3):315–329
- Corominas J, Mavrouli O (2011) Rockfall quantitative risk assessment. In: Lambert S, Nicot F (eds) *Rockfall engineering: from prediction to mitigation*. Wiley, New York, pp 255–296
- Crosta GB, Agliardi F (2003) A methodology for physically-based rockfall hazard assessment. *Nat Hazards Earth Syst Sci*3:407–422
- Glade T, Crozier MJ (2005) A review of scale dependency in landslide hazard and risk analysis. In: Glade T, Anderson M, Crozier MJ (eds) *Landslide hazard and risk*. Wiley, London, pp 75–138
- Jaiswal P, van Westen CJ, Jetten V (2010) Quantitative assessment of direct and indirect landslide risk along transportation lines in southern India. *Nat Hazards Earth Syst Sci* 10(6):1253–1267
- Thiery Y, Malet J-P, Sterlacchini S, Puissant A, Maquaire O (2007) Landslide susceptibility assessment by bivariate methods at large scales: application to a complex mountainous environment. *Geomorphology* 92(1–2):38–59.
- Zezere JL, Garcia RAC, Oliveira SC, Reis E (2008) Probabilistic landslide risk analysis considering direct costs in the area north of Lisbon (Portugal). *Geomorphology* 94:467–495

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Another important source missing is: "Analysis of landslide inventories for accurate prediction of debris-flow source areas" J Blahut, CJ van Westen, S Sterlacchini *Geomorphology* 119 (1), 36-51.

In order to enhance the introduction, the results presented in the present submitted paper have to be discussed in regards to these papers results.

8 – The English in the text should be revised carefully.

9- The map in the figures are somehow too small to appreciate some features. I also suggest to revise the legends and the scale in them (make them consistent and remove unnecessary descriptions). The coloring should be consistent and should communicate clearly the results.

10- I would suggest that the authors include a workflow chart or graphic as an extra figure that explains how was the work and the methodology carried out.

11- The authors should work and improve the abstract. It is quite poor at the moment.

Specific comments:

Line 4 page 82. Revise the sentence structure (English).

Line 7 page 82. Remove the words: "with the help" and use a better term.

Line 11 page 82 and Line 4 page 83. Fix the sentences (English).

Line 9 page 83. How does the spatial uncertainty of the source initiation assessed then?

Line 22 page 84. Is this obvious??

Line 29 page 83. The feeling that the authors are transmitting with this document is that they are trying to make a model fit no matter what; even if the data has bad quality and is not representative to the physical characteristics of the event. I challenge the authors to include in the discussion section, if this effort of twitching and fitting a model

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is better than to improve ways of obtaining a detailed inventory (i.e. with new mapping techniques).

Line 3 page 85. It would be interesting to include Derron, 2010 map as a figure and discuss briefly his findings, shortcomings and assumptions.

Line 6-13 page 85. Fix this paragraph.

Line 10 page 86. I disagree with this statement as all the statistical susceptibility models are far from having physical meaning. Why would the Weights of Evidence method have a better physical meaning than other statistical method??

Line 12 page 86. I disagree with this statement. Can a standard user make a rockfall assessment with a Weight of Evidence method using Spatial Data Modeller and not feel like it is a black box? How is this different from the other “complicated” models that the authors mention?

Line 9 page 87. Include more and relevant references.

Line 4 -10 page 88. Fix the paragraph. The description of the weights are confusing.

Line 19 -30 page 91. The authors should describe how was the training area chosen? Was it evenly distributed or also fitted in order to perform equally well?

Line 5 page 92. Is using a 1:250 000 geological map good enough? The authors should discuss in detail how resolution in terms of maps can affect an assessment like this.

Line 22 page 95. Is this relevant to the paper? The “topography and derived parameters” section should be rewritten because at the moment it seems more like an Esri tutorial than a methodological description.

Line 22 page 95. Was planar and profile curvature also computed? Why was not this considered as a parameter but slope aspect was? Author should discuss this in detail in this section as the selection seems inconsistent regarding terrain features.

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Line 15 page 101. How are these results affected and biased by the parameter classification?

Line 10 page 102. Given that the results presented are quite limited, I don't believe that the capabilities (strengths or weaknesses) of the model can be properly assessed by a reader. I suggest improving and enhancing the discussion and the conclusion section.

Figure 1 page 114. This figure should be fixed as the buffers seems like a continuation of the fjords. It poorly describes what the authors want to show. In addition, I would recommend to add two figures where you can see the inventory itself and the training area spatially distributed.

Figure 3 page 118. The influence of the inventory and its quality is clear from these graphs. Can the authors discuss how a better and more detailed inventory would enhance a study like this?

Figure 4 page 119. Can the authors elaborate more on this graph inside the body of the text. Is this a good success and prediction rate curve? Is the steepness of the curve good enough? Why?

Figure 5 page 120. Move the legend of the map. Now it is in the center.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 81, 2014.

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