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Discussions

Interactive comment on "Geomorphological surveys and software simulations for rock fall hazard assessment: a case study in the Italian Alps" *by* S. Devoto et al.

Anonymous Referee #3

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This paper presents an analysis of a rock fall problem that threatens the village of Cimolais, in NE Italy.

I have not found anything of specific or general interest in the paper, and the results presented. The technical and the scientific significance of the paper is very limited. There is nothing of relevance in the paper that was not know already in the literature.

Below, I motivate my negative opinion on the paper.

A significant drawback of the paper lays in the fact that not all the findings and the conclusions are supported by appropriate, robust evidence. This adds to the fact that

some of the discussions remain inconclusive, leaving the reader with the question of their significance and/or usefulness. I will make two examples, noticing that these are others.

First, Section 4.3.1 is non conclusive, with respect to the studied rock falls. It only tells the reader that, based on evidence discussed elsewhere in the literature, rainfall can trigger rock falls in the study area. But this is not new, or particularly significant in the context of the rock fall modelling performed in the paper. Further, rainfall is not considered explicitly in the hazard assessment either.

Second, the significance of section 4.3.2 is questionable. Nothing is concluded, really, and earthquake shaking is not considered in the rock fall modelling (a part from selecting an horizontal velocity to trigger the rock fall), or in the rock fall hazard assessment. So, why discussing it? Only because the area is seismic, or because earthquakes are known triggers of landslides? This is not sufficient.

For their modelling, the authors have used topographic information obtained from rather different sources, and namely (a) a high-resolution DEM obtained through a Lidar survey and (b) contour lines from topographic base maps at 1:5000 scale. Clearly, the quality of the topographic information is different, and it influences the rock fall modelling, somewhat. This is not discussed in the text. I consider this a major limitation.

It is not clear if the two different terrain data were available for the entire study area, or at least they covered the areas of the two profiles used for modelling. Where the two datasets were available for the same area, I would have expected a comparison of the modelling results. This would have strengthened the hazard results, if the modelling results were converging. Or else, it would have demonstrated the uncertainty in the modelling due to the different resolution and quality of the terrain information. This was not performed. I consider this an additional limitation of the work.

Given the inherent difficulty in determining the rock fall modelling parameters, and particularly the energy restitution coefficients, it is standard practice to consider ranges of variations for the modelling parameters. Apparently, this was performed (a) given the information listed in Table 3, and (b) given that the authors state that they have adopted a "probabilistic method (Frattini et al., 2008)". However, there are a number of different ways of using "probabilistic methods" for rock fall modelling, and it is not clear what the authors have really done in their work. The modelling uncertainty resulting from the range of values listed in Table 3 is not given, or discussed. This does not allow a reader to understand the level of uncertainly associated to the final rock fall modelling, which influences the hazard assessment, necessarily. This is a severe limitation of the rock fall modelling.

Section 5, Rock fall propagation modelling, reads like the simple, straightforward application of a software tutorial. Experience in rock fall numerical modelling tells me that there is more (much more!) than the application of a tutorial to a reliable numerical modelling. This section is central to the paper, and to the discussion. It is also central to the evaluation of rock fall hazard in the study area. However, the section is unsatisfactory, as it does not provide sufficient information on what was done, what were the modelling uncertainties, and the actions taken to consider them, if any. Reading this section of the text I got the impression that the authors consider rock fall modelling using their software code a straightforward numerical operation, and I strongly disagree with this approach.

On this same line, it is really not clear how the initial geomorphological assessment and the numerical rock fall propagation modelling were combined to determine rock fall hazard for the village of Cimolais. This, surprisingly, was not explained in the text. The numerical modelling of the rock fall trajectories along just two pre-defined cross sections cannot be considered sufficient to determine the full extent of the rock fall hazard in any area, not only for the village of Cimolais.

As a final general comment, and reiterating what I have written before, it is not at all clear to me what a reader would learn from this paper that was not known already from the literature on rock fall modelling. In other words, what is the innovation of this work?

Initially, I thought that the innovation was in the integration of geomorphological information analysis and the results of rock fall propagation numerical modelling. However, this is clearly not the case. Or at least it does not emerge from the paper.

Below, I list a few other comments related to specific sentences in the text.

Page 7330, Line 21. "hybrid computer program" What is this? This should be explained to readers unfamiliar to rock fall modelling. Also, it is the modelling approach that is "hybrid", not the code!

Page 7330, Line 27. "to immediately reduce the landslide risk." And how this was accomplished? I am not sure this is discussed in the text, a part from information on defensive structures shown in Figure 11, and not discusses in the text. Then, why talking about it in the Abstract?

Page 7331, Line 16. "often". "Often" refers to "time", not "places". I presume the authors mean "locally".

Page 7332-3. An awkward way of writing a paper, with a list of separated sentences instead of coherent paragraphs. This part of the text is way too long, and contains information that is not useful or necessary to understand the paper and its content. Consider cutting this part of the text significantly.

Page 7334, line 6. "traditional surveys"? What are these surveys? What does it mean "traditional"? Are they old fashion methods, or consolidated methods?

Page 7334, line 20-25. One should first decide on the criteria, and next use the criteria to determine the hazard. It seems to me that the authors have done the opposite. The fact that the local inhabitants are aware of the presence of the rock blocks at the top of the mountain does not say anything about its instability, really.

Page 7335, line 7. "their stability conditions are precarious." What does this mean, and how do you know this? The statement is very loose, and unsupported by evidence.

Page 7335, line 25. Marcato et al. 2006. Is this the right reference to support climate condition in the study area?

Page 7335/6, The section on Climate is way too long, and the information is not really used in the modelling or the hazard assessment. The same comment applies somewhat to sections 4.2 and 4.3.

Page 7337, lines 1-4. How is this relevant?

Page 7337, lines 5-14. This is all fine, but what about you study area?

Page 7337, lines 19-20 “provides a first indication about the amount of rainfall that can trigger rock fall events.” This is unclear. What does it mean?

Page 7337-8-9 The relevance of sections 4.3.1 and 4.3.2 is uncertain. Consider reducing the text significantly, or cancel the sections.

Page 7339, line 20 “a direct correlation between slope-failure phenomena and regional earthquakes is not evident but probable”. What does this mean, exactly? Be clear and specific.

Page 7340, lines 9-10 “The three modes of block motions are free fall through the air, bouncing or rolling over the slope topography (Dorren, 2003).” Although the sketch is taken from the literature, it is very simplistic and to some extent imprecise. Rock boulders very rarely (i.e., never) “roll”. A weal rolls on the ground, as during motion it always touches the ground. A boulder almost never proceeds by rolling, but through a series of repeated and short bounces. This behaviour is modelled as “rolling”, but from a physical point of view this is not correct.

Page 7340, lines 20-21 “The vegetation cover and the surface roughness determine the tangential coefficient of restitution, whereas the normal coefficient of restitution depends on the elasticity of the surface material.” This is partially correct. Surface roughness also influences the normal coefficient of restitution. The two components “normal” and “tangential” should be considered together, and not separately, in their

description.

Page 7341, lines 9-14. What is the rationale for the selection of these two specific profiles, and not other profiles? Why two and not e.g., one or three, or four, or more?

Page 7341, lines 15-27. How reliable is this modelling? How sensitive is the modelling to the decisions made here? This is a key point worth investigating.

Page 7342, line 25. “In total, 80 % of the samples stop within the vegetated talus, located northerly with respect to Cimolais.” This is not evident (or shown) in Figure 7.

Page 7343, lines 1-13. The problem with the text is that it compares profiles obtained from different topographic sources, and nothing is said e.g., on the influence of the resolution of the difference sources on the modelling results.

Page 7343, lines 17-18. “The simulated scenarios partially confirm the high rock fall risk for the northern buildings of Cimolais Village.” Well, this is not really proved by the analysis performed, and by the results shown.

Page 7344, lines 1-5. Again, awkward way of writing with separate sentences instead of paragraphs.

In many of the Figures the text used is very (too) small, and difficult to read.

Figure 3 and Figure 4. Consider using bar charts, which are more appropriate than line plots to show rainfall information. Text is very small and difficult to read.

Figure 6. This is very difficult to read. Consider using colours. Why using the big arrows to show a drainage line? Consider a simple blue line.

Figures 9A and 10A. Do the figures show that ALL blocks (10A) or most blocks (9A) stopped at the same location? This is not really very indicative for hazard assessment, and may indicate a possible problem with the propagation model.

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