

Nat. Hazards Earth Syst. Sci. Discuss., author comment AC3  
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## Reply on RC2

Andreas Schimmel et al.

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Author 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-AC3>, 2021

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### 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.*

The reviewer is right, the explanations provided are too succinct. This section will be expanded to present a clear flow chart on the different steps adopted, and the motivations for the window size used will be argued with numeric examples as well as with a figure to illustrate graphically the rationale.

- *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?*

The volumes are calculated by means of different techniques at the three test sites (e.g., topographic surveys, stage sensor measurements integrated in time), flow height measurements or 2-D laser scans and velocity measurements. We will add some information on that point in the text and we will add one column with the reference (i.e., the paper/report used as source of volume data). In the discussion, we will add a comparison of volume estimations presented here with results from Schimmel et al. (2018) only based on infrasound data.

- *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.*

We will add some additional discussion of our experimental results in comparison with the

theoretical predictions proposed by the mentioned papers.

- *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.*

We agree, we will use the video footage of a few selected events to support the interpretation of the effect of flow turbulence. Of course, the effect of coarse front vs liquid tail on the seismic signal is an important point, we will expand the discussion on that also considering recent the paper by Zhen et al.

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

The reviewer's suggestion is highly valuable, and we will add the error bars in the revised version of these figures.

#### OTHER COMMENTS

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

OK

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

Volume will be used throughout the ms.

- *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?*

Yes, the mass could be estimated with the Coviello et al. (2019) approach but the volume is poorly constrained because of the lack of the variability of sediment concentration, which is not measured. We will reword these lines providing more information on the uncertainties of the methods.

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

Ok

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

Ok, numbers can be added.

- *Line 113: this peak discharge seems rather high.*

That's the data I got from BOKU.

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

I think they are in an acceptable range.

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

Ok

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

This can avoid wrong correlation results since the possibility to have significant signal patterns for the cross-correlation increases with the window length. We will reword this.

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

Ok, wrong wording, will be rewritten.

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

Like a correlation coefficient above 0.8 - will be specified.

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

Correlation of wrong signal patterns. Will be clarified.

- *Line 160: "exaggerated averaging" is unclear.*

averaging of surges

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

Flow regimes have been characterized using the video of the events. This will be explained in the text.

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

Yes it is, and we will use the latter only as suggested.

- *Lines 166-168: This sentence needs a reference.*
- *Line 174: "velocity measured by the radar is often lower ..." needs a reference.*

We understand the reviewer's request, and we will add a reference as requested

- *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.*

Ok, depending on the site and application. We will discuss, that an installation in the upper catchment might can offer a volume estimation in time, but there are high uncertainties, while installations in the lower part offers more accurate results, but come late.

- *Line 193 and elsewhere: "sediment concentration" Do you mean "grain size distribution"?*

Yes, actually we did, it was a mistake. Thanks for highlighting this.

- *Line 193: "calculation of the magnitude" of what?*

We will use "calculation of debris flow volume"

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

This sentence will be rewritten.

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

The reviewer is right, and this statement will be removed.

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

Good suggestion, we will anticipate the reference to this paper in the introduction of the revised ms.

#### 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.*

OK