EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-1320-RC1, 2023


# Comment on egusphere-2022-1320

Anonymous Referee #1

Referee comment on "Coastal earthquake induced landslide susceptibility during the 2016 Mw 7.8 Kaikōura earthquake, New Zealand" by Colin K. Bloom et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1320-RC1, 2023

GENERAL COMMENTS

The paper investigates the differences between coastal and inland landslides and evaluates probabilistic landslide models regarding their prediction performance for regional assessments. The research scope and methodology are clear, and the results are well presented. However, the implications of the findings remain a bit vague. The discussion suggests that there are no significant differences between the coastal and inland trained model, raising the question on how the results of the paper can contribute to future landslide assessments. The paper could benefit from highlighting the value of the findings and elaborating on the scope for future research.

SPECIFIC COMMENTS

Line 46 on page 2: What kind of landslide models are we talking about here? If we are looking at probabilistic models which are trained on landslide inventories, and if "a significantly higher landslide density was observed on coastal hillslopes as compared to inland hillslopes", should we not expect that those models reflect the landslide distribution across these areas? Perhaps, you could provide a bit more context on the landslide models regarding how they are biased towards inland hillslopes.

Line 56 on page 2: "No clear physical control on landslide density was identified although several hypotheses were explored" – What hypotheses were explored? Further explanation would be helpful here.

Line 209 on pages 8 (Table 1): The LINZ 8 m DEM is used to calculate different features. LINZ data service states that the "main criterion in its production was the detailed and accurate depiction of natural landforms. It is therefore suitable primarily for cartographic visualisation. Because it was created by the interpolation of 20m contours with post-processing and filtering it is not suitable for terrain analysis." (https://data.linz.govt.nz/layer/51768-nz-8m-digital-elevation-model-2012/metadata/). What are your thoughts on this? Have you thought about potential issues in the outcome using the 8 m DEM data? I used the 8 m DEM and the 25m DEM by LRIS, which was derived from the same 20m contour lines, to calculate slope and other hydrology related features, and I encountered several issues as the results differ significantly in some places. So it would be interesting to know what your reasons are for using the 8 m DEM, especially given the fact that other variables such as the NDMI have a lower resolution.

Line 242 on page 10: It is stated that the ROC AUC was used to "demonstrate the relatively high performance of all trained models". Considering the high class imbalance (around 99% of the sample are negative cases), is it not possible that the AUC is biased towards the majority class?

Line 370 on page 18: What could be the reasons for the similar performance and coefficients? Could this be related to the observational data (Kaikōura)? Or could the conditions between the coast and the inland terrain be too similar, so developing a coast specific model does not add any (measurable) value?

Line 481 on page 22 (related to the previous comment): Considering that the slope is the driving variable in the prediction of landslides and considering that the slope along the coast is only 1° steeper on average, it does not seem surprising that a model trained on costal landslides leads to similar results. The discussion and conclusion are a bit vague on how the findings can contribute to future assessments. Adding a coastal factor to the model is an interesting idea. However, the ROC AUC charts (Figure B1) suggest that the prediction performance does not benefit from that factor. It would be helpful to provide details on what future research needs to do in order to better understand the differences between the coastal and inland landslide susceptibility.

Line 670 on page 34 (Figure B5): Have you plotted a probability map using the inland model for this region? It would be interesting to see where the differences are.

TECHNICAL CORRECTIONS

Line 71 on page 3 (Figure 1): The caption contains a lot of information that could be shortened, for example, by removing "The labelled MFS or Marlborough Fault System is north of the Hope fault and the NCD or North Canterbury Domain is south of the Hope fault." This could be mentioned in the text.

Line 107 and 116 on page 5: Different units are used for year ("$y^{-1}$" vs "$yr^{-1}$").

Line 146 and 174 on page 7: Assuming that "Land Information New Zealand Topo50 coastline" and "1:50k Topo50 New Zealand Coastline" refer to the same dataset, choose one description and use consistently throughout the paper.

Line 211 on page 9: Space between the number and the unit is missing in "8m" and "20m". Also applies to some cases in Table 1.

Line 291 (Table 2) and 297 (Table 3): Is there a reason why the numbers are presented in a table and not a chart (e.g., bar chart)?

Line 799 on page 38: Please provide further information of the data references, for example, name of the dataset, date of access etc.

Line 901 on page 41: Similar to previous comment. Please provide further information on the ground shaking data used for the assessment.