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Comment on nhess-2020-411

Fabian Walter (Referee)

Referee comment on "Debris flow velocity and volume estimations based on seismic data" by Andreas Schimmel et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-411-RC2, 2021

In their submission, Schimmel et al. test relationships (some of them empirical), which compare metrics of seismic measurements near a torrent to debris flow velocities and volumes. Similar calculations have been made in the past, but this study uses data from different torrents, which provides valuable insights for potential alarm and monitoring systems.

The scope of the study falls into seismic monitoring of Alpine mass movements, which is currently an active research field. So I expect this study to be met with interest within the journal's readership. The study is more on the technical side, which is OK given the journal scope, although I suggest some more discussion in terms of physical mechanisms as I outline below. Moreover, the details of the documented calculations are unclear and should be rewritten, especially since they constitute the paper core. The manuscript is concise, structured and easy to follow. However, the English contains numerous grammar mistakes and has to be revised before the paper can be published. Overall my I recommend major revisions.

Fabian Walter.

MAIN COMMENTS

The velocity calculations are not well described. Which three different sliding windows do the authors refer to? What is the relation between minimum and maximum amplitudes? A ratio? How can the number of samples be equal to some distance? Which distance?

Distance in which unit? What is a "significant signal shape"? In its current state, it is not possible for a reader to use the explanations to reproduce the calculations.

I may have missed this, but how are the ground-truth debris flow volumes calculated, which the seismically-derived values are compared to? I was surprised that the authors do not discuss Schimmel et al. (2018), who use seismic and infrasound data to calculate discharge and estimate debris flow volumes. Is the current technique an improvement compared to this previously suggested one?

Except for a small part of the discussion, the authors give no explanations on the physics behind debris flow seismicity. The cited papers by Lai et al. (2018) and Farin et al. (2019) make specific predictions between seismic signature and debris flow velocities, grain size distributions and other parameters. Even if the authors do not want to dive into details, they should use these theoretical assertions to offer explanations for their observed volume scaling.

In several parts of the manuscript, the authors refer to the turbulent flow front. They need to provide evidence that their flow fronts were indeed turbulent and that this explains their observed signals. Some video or still footage could serve this purpose. Alternatively, I would expect that boulders in the flow front cause a distinct seismic signature compared to the flow tail. In a recent paper (Zhen et al., 2020, in GRL), we showed how the flow front's seismic signature is dominated by ground impacts of the largest boulders.

Finally, Figures 8 and 9 should include error bars or at least some short discussion on uncertainties should be offered.

OTHER COMMENTS

Line 18: "feasible" should be deleted, as it is implied by "effective".

Lines 24ff: What are the physical concepts these velocity estimates?

In several instances, the authors use the word "magnitude". If this is synonymous with "volume", then use the latter, only.

Lines 35ff: Here it seems that the authors argue that mass could be estimated with the Coviello et al. (2019) approach, but volume is poorly constrained. The difference between the two is the factor density. Why is this so poorly constrained?

Lines 40ff and elsewhere: Avoid 1-sentence paragraphs.

2 Methods: It would be interesting to see rough numbers of debris flows per year for the different sites.

Line 113: this peak discharge seems rather high.

Line 126: Velocity measurements around 2500 s in Figure 6 do not seem "consistent" as asserted in the text.

Lines 142-143: "can be an useful tool to analyze the flow behavior" This statement is trivial.

Line 148: "permitting to avoid wrong correlation results" unclear

Line 152: It is not clear how longer distances offer better resolution (resolution should be lower …?).

Line 155: "so that the cross-correlation offers useful results" You should be more specific here.

Line 159: "determine problems for the cross-correlation analysis" is unclear.

Line 160: "exaggerated averaging" is unclear.

Line 164: I do not understand how the authors arrive at granularity here. This should be explained.

Line 169 and elsewhere: Is “process” synonymous with “debris flow”? If so, only use the latter.

Lines 166-168: This sentence needs a reference.

Line 174: “velocity measured by the radar is often lower ...” needs a reference.

Lines 184-185: I am not sure that the volume estimate would always come in too late. It should be OK if the measurements were made high up in the catchment.

Line 193 and elsewhere: “sediment concentration” Do you mean “grain size distribution”?

Line 193: “calculation of the magnitude” of what?

Lines 200-201: “among the different methods deployed and the different catchments” be more specific.

Lines 203-204: “but still further research on different ...” this is an unnecessarily generic statement. Why exactly is more research needed?

Lines 206-209: I suggest discussing Zhen et al. (2020) in the context of this sentence earlier in the manuscript.

FIGURES

Captions of Figures 5, 6 and 7: Rewrite so that site name appears in the first sentence of each caption and so that it is clear which “two geophones” are meant.