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Interactive comment on “Application and analysis of debris-flow early warning system in Wenchuan earthquake-affected area” by D. L. Liu et al.

Anonymous Referee #2

Received and published: 29 October 2015

General comments:

The paper “Application and analysis of debris-flow early warning system in Wenchuan earthquake-affected Area” addresses the important issue of providing reliable warnings for the mitigation of debris flow (DF) risk. The topic is suitable for the journal nhess and it is of interest for the scientific community. The paper describes a method for debris flow formation previsions based on the combination of rainfall forecasts and simplified modelling of debris flow triggering. Tough there are some interesting ideas I think that the paper has many week points that should be amended and carefully addressed.

The main problem is related to the physical interpretation of the triggering mechanism. The topic is widely addressed in literature and many authors agree that two possible

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triggering mechanisms are possible: the evolution of shallow landslides into DF and the progressive entrainment of bed sediment material into run-off. The authors of this paper assess that both the triggering factors must occur simultaneously for effective DF formation. I don't see the reason of this assumption and as far as I know, it hasn't previously been discussed in literature. The authors should justify this assumption or modify it.

Another problem is linked with the calculation of DF density. It is computed by a weighted average between sediment density and water density. The total volume of sediment taken into account is assumed equal to the volume of instable mass. This is not true because soil porosity must be taken into account. Also in the computation of the water volume there is a mistake because the pore water is not considered in the calculation.

The third problem deals with the estimation of successfulness-unsuccessfulness of the predictions. The authors exclude from the computation of the success-rate some pixels in which the previsions are wrong, without a rigorous reason. In particular, as the method proposed is based on the combination of rainfall forecasting and DF triggering model, the success-rate must combine the mistakes of both these two previsions. Of course the author may preliminarily assess separately the reliability of the two previsions by comparing forecasted rainfall with measurements and by evaluate the results of the predictions of DF formation under measured rainfall (instead of forecasted ones) In this case they may provide two success-rates for the two components of the method and then one combined success-rate for the whole method.

Considering all the above problems, I think that the paper can be considered for publication only after major revisions. The revision of the English test is also necessary in order to help the comprehensibility of the paper.

Specific comments:

- The hydrological model should be briefly described

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- The content of table 1 is arbitrary, the choices made need a justification. In my opinion for example the probability of DF occurrence first increases with mixture density but at very high mixture densities it decreases.

- It is not necessary to write the GIS instructions, it would be better to describe the methods (section 4.3)

- 5864/1-7 The problem of long computational time for antecedent conditions could be solved by running continuously the model, in this way, the antecedent conditions are ready every day for the computation of possible triggering.

Technical comments:

5847/10 the meaning of “contribution factors” is explained only after and it is not comprehensible for the reader in this point.

5847/26 please justify the sentence: “enhanced DF will last 20 to 30 years”

5855/1 what does it mean “prediction regional DEM”?

5859/22 please add reference

5861/22 and 23 and 5862/2-3 I disagree with the correction of success-rate here introduced

Figure 5 and 6: please add measure units

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 5847, 2015.

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