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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Comparing multi-criteria methods for landslide susceptibility mapping in Chania Prefecture, Crete Island, Greece" *by* M. Kouli et al.

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I like to offer a few personal comments to the paper published in NHESS Discussion (NHESSD) by Kouli and co-workers.

The first comment concerns the quality of the data used for the landslide susceptibility modelling, and the extent to which the quality of the thematic and landslide data affects (conditions) the susceptibility models and associated zonations. Uncertainty (including biases and errors) in the landslide thematic layers propagates in the susceptibility modelling, and this should be considered in the analysis and in the interpretation of

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the results. In their Conclusions the authors state that the WoE method provided more accurate results that the WLC methods. I presume this is based on the values of the success rates (87.4% for WoE and 84.7% for WLC). However, what is the uncertainty associated with the success rate curves. If the uncertainty is just 5%, which can be a very low value in my experience, the two methods may be indistinguishable in terms of their performances. A related comment is on the combined model (CM in Figure 9) that performed only marginally better (85.0%) than the WLC model. Do the authors have an explanation for this result?

The second comment concerns the landslide inventory. The authors provide little information on how the landslide inventory was prepared, on the types of landslides inventoried, and on the statistics of landslide sizes. This makes it difficult to understand the role of the inventory in the landslide susceptibility modelling. Reading the text and looking at Figure 3, I got the impression – which may be totally false – that most of the landslides were inventoried along roads. Should this be the case, it will not be surprising at all that distance to roads is a good predictor of landslide occurrence. But this clearly would introduce a strong bias in the modelling. A related comment is on the triggers of the landslides that were inventoried. Were these landslides directly or indirectly related to the presence (e.g., construction, maintenance) of the roads? If this is the case, again it will not be surprising that distance to roads is a good predictor of landslide occurrence. But, again, this would introduce a bias in the modelling.

The last comment has to do with model validation, presented in section 6 (page 87). If I understood properly the procedure used for the validation of the two susceptibility models, the outcomes of the models were compared with the available landslide inventory. However, the result obtained gives the authors only a measure of the “model fit” i.e. a measure of how well their models fit the known distribution of the landslides used to prepare the models. The ability of the models to properly predict “future” landslides should be tested against independent landside information not used to construct the susceptibility models. The difference is relevant, particularly considering the statement

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made in the Conclusions (page 88): ". . . both methods provided accurate susceptibility maps which they can be used safely from the local authorities for slope management and land-use planning".

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 73, 2013.