Response to Reviewer 1

Submitted on 01 Aug 2023
Anonymous referee #1

Yes No

Anonymous in acknowledgements of published article: Yes No

Checklist for reviewers

1) Scientific significance Does the manuscript represent a substantial contribution to the understanding of natural hazards and their consequences (new concepts, ideas, methods, or data)?	Excellent **Good** Fair Poor
2) Scientific quality Are the scientific and/or technical approaches and the applied methods valid? Are the results discussed in an appropriate and balanced way (clarity of concepts and discussion, consideration of related work, including appropriate references)?	Excellent Good **Fair** Poor
3) Presentation quality Are the scientific data, results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of technical and English language, simplicity of the language)?	Excellent Good Fair **Poor**

For final publication, the manuscript should be

accepted as is.
accepted subject to **technical corrections**.
accepted subject to **minor revisions**.
reconsidered after major revisions:
rejected.

Were a revised manuscript to be sent for another round of reviews:
I would be willing to review the revised manuscript.
I would not be willing to review the revised manuscript.

Although we noted an improvement in the discussion of certain modelling points (notably the link between modelling units), the author did not take into account most of the comments made in the first round. The resulting manuscript is therefore potentially very interesting but still very rough and unclear in its presentation of the modelling process and results. Although the level of English is not a problem, in my opinion, for a short article on field observations of landslides, for example, imprecise wording in an article as technical and comprehensive as this one makes it very difficult to read, and I cannot recommend its publication in its present form. I, therefore, suggest major revisions or rejections and refer to my previous comments to improve the manuscript.

Dear Reviewer 1

According to your suggestion we have improved our article working on three main topic:

1) English improvements;
2) Clarification and reworking on some points adding further references (see the list below);
3) Reorganization of chapter 2-3-4 avoiding repetitions/typos/assertions.
4) Figure and caption improvements.

A point by point reply was prepared for the most important comments with remarks about the part of the paper we have modified.

Abstract and Introduction:

Nice paragraph. Could you also (quickly) explicitly specify which are the limits of these models that CRHyME will overcome? Or what is new in CRHyME in comparison with these models? Maybe at the end of this paragraph or at the beginning of the following

Errors were corrected considering your suggestions. CRHyME novelties paragraph was included in the report at the end of the introduction (chapt 1).

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.

Chapter 2 and 3:

Figure 2 and 3 and Table 1 have been corrected following your advice.

Paragraph 2.2.3.1 has been clarified, specifying all the terms added to the equation and making sentences easier to understand. The paragraph 2.3.2 has been completely reworked to justify the choice of 9-pixel buffer. We have added an explanatory figure (Figure 5) to make our choice clearer and we have added a comment in the -discussion Par 4.4 that highlight uncertainties and possible solution to deal with this issue. Figure 6 have been completed reworked and improved adding a sort of chart flow to explain the application of the method. Further references have been added to support our decision bearing in mind your doubts about the scale dependency of the method.

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 modelling decisions.

In this regard, we have fixed most of the unclear statements which were not well supported by the literature. We hope that all the doubts have been clarified especially regarding sediment transport and landslide modelling decisions. In this regard we have reworked the paper in this way: In Chapter 2 a paragraph 2.2.3.3 describing the connection between landslide and sediment transport is explained, motivating the choices and adding further references. In the Discussions, in the paragraph 4.4, all the model limitations have been included with a special section about the geo-hydrological parameterization issues we have experienced in this work.

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".

In the results, we have fixed this part making the description much clearer than the previous revision. See also all the Figures 9 and 12 for more details (mainly in Chapter 3).

Main Comments:

Sediment Erosion

I don't see how the second implementation is more of a detachment-limited model: if the critical value of incipient motion is not exceeded, there is no sediment transport. In a detachment-limited model, as soon as a particle is detached from the slopes, into the river bed, it is transported away by the river. An example of reference to add for this kind of discussion could be: Davy & Lague 2009, Fluvial erosion/transport equation of landscape evolution models revisited.

We better explained it inside the dedicated paragraph (2.2.3.2) highlighting the differences between the two methods. In TL we do not directly compute the sediment balance since we calculate the amount of sediment transited (term Ts) with empirical formulae (stream power) as the maximum capacity of a reach (see empirical formulae based on local slope, water level and D50). In EL we computed the entire balance, and the transport terms now depend also on the sediment availability h solid (we considered a kinematic approach where is specified the stage-discharge relation valid for the sediment transport). Please check our reworking. The suggested reference has been added.

If this represents the change of topography or the change in sediment deposit thickness in the river bed, then I do not agree to the incorporation of the transport term Ts(t) into the equation. To my knowledge, only the deposition and erosion of sediments influence the topography/deposit thickness. I would take example on Campforts et al. (2020) for the sediment balance.

Following your suggestion, we have modified also the eq n° 3,5,6 and 16 highlighting better all the terms in this way: on the left of '=' we have temporal derivate and the advection term (the fluxes) (cupling together they make the partial derivative complete) while on the right the sink and the source of the equation. In this way we retain the balance equation is clearer. The sediment balance was intended similar to the water balance: E is the erosion term (a source term) coming from the EPM method, D is the deposition (as a sink that is a function of hsolid) while the transport term T(s) is the flux of sediment depending only on reach hydrodynamics and upstream sediment availability. The sediment balance is therefore different from the snow balance where the advection term is 0 since the snow is a static solid. Please check paragraphs 2.2.2 and the dedicated paragraph (2.2.3.2).

Figures and graphs

This figure is not super clear.

1) Use a choice of word consistent with line 425 ("reported landslide", "predicted by CRHyME", "detected unstable pixel", "surveyed landslide point"). I would use "observed" versus "modeled".

2) You use the word "buffer" for both the 8 pixels around the trigger pixel and the radii around each observed landslide. This is confusing. I would recommend calling the "pixel-buffer" an "unstable area" as you do in your figure.

3) Write the equations of the true positive and false positive rates in the figure to make the link between ROC curves and the classification scheme.

4) Add the fourth buffer zone to Fig5c)

1) Remove the black, yellow and white shadings inside the catchments and inside the red circle since they make it harder to see the topography.

Use contours only instead.

2) Put the signification of the blue and green triangles in the legend.

3) Zoom in the b) figure.

4) Highlight the outlets with red points.

5) Indicate where the Fuentes section is located.

6) Make the maps bigger.

7) Plot the river network on top of the map.

8) Only write the names that are used in the text (i.e. remove 'Teglio', 'Belviso', 'Valbondione', etc.)

These advices also apply to the Figure 8a.

All the figures (from 2 to 13) have been revised improved and changed where the information was not clear following your suggestions. Also, the captions have been rephrased in order to explain better the figure.

Lithology, cohesion and friction angle

How do you justify this? Lithology, etc.

It would make sense to also carry out this sensitivity analysis for the other catchments to be sure the friction angles aren't overestimated as well. If it doesn't take too much time , it would be nice to present the results of this sensitivity analysis in a supplementary.

Furthermore, I would call it a "calibration" rather than a "sensitivity analysis" since you did it because the original parameter set wasn't giving good results.

True.

You should add a very short description of the geology of the two study areas in the "Cases studied" section. I believe that there is a presence of karsts in the Emilia region?

Very strong assertion. Soil cohesion is often measured in the field (plasticity test, the thumb penetration test, and the pocket penetrometer test, etc.), and internal friction angles in laboratory with triaxial tests or empirically with topographic analysis...

Change and add references.

We have left out all the strong assertions you suggested to erase, and we better commented on the calibration of cohesion and friction angles parameters adding a reference to justify our choices (see result sections (pars 3.2 and 3.3). Moreover, to better explain the results between the Alps and the Apennines we have included some references to the local lithology and geomorphology, both in the case study presentation (pars 2.4) and in the results (pars 3.2 and 3.3) and discussion (par. 4.3). Please check our modifications

We thank you the reviewer for the comments and for the precious suggestions that have contributed to improving our work.

The Authors