

Interactive comment on “Forest harvesting is associated with increased landslide activity during an extreme rainstorm on Vancouver Island, Canada” by J. N. Goetz et al.

Anonymous Referee #1

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NHESS – 27 October 2014 (Post) 2, 5525-5574, 2014 Received 07 August 2014; Accepted 18 Aug 2014; Published 27 August 2014 Comment on “Forest harvesting is associated with increased landslide activity during an extreme rainstorm on Vancouver Island, Canada” J.n. Goetz, R.H. Guthrie, and A. Brenning Manuscript Type: Research Article

General Comments The theme addressed in the manuscript is of interest and relevant within the scope of landslide susceptibility analysis, in particular for areas where landslide occurrence during a specific rainfall event could be related with forest harvesting. The manuscript, in my modest opinion presents some fragilities but may be published

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after major revisions. Strengths: the methodology/the scientific method. Weaknesses: The type of data available to characterize the preparatory/predisposing factors to landslide occurrence.

Specific comments Scientific Significance/Originality: The manuscript presents and interesting approach to evaluate landslide preparatory conditions that are responsible for landslide occurrence during a rainfall triggered landslide event. A nonparametric statistical method, the generalized additive model (GAM) is used. In my opinion one of the main contributes is the approach used to determine the regional effects of forest harvesting activities, among other variables, to landslide initiation. With respect to the preparatory factors it is worth nothing that apart of the limitations relative to some variables, such as lithology, or rainfall, an accurate approach to classify and spatially validate forest cover map is made.

Scientific Quality: The manuscript address scientific and technical subjects relevant within the scope of NHESS, nevertheless, and in the opinion of the reviewer, some drawbacks are present in the different sections of the manuscript and must be clarified, modified, eliminated or discussed in more detail.

1)In relation to the critical rainfall conditions that are responsible for landslide initiation on the Pacific Coast of British Columbia, including the mid-November 2006 landslide event, authors refer or cited: “heavy rainfall”/ “heavy precipitation” (Page 5526, line 21/22); “extreme weather event” (Page 5528, line 16/23); “high intensity rainfall and extreme winds” (Page 5528, line 17/18); “extreme rainstorm (page 5531, line 3); “long-lasting high-intensity precipitation events as observed in this study” (page 5544, line 9/10). Please clarified and define if possible how exceptional the rainfall event was. Additionally authors should explain better why select the 15 days of antecedent rainfall as the antecedent preparatory conditions responsible for the event of instability. Basically, why not 10 days or 20 days? Were those critical conditions amount/duration based on, for example, the higher return period? Please clarify in the manuscript.

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2) To section 2 authors called "Methods". In the reviewer opinion, what are presented there are not only methods but also the description of the study area and the description and preparation of data relative to the preparatory / predisposing variables and that should be clarified / reorganized / eventually shortened. Section 2.1 is too long and part of that information should be included / adjusted in the following topics (2.2 to 2.5).

3) In section 2.2, page 5531, line 5, authors say that "the location of the initiation point was manually digitized where the main scarp may be expected. These initiation points were used to approximate the environmental conditions that led to slope failure". Is one point by landslide enough to determine the predisposing conditions that lead to slope instability? In my opinion this is an important drawback of the approach. Authors should provide a more detailed morphometric analysis of the landslides in order to reduce the uncertainty related to the type of landslide representation. Additionally, what is the estimated error associated to the manual determination of the landslide initial point in the expected landslide scarp?

4) Figure 3 should be modified. The relation between daily rainfall and the altitude of the station is not easy to understand. Please reformulate the figure or consider to remove it.

5) Authors say in Page 5537, line 17 "the sensitivity of each model at a specificity of 90% were estimated; a specificity of 90% means that (only) 10% of the non-landslide area is misclassified as susceptible to landslides, creating false-positive predictions" and what about the false negative predictions?

6) Figure 5a was not cited in the manuscript.

7) Why in the section 3.3 the characteristics of landslides authors compare only with forest cover and forest service roads and why not with rock type or slope angle or other variables as authors state in line 13 of page 5540?

8) Why authors do not use soil depth or as a predisposing factor instead of rock type?

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What the reason for the weak association described in page 5541, line 23?

9) Table 5 was not cited in the manuscript.

10) In case of the landslide susceptibility model presented by authors in Figure 11, if I understand well (page 5542, lines 22-27) the model predicts in the 10% of the area classified as more susceptible 54% of the landslide incidences. In this case the validation is done with the landslide initiation points. My question is how much of the total landslide initiation area the model is able to predict? In my opinion for a complete model validation authors should use the entire landslide initiation area in the validation process. That kind of more robust validation, is fundamental to evaluate the uncertainty associated to the initial assumption that a single point is enough to determine the preparatory conditions that turns unstable a part of a slope.

Presentation quality: Overall, the manuscript is well written, well-structured and presents a clear language that is understandable and scientifically precise. The title, the abstract, the subtitles and the figures and tables captions are in general adequate. With respect to figures and tables they present generally a good quality and are almost all adequate to the purpose of the manuscript and level of the NHESS journal. Nevertheless and in respect to those items described above some comments are made in the "Scientific Quality" section and in the attached commented manuscript.

Technical comments (typing errors, format, etc.) Some additional comments regarding some problems with the references list and citations along the text are described below. For supplementary comments/suggestions see the attached file. Additionally, it is worth noting that all references are in English and a substantial number of references are accessible to the fellow scientists.

Items cited in the manuscript but not present in the References list: Page 5530, line 12 - Guthrie and Evans (2004) – confirm if it is 2004a or 2004b; Page 5531, line 18 - Guthrie et al (2010) – confirm if it is 2010a or 2010b; Page 5532, line 6 - Goovaerts (1997) – confirm if it is 2000; Page 5532, line 19 – Swain and Davis (1978); Page 5544,

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line 17 – Wu and Mckinnel (1979) – confirm if it is Wu et al. 1979; Page 5544, line 25 – Guthrie et al. (2002); Page 5545, line 7 – Mills (1997); Page 5545, line 27 – Goetz et al. (2012);

Items not cited in the manuscript but present in the References list: Page 5551, line 18 - Davis et al. (1978) Page 5551, line 23 - Dhakal and Sidle (2003) Page 5551, line 29 - Foody (2004) Page 5552, line 15 - Guthrie and Evans (2004b) Page 5553, line 17 - Lineback Gritzner et al. (2001) Page 5553, line 28 - Montgomery and Dietrich (1994) Page 5556, line 5 - Wu et al. (1979)

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/2/C2341/2014/nhessd-2-C2341-2014-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5525, 2014.