Review
on the paper

Large scale landslide susceptibility assessment using the statistical methods of logistic regression and BSA - study case: the sub-basin of the small Niraj (Transylvania Depression, Romania)
by S. Rosca et al.

The paper entitled **Large scale landslide susceptibility assessment using the statistical methods of logistic regression and BSA - study case: the sub-basin of the small Niraj (Transylvania Depression, Romania)** by S. Rosca et al. aims at providing a comparative study of two statistical approaches of landslide susceptibility.

Overall, the paper is apparently rather well structured and the authors are using simple and clear sentences (however, another English check should have been definitely performed). The conceptual framework of the entire approach is just partially explained, despite the numerous bibliographic references. The purposes of the paper are somehow missing: what is its scope, who might be interested, what is its added-value? The literature of the last 5-10 years is extremely rich in comparisons between qualitative and quantitative approaches or among two or more statistic-probabilistic methods meant to assess landslide susceptibility.

Like more and more papers found in modern literature, the authors are emphasizing just the purely statistical approach, while important parts like argument or data quality and quantity are poorly approached. Starting with the title, the use of "logistic regression" vs. "BSA" is not correct, since the correspondent of BSA would be (in case acronyms would have been uniformly used) "MSA" (multivariate statistical analysis).

The chapter "General considerations" floats somewhere between an introduction and an argument. From the beginning, the authors are making confusions among preparing, triggering and predisposing factors used in susceptibility assessment (the lines 18-21 should be revised). Moreover, the authors are mentioning a "complete" landslide inventory, but there is absolutely no further additional information on the inventory itself and on its completeness (quite a relative term without a proper and strong demonstration) character. Line 11 should be also revised, since there is a sentence disagreement.

The "Study area" chapter is rather skinny, containing absolutely no information on the physiography of the study area. Descriptions of the lithology, morphometry, land use or land cover and even seismicity, further on used as explanatory variables, are not detailed at all, neither through maps nor even through simple descriptions. Moreover, here the study area measures 68 sqkm., while in the abstract the value is 87.

The "Database and methodology" chapter is missing important content: sources of the used data and, more important, there is absolutely nothing on the landslide database. Later on, the term "active landslide" is used, but there are no info on the landslide typology, mapping technique, representativity and so on. The expert judgment used in the database validation should be more carefully explained, since the formulations are quite confusing. The factors are mentioned in a complete manner just in page 8, previously inducing a lot of confusion since in page 4 they are 12 and in page 6 are 15. The same confusion among predisposing, preparing and triggering (the latter, normally shouldn't be took into consideration within a susceptibility analysis without a proper substantiation of the reasoning) factors still persist. The authors make no explanation of the reasoning behind the splitting of the datasets into training and validation, an extremely important fact in the final look of the map (admitted actually by the authors later on, within 4.1. subchapter).

"4.3. Comparison of results" starts with a poor explanation of the differences between the two maps. Figure 7 and the reasoning behind the differentiation of susceptibility classes are not

explained with enough details. Actually, here lies the main argumentation: what is the purpose of the paper and which stakeholders are benefiting from it? If the stakeholders are represented by scientists, the paper fails in bringing a strong added-value, since the subject has been extensively debated during the last years. If the stakeholders are the end-users, then the authors should have better explained the major differences between the two maps, extremely important for establishing proactive measurements.

Some of the figures and captions needs improvements as well: Table 3 and 4 have exactly the same titles; Table 5 has no proper explanation/reasoning of the classification of different variables; Fig.7 - the medallion cannot be read and the position of the study area is not obvious at all; Fig. 4 - should be training and validation data sets; Fig. 5 shows a strange pattern induced by one of the rasters.

According to the above-mentioned arguments, we consider that the presented paper may be published if the authors are willing to incorporate **major revisions** (at the limit with rejection).