Comments to Abancò et al.

The paper presents some new field data on the seismic monitoring of debris flows recorded in a Spanish catchment. As the authors point out the correct interpretation of the seismic signals produced by debris flows still presents many uncertainties, so this contribution is certainly of some interest for the scientific community and worth of being published.

Some comments/suggestions follow:

1. 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 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 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)?
2. 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 $EMth_{IMP\ s^{-1}}$ of 10 and an $EMth_{dur}$ of 3–5 s for the geophones with box. In contrast, a GVth of 0.005–0.03mms^{-1} and the same EMth 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.
3. 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?
4. 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.
5. 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?

6. 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.
7. 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)?
8. 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.
9. 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).
10. 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?