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Interactive comment on "Towards a standard typology of endogenous landslide seismic sources" *by* Floriane Provost et al.

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The paper by Provost et al. proposes a classification of landslide-induced seismic signals detected at close distances (< 1 km) based on data gathered in 14 field sites. A standard classification of seismic sources generated by mass movements detected at local scale is definitively of interest for the scientific community dealing with environmental seismology. However, it is not clear if this paper is a research or review article. A collection of study sites and already published seismic data is presented but a systematic testing of the proposed classification is missing. I would suggest the authors to choose one of these two possible formats, research or review article, and to consequently reshape the manuscript. I recommend the paper for publication in Earth Surf. Dynam. after major revisions.

Major comments:

I think that the abstract needs a significant rewording. The first lines sound like an introduction on environmental seismology. Please focus more on objectives, methods and results of your work. In addition, I disagree with the statement "The seismic networks installed on these sites are roughly similar (i.e. sensor, network geometry)". What does "roughly similar" mean? There is a significant difference between a BB seismic network installed on a large, slow moving earth-flow and a linear array of geophones deployed along a debris flow channel. Moreover, if the authors are focusing on "seismic events detected at close distances (< 1 km)" the sensor network characteristics and geometry, as well as the geological and geomorphological contexts, have a strong impact on the recorded signals.

To achieve a standard source characterization, in my opinion there are three major topics that would need to be addressed: i) distance sensor-source, ii) typology of sensor, iii) sensor installation methods. Given the pretty ambitious title, I would expect some discussion of their effects on landslide sources. However, I have the impression that the paper leaves more open questions than clear responses. In the following more details on how these three aspects have not been adequately addressed are given.

i) The authors briefly touch this point in the discussion: "The differences in the frequency content of simple slopequakes may be explained either by the attenuation of the high frequency at large distances during the propagation or by different rupture velocity and/or the presence of fluid in the fault plane". I encourage them to stress more on the possible limitations of a spectral analysis to be employed in a general classification. For instance, consider what was already published about the effect of the sensor-source distance on the seismic signal produced by flow processes (Gimbert et al., 2014; Schmandt et al., 2013). ii) At P7 L5-6 the authors state "The relatively low energy released by the landslide related sources makes the choice of the seismological instruments to deploy very important". I agree, and I think that this point should be developed more. Section 3.1 describes the main classes of sensors employed for the detection of mass movements but I do not see a proper discussion of this point when the authors present their dataset. iii) Considering flow detection at channel scale, the sensor installation method has a strong impact on the features of the recorded signal, both in amplitude (e.g., Coviello et al., 2015) and frequency domain (e.g., supplementary material of Kean et al., 2015). Again, this issue is shortly introduced at P3 L11 “The location of the sensors and the type of waveguide are also critical to capture the slope behavior” but a discussion based on the analyzed dataset is missing.

Standardized datasets and field experiments are probably needed to systematically address those topics. I am skeptical about the possibility to develop a standardized source-mechanisms characterization of landslide-induced seismic signals from a collection of heterogeneous case studies.

Additional comments:

P2 L5: references needed

P2 from L26: concerning repeaters, I would suggest to the authors to read the reviews of the paper by SchoÌĹpa et al. (2017), an interesting discussion is made there on this point

P3 L16: “low frequency ranges (1-500 Hz)”, why do you define this pretty broad frequency range as low? Compared to what?

P4 L30: “13 monitored sites”, 13 or 14?

P4 L33-35: concerning “we first discuss all the physical processes that occur on landslides… We further present the seismologically-instrumented landslides in the world… Then we establish a classification scheme”, I suggest the authors to rephrase in order to be more realistic. I think that the main physical processes were discussed, that only a few (14) of the seismologically-instrumented landslides in the world were presented and that a possible classification scheme was proposed.

P8 L13-24: these lines sound more as part of introduction than data. The paragraph

data should start from current L25 but a description of the analyzed dataset is actually missing: which is the length of the analyzed time series? How many events did you analyze? How did you select the events analyzed in the following paragraphs? I guess those were well-known events, or did you applied an automatic detection methods?

P8 L26: “For all sites, the instruments are deployed close to the landslide”, what does “close” mean? Please be more specific. I guess that authors agree that, for example, two seismic sensors, one installed at 10 m and another one at 900 m from the very same landslide, would record signals pretty different, especially in terms of spectral content.

P15 L14: “The signals present significant differences with the chosen features”, please reword, the reader does not understand the meaning of this sentence.

P15 L15: “in the field, the differentiation…” I am not sure to have correctly understood, maybe you meant “only from the seismic signal analysis, the difference between…”?

P17 L23: please avoid references that are not published work, i.e. Helmstetter et al. (2017a), especially if the reference is used to support a very strong statement such as “the high correlation between the repetitive events could only be explained by stick-slip movement of the locked section(s)”. A sentence like this must be accompanied by supporting data or published results.

P17 L29: concerning “most collapses occurred without precursory sequences (Allstadt et al., 2017)”, I would suggest tuning down this statement, which is also in contradiction with P2 L24. There are a number of cases where precursory seismic signals related to small rockfalls were documented, especially when a station is installed nearby the slope or there is a local monitoring network. On the contrary, when the closest station is distant or we do not dispose of other monitoring data, recognizing those precursory events is difficult but potentially there are. I also believe that the reference Allstadt et al. (2017) is not consistent here

P18 L16-20: I do agree with “several descriptions of the seismic sources are proposed for each study case” and a standard classification would help to discuss and compare landslide-induced seismic signals. I understand that the authors are proposing their classification as general reference, but I would suggest to the authors to delete the sentence “we encourage future studies to use and possibly enrich the proposed typology”. In my opinion the scientific community does not need to be encouraged to adopt one classification or another. By the way, why you do not adopt the classification proposed by Allstadt et al. (2017)?

P18 L25-27: reference needed

Reference list: the style in not consistent with the journal guidelines, in many cases the doi is missing, there are repeated references (Hibert et al., 2014a), others are missing (Provost et al., 2018) and there is some text here and there probably out of place (e.g., P29 L10-11). An accurate revision of the reference list is needed. Moreover, I do acknowledge the significant contribution of some of the authors to the field but I have the impression that self-citations are really abundant (five papers by Hibert et al., six by Helmstetter et al.). Please try to select your most significant works and refer to them.

Figure 1: I would prefer the author to focus more on the sites from which they present some data instead of showing a collection of points in a global map. In addition, Figure 1 is redundant if one considers the list presented in Table 1

Table 1: some details/revisions are needed. 7 Alestch-Moosfluh: this landslide is also monitored with a geophone network (Manconi and Coviello, 2018); 8 Torgiovannetto, Assise: please modify in Assisi; 15 Aiguilles: Aiguilles Pas de l’Ours?; 22 US highway 50, CA: there is no reference/website about that?; 24 Millcoma Meander, Oregon: same as above; 33 Matterhorn peak/Mont Cervin: please use the international name (Matterhorn) or the Italian one (Cervino) and add the reference describing the more recent monitoring network (Occhiena et al., 2012); 48 Piton de la Fournaise caldeira: Piton de la Fournaise is not enough?; 53 Marderello torrent: the reference for this net-

work is Coviello et al. (2015); 69 La Colima volcano: please use the international name (Colima Volcano) or the Mexican one (Volcán de Colima); 70 Merapi volcano flanks: please use Merapi volcano, be consistent with the list format; in addition, a number of sites are missing, especially overseas in USA (e.g., Kean et al., 2015), New Zeland (e.g., Lube et al., 2012), and South America (e.g., Kumagai et al., 2009; Worni et al., 2012).

Figure 2: what about adding a sketch of the signal associated to each process?

Figure 13: I guess this is the most important figure of the paper, why does it only appear in the discussion? Given the large seismic dataset I suppose you have at your disposal, why did you plot only between 2 (most of the cases) and 6 (few cases) examples? I wonder if the variability of the attribute shapes is representative given limited number of examples here presented.

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