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Discussions

Interactive comment on "Analysis of the ground vibration produced by debris flows and other torrential processes at the Rebaixader monitoring site (Central Pyrenees, Spain)" *by* C. Abancó et al.

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Response to Review of Manuscript from Dr. Massimo Arattano

We would like to thank Dr. Massimo Arattano for his very valuable and useful comments, which strongly helped to improve the paper. In the following, we will answer in detail his remarks and describe how the manuscript improved according to his suggestions and comments.

1. Dr. Massimo Arattano (in the following MA): Since the author stress the potential use of their data also for warning systems, I would like if they provided at least some

indication on how to diminish the number of false alarms that they got. Apart from the malfunctioning of one sensor, there were 126 triggers due to small mass movements at the lower part of the scarp area, that did not progress downstream (see pag. 11). The authors state to have observed this during periodic field reconnaissance carried out, which indicated that no apparent geomorphic changes occurred in the channel reach after some of these triggers. The authors actually specify at pag. 18 that the values of GVth and EMth (now Ethd) should be defined for each specific geophone, according to its placement and assembly and that this calibration has a crucial importance for warning systems. They write that since in the Rebaixader site the installation was intended to research purposes, the thresholds have been maintained constant and low for all the geophones. But what could be done to avoid these triggers if a warning should be issued? What is the suggestion of the authors (see also point 2 here below)?

Authors (A): We would like to stress some considerations on the application of IS technique for warning purposes. We agree with MA on that point that the detection threshold should be adjusted in order to avoid false alarms in a future warning system. In the Rebaixader site, the false events (including all the system triggers, which were not caused by torrential events) have mostly 3 different origins:

a) Malfunctions of the system: these false events may have been avoided with more frequent maintenance work

b) Small mass movements (in the scarp area): these false events could be avoided by including additional sensors that may give another type of information (e.g.: ultrasonic or radar devices) in the triggering algorithm or combining several geophones at different placements along the channel reach. This would help to verify if the mass movement is propagating downstream. We refer to the Illgraben site for more information on this aspect (Badoux et al., 2009).

c) High runoff episodes that don't develop into a debris flood or debris flow: this type of false events is the most difficult ones to minimize. In addition, false events should be avoided by a better calibration of the Detection Threshold (Dth). Regarding the three parameters of the Dth, the following aspects could be taken into account:

- Increasing the GVth-value would induce a loss of information of the event. There is also a risk of missing an event, if GVth is too high (see Abancó et al., 2012)

- Increasing the EMthdur (Ethd in the new ms)–value has been proved to be useful according our experience. The value of EMthdur (now Ethd) was increased in 2011 from 1 sec (early versions of the system) to 3 sec, providing a loss of false triggers from 232 (2010) into 37 ones (2011).

- Increasing the EMthimp/sec (now Ethi)–value may be useful, especially for the lower geophone (Geo4), which has shown promising results to identify debris flows (see Hürlimann et al., 2013).

In the author's opinion, although the seismic detection of debris flows have advantages over other types of sensors (Kurihara et al., 2007: Study on method of setting threshold of ground vibration sensor for detecting debris flow, 4th Int. Conf. on Debris-Flow Hazards Mitigation, 603–611), the best combination for a warning system would be to crosscheck the detection information between several geophones or/and between different types of sensors (not only geophones). Another option would be the better calibration of EMth (now Eth), which could provide valuable results. However, this last option would require a longer time of calibration and testing. New sentences were included in the revised ms: "For the same reason, a too high value of GVth could induce a loss of an event, which would be fatal for an alarm system. In order to avoid the false alarms, the option would be to verify the propagation of the flowing mass by cross-checking different geophones." **

2. MA: At pag. 19 the authors actually propose that the best configuration at the Rebaixader site,for the detection including small events, would be a GVth from 0.1 to 0.2mms−1; an EMthIMP (now Ethi) s−1 of 10 and an EMthdur (now Ethd) of 3–

5 s for the geophones with box. In contrast, a GVth of 0.005–0.03mms−1 and the same EMth (now Eth) parameters are proposed for the geophones directly fixed at bedrock. Considering that the most important factor in Dth is the GVth, a range of 0.005–0.03mms−1 is quite high, almost one order of magnitude. How could it be safely chosen the value for a geophone fixed at bedrock? By trial and error like they did in their torrent? If this is the author thought or suggestion I would like to see it clearly specified at pag. 19, right after their indications. This would mean that a warning systems based on these ideas would require the presence of an expert not only to suggest the value of the parameters but also to test them in time. Any warning system of this time would thus require a period of testing before being ready to work.

A: We agree with MA that the given ranges of values may be a bit imprecise. However, the purpose of this section was to demonstrate the importance and sensibility regarding a proper definition of the parameters values of Dth. Since the objective of the paper was not to define the values for a warning system, the ranges were roughly defined, always considering that the Rebaixader is a scientific test site. It should be emphasized that the detection threshold for separating torrential events (true events) and non-torrential events (false events) depends on the local conditions of each geophone, as it could be expected and as it was also found in the study site. The values of Dth parameters are given in the manuscript as a range to include his variability. We added two sentences in the ms to clarify that. In our opinion, the values of the three Dth parameters should be tested and refined applying the two following methods: a) a calibration of the parameters in the field during a testing period of the system (including additional field tests), or b) a detailed sensibility analysis of the three parameters applied over events recorded in FLOW-SPI and transformed into impulses using different values(for this option, a greater database of events recorded in FLOW-SPI station should be available). In our study, the assessment of events (false or true) was done after the events occurrence (a posteriori). As the reviewer points out, the implementation of a warning system will require defining of alarm thresholds (e.g. using the methods indicated above) and a testing period. Of course, warnings (or alarms) given by the sys-

tem should be based on an automated expert system. Such a system needs a large database to be developed, trained and checked for its predictive capability. New text has been included: "For the implementation of an alarm system in the future, all these threshold values should be tested applying the following methods: a) a calibration of the parameters in the field during a testing period of the system (including additional field tests), or b) a detailed sensibility analysis of the three parameters applied over events recorded in FLOW-SPI and transformed into impulses using different values (for this option, a greater database of events recorded in FLOW-SPI station should be available)."
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3. MA: Would there be any chance to improve the trigger and reduce the false alarms using two geophones instead of one? Thus requiring that a certain threshold were reached on two sensor instead that only on one? Would this introduce any difficulties or risk to lose events according to authors viewpoint and experience? Are there other suggestions to improve the triggers?

A: We very much appreciate this idea. As it was mentioned above, we think that the threshold for triggering the event mode of the system (i.e. Dth) should be distinguished from an eventual alarm threshold. The system trigger must be carried out using a detection threshold low enough to record all the potential true events, although some false events (not false alarms) could be recorded. This condition is more efficiently fulfilled, when the system is triggered by any of the geophones of the network. Such a trigger could be used for a first warning (a pre-alarm level). We agree with MA that the combination of different sensors is a promising option in a warning system to activate the alarm level and to avoid false alarms. The sensors could be of different type (such as: flow depth sensors, pressure sensors, etc) or only geophones. In the Rebaixader, two groups of geophones may be separated:

- Upper geophones (Geo1 and Geo2)

- Lower geophones (Geo3 and Geo4)

However, in order to get the maximum reliability, it should be required that the two groups would exceed the Dth conditions. However, the main drawback of such a solution is the fact that the alarm would be activated later, and therefore the response time for the alarm messages or actions would be shorter. This composition of the trigger would not apparently induce risk of losing events, since the reliability of Geo4 is demonstrated (Hürlimann et al., 2013). Other higher values for an alarm threshold, or a more complex combination of sensors specifically could be also used for triggering several increasing levels of alarm.
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4. MA: The authors recognize the presence of three different shapes of the IS time series curves (type A, B and C). The shape of the time series has been recognised by them as one of the key parameters to identify events and to distinguish between different types of torrential processes. However when they analyse the data obtained at station FLOW-SPI, where they have the data recorded at 250 Hz, in order to analyse the recordings they identify 4 different phases (P0, P1, P2 and P3). I do not understand why the authors did not use the previous classification, transforming the signal in IS and then trying to recognize if the output belonged to one of the three IS time series curves (type A, B and C). On the contrary their distinction in 4 phases appear very subjective and so quite arbitrary. The difference in fig. 4 among the three different shapes is crystal clear. Which is the real difference in fig. 6a between P2 and P3, for instance? Where is the limit between the two? On the contrary in fig. 6b I would have put a P1 quite easily . . . I think that this distinction of 4 phases is somehow unnecessary.

A: The complete section has been rewritten in the new ms. New graphs have been included to Figure 6. A frequency analysis is presented in the new ms, and the characterization of the events from FLOW-SPI station is also based on this information. The frequency analysis is carried out by temporal windows in order to observe the evolution of the frequency content over time. The results show that the differences of the different phases are visible in the frequency domain and may

be useful to delimitate the phases of the event apart from the time domain series.
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5. MA: By the way, the authors then transform the signal in IS for geophone 5 and show the results in fig. 8. Confronting fig. 8d, however, with fig. 5h it is a little bit difficult to recognize the same event. The shape is different and so is the number of IS. For instance in fig. 5 h, about 220 sec after the first, main front there is a surge (followed by a smaller one) that is much smaller than the first front (smaller means with a smaller number of IS). In fig. 8d that surge appear even higher than the main front. Why? Do the authors have any explanation?

A: The differences in shape and number of IMP/sec vary from one geophone to another. The local site effects and the position of the geophones along the channel are responsible for these variations. Geophone Geo5 is placed higher in the channel, in relation to geophone Geo4. Geo3 is actually at almost the same cross section of Geo5. In Figure 8a and Figure 8d, the first and the second peak are relatively similar one to each other. In contrast, at Geo4 (Figure 5h), the second peak is smaller than the first. This could be attributed to an evolution of the flow along the channel reach. The following text has been included in the ms to clarify this point: "Finally, the influence of the distance from the source can be noticed by comparing Figure 8d (Geo5) with Figure 5h (Geo4). For the same event, at Geo4 two different waves can be recognized, while they are not visible in Geo5. This can be explained by the evolution of the flow along the channel while it travels downstream."
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6. MA: This leads to the following observation. At pag. 12 the authors state that the video images and geomorphological reconnaissance clearly showed that A-curves were recorded during debris-flow events (Fig. 5b, d, f and h). However, only Geo4 recorded A-curves for all the debris flows. The time series recorded at the upper geophones show other types of curves, different than A-curve, especially during the "small-magnitude" debris flows (Fig. 5a and e). The authors interpret that only debris

flows generate A-curves, but only when the flow reach the location of Geo4 debris flows are fully developed, showing a well-defined front. Then the authors observe that geophones 1–3 are located at greater distances from the active channel (15–25 m) than Geo4 (8 m) and the attenuation of the vibration with distance may probably play a role in the recordings of debris flows by geophones more distant from the flow path. I refer the authors to a paper of mine where it is discussed the possible absence of a well developed front before the debris flow has flowed a certain length in the channel and reached a certain position in it: Arattano M. (2003) Monitoring the presence of the debris flow front and its velocity through ground vibration detectors. Proc. 3rd International Conference on Debris-flow Hazard Mitigation: Mechanics, Prediction and Assessment, Millpress, Rotterdam: 719-730. This paper might give some ideas.

A: We thank MA for his recommendation. We read the paper and we found it very interesting. The observations of MA in the Moscardo torrent fit with the observations in the Rebaixader torrent. We included the reference in the ms. "This interpretation is supported by the observations of Arattano, (2003) in Moscardo, where in some events the proper debris-flow front was only visible downstream of the fan apex."
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7. MA: Could this latter observation also explain somehow the differences of shape of fig. 5h and 8d? Was it due to the change of the wave as it moves along the channel (see point 5 above)?

A: We think that this may be exactly the explanation that we mentioned in point 5 above.
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8. MA: In fig. 5 the scales of the ordinates of the different graphs are almost all different and this may be misleading. At least the graphs that appear side by side should have the same scale. Otherwise the reader might be induced in misunderstandings. In fact I was, at first.

A: We thank MA for this observation and we

adapted this graphs according to his recommendation.
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9. MA: It is my understanding that the electronic conditioning circuit board that is connected to each geophone and performs the signal transformation, operates analogically. That is, the board does not first digitalize the signal at a certain frequency and then performs the IS calculation. It sorts of "listen" to the signal and detect when it gets greater than the fixed threshold. In other words it does not have a sampling frequency (like the 250 Hz sampling frequency of the station FLOW-SPI). So the board could be used to calculate the IS also for a signal of, let's say, 1 KHz or even more. Am I right? If this is the case it should be emphasized, because it might not be clear at a first glance. I was drawn to this conclusion by the observation of the sometimes exceptionally high value of impulses measured (more than 250) that would require a sampling frequency of at least 500 Hz to be detected (for the Nyquist rule).

A: The understanding of MA about the board operation system is correct. We emphasized this explanation in the revised ms in order to avoid possible misunderstandings. We included the following text: "The signal transformation consist of, first, a filtering of the original voltage delivered by the geophone to remove low ground velocities, which are assumed to correspond to seismic noise of the site, and, second, a transformation of the voltage exceeding a certain threshold into an impulses signal. (...) After this filtering, the signal is transformed into an impulses signal by the conditioning circuit (for further details, see Abancó et al. 2012). The signal is sent to the datalogger, which counts the number of impulses each second."
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10. MA: Finally I could not find Table 2 mentioned in the text. Please put some ref. in the text about table 2. How were calculated the volumes shown in that table? Were they estimated, measured, surveyed?

A: We added the reference to Table 2 in the text and also improved the

caption of Table 2 into: "Table 2: Characteristics of the events analysed in this work. Volumes were estimated using the sensors (geophones, ultrasonic device, the video-camera) and field observations."
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