Dear Reviewer

We thank you a lot for the punctual revision of our work. The paper represents a summary of my PhD thesis that I have adapted into a scientific publication. I am aware that the topics involved in CRHyME development are very wide and touch different disciplines so I am grateful to have received useful hits for improving my work. In the next parts, you can find the reply to your suggestions and observations.

Review comment for "CRHyME (Climatic Rainfall Hydrogeological Model Experiment): a new model for geo-hydrological hazard assessment at the basin scale" by Andrea Abbate et al.

The authors present a new software that combines hydrological modeling with landslide and sediment transport modeling. With its distributed approach and physics-based modeling, CHRyME is a necessary complement to existing software.

This manuscript is an impressive paper that, because of its cross-disciplinary nature, addresses several very different topics. However, the sentence structure and the English do not always reach an adequate level, which, coupled with the vocabularies specific to the different themes, sometimes makes the text very difficult to understand. I would recommend checking the structure and meaning of each sentence using a translation tool. Furthermore, this article would benefit from additional references. I have made suggestions in this regard in the attached text.

We apologise for the English, and we have planned to do a language proofreading and we want also to split long sentences that may lead to difficulties in reading. About vocabulary we think that a list of acronymous or keywords should be made in one of the appendices in order to facilitate comprehension by the reader, also subdividing them into categories such as (i.e., climatology, meteorology, hydrology …)

Here are the main points:

1) The connection between the different modules is not as clear as it should be in Figure 3. For example, the amount of sediment available for transport is not determined, as I would have expected, by the number and size of landslides triggered, but by the Gavrilovic equation. Similarly, the link between the hydrologic model and the water-dependent variables in the stability equations of the landslide module is also not clearly established. It is therefore difficult to assess the degree of novelty of the model as a whole.

This represents a critical part and we think that a paragraph should be dedicated for.

The amount of sediment is still determined using the Gavrilovich method since is quite well-established methodology for spatially distributed models. The mass wasting coming from landslides is not properly taken into account now since only shallow landslide “triggering” models are included (infinite slope) while erosion is not “perturbed” by local mass wasting. In our opinion, mass wasting processes should be better investigated at the slope scale level according to the literature but haveìing an “rough” estimation also at basin scale could be interesting. This is a feature we are planning to include in the future version of the model that in our opinion is very useful for now-casting prevision or singular case study analysis.

The link between the hydrological cycle and slope stability is made by soil moisture estimation, that is a variable needed to assess stability in partially saturated slope, avoiding the hypothesis of complete saturation (generally considered as conservative solution). Also this part should be better highlighted in the text.

2) I am not very convinced by the 9-pixel buffer, nor by your validation using ROC curves (usually the area under the curve is calculated to quantify the quality of the indicator), for example. However, I think all these points would be much easier to accept if you developed a good "Model Limitations" section in your discussion.

We agree with you. The ROC methodology we applied state for identifying if the slope instabilities computed by the model CRHyME (preditor) were similar to those one recorded by the IFFI landslide catalogue (reference) after the rainfall events examined. This methodology is generally adopted for susceptibility mapping but here we have adapted it to measure the single-event model performance. This part will be revised and better explained.

In our opinion, addressing landslide failure to a single pixel instability is quite reductive since the uncertainties are related to local slope stability parameters and territorial morphology (we are using spatially distributed data coming from a worldwide database with different spatial resolutions that are ok for catchment simulation but still have high uncertainties on slope scale). This strategy (9-pixel buffer) was taken into account bearing in mind the possible risk in the surrounding areas (crowns and landslide boundary instabilities) according to regulations reported in the literature (*PROTOCOL FOR SHALLOW-LANDSLIDE SUSCEPTIBILITY MAPPING, Oregon Department of Geology and Mineral Industries, Portland, OR*) . As a result, we have extended the unstable areas a bit to be more conservative. Anyway, we will rethink this part to make the methodology description more straightforward and comment further on the dedicated section you have suggested.

3) Also, you sometimes write assertions that are too strong or are not well supported by references. I have highlighted some of these in the text. Try to be more nuanced and explain more your modeling decisions.

Thank you very much for this observation. Further investigation in the literature will be done in this sense and a “smoothing” in some parts is advisable according to your suggestions.

4) It is very easy to get lost in the names of different watersheds, subwatersheds, rivers, and stations. So I would recommend having a very clear map showing all the names and referring to it often. In addition, I would suggest referring to stations with the river names in parentheses afterwards, and always specifying the type of thing you are referring to: "Nure rivers" not "Nure".

Thank you also for this piece of advice. We will provide a better representation of the sites we have investigated to make their identification through the text more clear.

I would recommend this article to be accepted under Major revisions, since its content is very interesting, but the way it is presented does not put it to its advantage. However, I also think that Rejecting it would be a good option, notably because I think that the GMD (Geoscientific Model Development) journal could be a (more) appropriate fit for a resubmission. Indeed, this manuscript could be of interest for audiences in natural hazard, but also for more fluvial geomorphology or hydrology publics.

We thank you the reviewer for the comments and for the precious suggestions that will surely improve our work.

The Authors