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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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Dear Editor,

We thank warmly Dr. Velio Coviello and an anonymous referee for their in depth lecture and their many thoughtful and constructive comments. We propose below detailed answers, thoughts and clarification concerning the main points of interrogations of both referees. For clarity, redundant comments of both reviewers and technical/typos comments have been removed or just indicated as OK in the letter.

Sincerely,
Floriane Provost on behalf of all co-authors,

NOTE: In the following document, the referee comments are in normal fonts and the answers are in blue font.

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Reviewer 1: Dr. Velio Coviello

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.
Thanks. We have rewritten part of the abstract in order to state more the focus of the 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.
We do not completely agree with this affirmation. We rephrase the abstract in order to precise that we analyze the signal recorded by geophones and BB seismic sensors in the same frequency band (between ca. 1 Hz and 100 Hz). We do not investigate the information recorded at lower (BB) or higher (Geophones) frequencies. In order to ease the understanding, we also propose a new table (Table 2) presenting the list and the specifications of the seismic instruments and of the seismic network geometry of

the 13 sites where data is presented and analyzed.

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.
Indeed, but this statement is true for every seismological studies. More than the distance alone it is the wavelength of the seismic waves and the source dimension compared to the recording distance that is important. We analyzed seismic networks where at least one sensor is installed on or at a very close vicinity ($<$ 50 m) to the active zone. Regarding the geological and geomorphological contexts, our assumption is that if we can observe similar signal features in different sites they can only be explained by the similarity of seismic sources.

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.
As mentioned in the previous comments, we analyzed seismic networks where at least one sensor is installed on or at a very close vicinity ($<$ 50 m) to the active zone and we also filter the signals in the same (low) frequency band to limit the influence of the wave propagation of the signal.
Concerning point ii), to compare signals from different networks the most important sensor-related properties to take into account is the instrumental response of the sensors. For each case presented in our study, we have removed the instrumental response of the recorded signals (and filtered the signal in the same frequency band (f_c to 50 Hz) recorded by every sensors in our dataset to compute quantitatively their properties).
Concerning point iii), if the reviewer means network geometry by "sensor installation"

we do not agree that the sensor installation will play an important role in the signal features. The latter is mostly controlled by the source to sensor distance (and we answer to this comment in point i)). The sensor installation will play an important role in the magnitude of completeness and in the location accuracy.
We have added in the Table 2 some information on the distance sensor-sources for each case studies in order to provide more information about the analyzed datasets. We state clearly that we do not investigate low and high frequencies (P10, lines 12 to 15). As we choose highly energetic examples for each class we do not expect a dominant impact of site effect on the features we selected and we discussed if needed be, the effect on the interpretation.

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).
We agree that spectral analysis of the seismic signals present some limitations for signal comparison but it is also the most common approach to investigate seismic datasets. Spectral analysis is used in most classification processes (automated and manual) whether it is for volcano or reservoir monitoring, local, regional or even global seismology.
It must be noted that 1) we do not only analyze the spectrum (4 over 9 of the signals properties are not directly correlated to the spectral content), 2) in order to reduce the influence of the seismic to sensor distance, the signals are filtered in the same

frequency band (< 50 Hz) before the computation of the features (this is not the case on the signal figures) and 3) we analyze the most energetic recorded signals in order to reduce the influence of the seismic geometry.
Concerning the two mentioned studies, they show that the source mechanism is a predominant factor controlling signal spectral content although the sensor to source distance plays a role in the contribution of certain frequencies to the signals amplitude. As the simplest deconvolutive model, propagation acts as a filter, but the remaining spectral content is controlled by source properties. Hence, even if we loose spectral information due to attenuation, the peculiarity of the spectrum controlled by the source mechanism is most of the time conserved. Therefore we think that including spectral features is relevant in our classification.

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.
We added a section about the seismic network deployment where we address this comment. We also modify the last paragraph of section 3.1 (P6. l32, P7. l13).

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.
Thanks. We refer to the new section “Data” introducing Table 2, and we also indicate more explicitly which sensor types are used and how they are analyzed. As mentioned previously, we corrected every sensor response and we decided to work in the f_c-50 to 100 Hz frequency band were all analyzed sensors are sensitive.

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.
We added a section about the seismic network deployment (Section 3.2) where we address this comment. More than the sensor installation geometry is the distance to the source that plays an important role in the recorded amplitude and the frequency content. We already answer about this influence in previous comments.

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.
We are a bit confused by this comment. On one hand the reviewer stresses that “standardized dataset are probably needed” but on the other hand that it is impossible to do so from a collection of case studies. Then how can one compile standardized datasets?
We believe that the compilation of case studies and the standardized processing and representation of the seismic events recorded on landslides we propose is relevant for the following reasons : 1) standardized classifications exist in other fields of micro-seismology such as in reservoir monitoring, slow earthquakes (LFE, VLFE, etc) and volcano monitoring; 2) Those standardized classifications have proven to be useful starting points for further discussions : the classification is never frozen and should evolve following new observations and models; 3) Compiling datasets from very diverse case studies allow to bring out the control of the source on the signals from each class (different media and different propagation paths but same signal characteristics at different sites = source-controlled features).

Additional comments:

P2 L5: references needed

OK. We have introduced different references for glaciers, snow avalanches and landslides.

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

We added a sentence concerning this discussion (P3. l15-20).

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

We recognize that this sentence is awkward. We meant compare to Acoustic Emission signals. The term "low frequency" has been removed for clarity.

P4 L30: "13 monitored sites", 13 or 14?

OK. The correct number of sites is 13.

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.

We have rephrased the sentence: *"Then we establish a classification scheme of the landslide seismic signals from relevant signal features based on the analysis of the datasets of 13 sites."*

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?

We added these information in the new version of the paper and in Table 2, section 4 and section 5 of the new version.

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.

We added these informations in the description of the sites and in Table 2 and section 4. We mentioned that for each seismic network analyzed, at least one sensor is installed on the active zone or at its vicinity ($<$ 50 m). Moreover, we choose to work with the most energetic trace for each recorded events that we assume to be the closest station to the source and hence, the most representative of the seismic sources properties.

Of course, the distance and the medium contributes in the features of the seismic signals and we do not decorrelate its contribution. But as mentioned in earlier answer, the source mechanism also contribute to the signals feature. We already justified our approach to limit the wave propagation influence (see earlier answers to the same comment). Basically, our assumption is (as mentioned in previous comment): different media and different propagation paths but same characteristics at different sites = source-controlled features.

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

We have rephrased the 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”?
OK. We have rephrased the sentence.

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.
We removed the mentioned reference in this sentence.

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.
OK. We agree and removed this sentence.

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.
We disagree with this statement – standard typology does exist for instance for volcano-related seismic sources or for glacier-related sources and have been very useful to progress in the comparison of the seismic signals on all volcanoes and in the creation of comparable catalogs. Though any standardization/harmonization methods can be questionable, we believe that proposing a nomenclature of sources is important for further discussions including rejecting the proposed classification or interpretation.

By the way, why you do not adopt the classification proposed by Allstadt et al. (2017)?
The classification proposed by Allstadt is not comparable to our classification as it is related to detection and cataloging landslide failures at regional scales ($>$ 1 km); the purpose of our classification is the slope scale.

P18 L25-27: reference needed
Done. We added the reference.

Reference list: the style is 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.
We corrected the style of the references taking into account the journal guidelines, and also updated the reference list.

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.
We believe the citations related to the papers of Helmstetter and Hibert are relevant.

We also added several new references to the manuscript from other research groups as proposed by reviewer # 1. We tried to be exhaustive in the references and we cite more than 130 papers (a significant number due to Table 1), in total around 18% of the citations are self-citations of the co-authors which we think is not over-abundant.

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.
We added a table gathering the informations about the analyzed sites and their seismic networks (Table 2). We removed Figure 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).
OK. Thanks for providing this detailed information. We have corrected the Figure and Table 1 accordingly.

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

We do not think this would had information at this stage of the paper. It seems to us that simple sketching cannot capture the complexity of seismic signals and that the representation we propose on figure 13 is more suited to expose this complexity.

Figure 13: I guess this is the most important figure of the paper, why does it only appear in the discussion?
This figure summarizes the presented signals properties. We do not think that an earlier presentation of this figure is necessary.

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.
We present more examples in the discussion in the new version of the paper (Figure 12) and comment the variability of the attributes in the discussion (P17. l2-15).

References

Coviello, V., Arattano, M., and Turconi, L. (2015). Detecting torrential processes from a distance with a seismic monitoring network. Natural Hazards, 78(3), 2055–2080. https://doi.org/10.1007/s11069-015-1819-2

Gimbert, F., Tsai, V. C., and Lamb, M. P. (2014). A physical model for seismic noise generation by turbulent flow in rivers. Journal of Geophysical Research: Earth Surface, 119, 2209–2238. https://doi.org/10.1002/2014JF003201

Kean, J., Coe, J., Coviello, V., Smith, J., Mccoy, S. W., and Arattano, M. (2015). Estimating rates of debris flow entrainment from ground vibrations. Geophysical Research Letters, 42(15), 6365–6372. doi:10.1002/2015GL064811

Kumagai, H., Palacios, P., Maeda, T., Castillo, D. B., and Nakano, M. (2009). Seismic tracking of lahars using tremor signals. Journal of Volcanology and Geothermal Research, 183(1–2), 112–121. doi:10.1016/j.jvolgeores.2009.03.010

Lube, G., Cronin, S. J., Manville, V., Procter, J. N., Cole, S. E., and Freundt, A. (2012). Energy growth in laharic mass flows. Geology, 40(5), 475–478. doi:doi.org/10.1130/G32818.1

Manconi, A., and Coviello, V. (2018). Evaluation of the Raspberry Shakes seismometers to monitor rock fall activity in alpine environments. Geophysical Research Abstracts, Vol. 20, EGU2018-16183.

Occhiena, C., Coviello, V., Arattano, M., Chiarle, M., Morra di Cella, U., Pirulli, M., Pogliotti, P., and Scavia, C. (2012). Analysis of microseismic signals and temperature recordings for rock slope stability investigations in high mountain areas. Natural Hazards and Earth System Science, 12(7), 2283–2298. doi:10.5194/nhess-12-2283-2012

Schmandt, B., Aster, R. C., Scherler, D., Tsai, V. C., and Karlstrom, K. (2013). Multiplefluvial processes detected by riverside seismic and infrasound monitoring of a controlled flood in the Grand Canyon. Geophysical Research Letters, 40(18), 4858–4863. doi:10.1002/grl.50953

Schöpa, A., Chao, W.-A., Lipovsky, B., Hovius, N., White, R. S., Green, R. G., and Turowski, J. M. (2017). Dynamics of the Askja caldera July 2014 landslide, Iceland, from seismic signal analysis: precursor, motion and aftermath. Earth Surf. Dynam. Discuss., in review. doi:10.5194/esurf-2017-68

Worni, R., Huggel, C., Stoffel, M., and Pulgarín, B. (2012). Challenges of modeling current very large lahars at Nevado del Huila Volcano, Colombia. Bulletin of Volcanology, 74(2), 309–324. doi:10.1007/s00445-011-0522-8

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Reviewer 2: anonymous referee

After reviewing the manuscript I read the review of Dr. Coviello. I fully agree

with his comments. I will not repeat his comments in my review. I found in the manuscript some mistakes and problems. Accordingly, I propose a major revision of the paper (if not rejected in its present form), which is in line with my comments and those of V. Coviello. The document is verbose, with a lot of information (perhaps with little consistency in terms of content) and poor in conclusions. Please, be more concise and remove the unnecessary information. Justify the purpose of the paper better. References must be selected to shorten their number.

We thank the reviewer for these statements. The introduction of the paper has been thoroughly revised to better highlight the focus of the work. We also rephrased or deleted some sentences considered as verbose by the reviewer. All these changes are indicated in track mode changes in the revised version of the manuscript.

In general, I am very skeptical about the purpose of the paper: to establish a standard typology of endogenous seismic sources.

This comment has also been addressed by Reviewer #1. We believe that the compilation of case studies and the standardized processing and representation of the seismic events recorded on landslides we propose is relevant for the following reasons : 1) standardized classifications exist in other fields of micro-seismology such as in reservoir monitoring, slow earthquakes (LFE, VLFE, etc) and volcano monitoring; 2) Those standardized classifications have proven to be useful starting points for further discussions : the classification is never frozen and should evolve following new observations and models; 3) Compiling datasets from very diverse case studies allow to bring out the control of the source on the signals from each class (different media and different propagation paths but same signal characteristics at different sites = source-controlled features).

It is true that there has been a dramatic increase of monitoring/detecting seismic signals generated by different ground phenomena in the last five years. However, we have to bear in mind that seismic measurements are not a direct measure as

they could be extensometers, for example. The terrain is so complex that I am skeptical about whether seismic monitoring could give detailed information about the phenomena.
We disagree with this statement. The arguments for using seismology, waveform analysis and analysis of the temporal and spatial distribution of seismic sources on landslides as complementary sources of information on the mechanics of the processes are:
1) The temporal resolution of seismic instruments provides very accurate timing of the deformation processes and is non-invasive as it can detect events at distance from the sensor installation. These advantages are hardly met simultaneously with other types of sensor. Several studies have demonstrated the major contribution of seismology to built near-exhaustive catalogs of events at slope scale (Helmstetter et al, 2011; Dietze et al, 2017b), at regional scale (Hammer et al, 2016) and its potential for early-warning of debris-flows (Walter et al, 2017; Arratano et al., 1999; Burtin et al., 2009).
2) It records also the spatial distribution of the sources occurring in depth (Spillmann et al, 2007, Lacroix et al, 2011, Tang et al, 2015) which is not the case of extensometers for example. The location of the seismic activity represent valuable information to update geo-mechanical models determining the factor of safety of the slope (Tang et al., 2015).
3) The seismic signal features are controlled by the source mechanism providing insights in the mechanical behavior of the deformation.
4) Recent papers have also documented seismic signatures preceding the collapse of large landslides (Amittrano et al., 2005; Yamada et al., 2016; Poli 2017; Schöpa et al., 2018) proving the presence of seismic signals associated to slope instabilities deformation.

So, seismic data alone are very difficult to manage for mass movement studies, mainly if the signals are very short and are related to small energy release. We must be aware of the type of phenomena. In my opinion, it is the combination of different

measurements that can contribute to information about the phenomena.
We agree and we never mentioned to consider seismology as a standalone technique for landslide monitoring.

For this paper, I suggest you only include the very significant seismic signals and avoid small events.
The purpose of the paper is clearly mentioned: we analyze the signals recorded at close distance to the slope ($<$ 1km) with seismic sensor sensitive to the $\tilde{1}$-100 Hz frequency band. This means that we are exploring larger events and more distant events than Acoustic Emission studies (Dixon et al, 2015; Michlmayr, 2012) but smaller events than large slope failure (volume $>$ 1O^6 m^3, Ekstrom and Stark, 2013). Obviously, the examples are “significant” signals at this scale as they are clearly above the noise level.

Mainly, because of the difficulty of a subsequent interpretation. This is in some ways one of the conclusions of the authors, given that they unify the “new named” slopequakes by including them all in one group. The slopequakes can be so complicated that the present catalogue is probably not complete.
We agree with the statement that slopequake signals can be “complicated” and “that the present catalogue is probably not complete”. However, we also propose sub-classes taking into account the complexity of this class while keeping a uniform denomination because they are usually analyzed as one class in the previous and current studies. The name “slopequake” was chosen in order to remove the source mechanisms interpretation induced by the name “slidequake” or “micro-earthquake”. As mentioned earlier, the present classification is not frozen and can be enriched and/or discussed. In particular, for certain sub-classes we explicitly mentioned that surface processes may also generate these type of signals (SQ-tremor like signals and SQ-with precursory).

All efforts in this line should be devoted to monitoring one site with different in-

struments and to interpreting the events in coordination with different specialists.
This is done on most of the recent sites being instrumented by different research groups. However, the source mechanisms and the variability of the slopequake features remain poorly documented. Understanding this variability and the underlying physical processes remains a strong challenge, and we hope that the classification we propose will bring some insights leading toward a better grasp of those processes.

Having said this, see below for further comments.

1) Table 1. This is a very risky approach. Given the present increase in mass monitored studies you probably miss one unless it is the intention of the authors only to mention those in which they are involved. In this case, you must mention this specifically, and give your reasons.
We agree that we probably missed some sites especially the new sites recently instrumented. We added the missed sites suggested by V. Coviello (4 over 70). If we are missing further references, please, let us know and we will add them to the table.

2) As regards field instrumentation, it would be useful to better explain the characteristics of the instruments and their different site conditions. Site effects are completely ignored in the interpretation/description of the signals. In fact, most of the presented data were already the subject of different interpretations and I assume that these have been described in the corresponding papers. However, when seeking to establish a standard typology, consideration of the peculiarities (or not) of the site effects is very important.
This comment was already addressed by Reviewer #1.
Concerning the field instrumentation, we propose a new section “Data” (section 4) to describe precisely the seismic network configuration (also summarized in table 2). The geomorphological and geological context are indicated in Table 1 with the references for further information.

Concerning the instruments, we corrected the instrument response and analyzed their common frequency band.
Concerning the site effects, it is true that we do not correct. However, we believe that the comparison of signals from different sites of various geomorphological and geological contexts is precisely a good strategy to discriminate the contribution of the source mechanism from the site effects/attenuation. We hence describe the features shared by all the selected examples without focusing on particular features of certain signals that are likely linked to site effects.

3) In the definition of the parameters of processing methodology, if I am not mistaken, no amplitudes are considered (only once on pag. 15 line 21). Why are the amplitudes (nm/s) not indicated in the events? It is true that attenuation can also affect amplitudes, not only the frequency content, but it could be useful for differentiating events. The relative “energy” released together with the duration of the signals can give significance to some events.
Amplitudes are indicated on the figures for each trace of each example in nm/s. We did not choose to analyze the amplitudes or Energy/duration relationships (even so, they can be significantly different from one class to the other) because we are focusing in the features that can be related to the source mechanisms and not to its magnitude.

4) Additionally, the authors devote a large description to the frequency content of the events. However, in figure 13, the maximum attention is devoted to other parameters that are basically in the time domain.
Over 9 parameters presented on figure 13, 5 are related to the frequency content and 4 to the time domain. The waveforms presented on figure 13 gather information on both time and frequency content. Moreover, we think that the format of the figure used to present examples for each classes (Figures 3 to 12) summarizes all the informations needed to discuss the signals. On Figure 13, we adopted “star” diagrams in order to ease the visualization of all the selected features and not to focus only on frequency

nor time domain properties.

Moreover, the parameters introduced in the processing methodology section are not sufficiently considered in the description of the events. Note that in the description, few of these parameters are mentioned.
OK. We reviewed these sections to add these informations.

Furthermore, the last sentence of section 4 merits a detailed explanation and challenges the classification. In this sentence, the authors mention the real problem of the dependence of the defined parameters on the source to sensor distances and on the propagation media properties.
OK. We have rewritten this part of section 4 (Section 5. in the revised manuscript) to describe our approach to analyze the datasets and compare them (P10. l21-30).

5) Pag. 11. Explanations and description of the signals are very poor. Some explanations correspond to other cases. I include my comments about the case of RF (pag. 11) only as an example.
We reviewed these sections to add further informations.

Pag. 11 Line 2. Please, indicate if the rockfall was monitored. Information specific on this event is necessary.
OK. We added information about this event on the description of the datasets.

Line 5. What does it means:. energy below 10 Hz is present for volume larger than 1 m^3 (Fig 3a). Is this your case, because you mention this figure here?
It is not only the case of this specific event. We added references to support this statement in the next paragraph: *"The frequency content is also controlled by the block mass i.e. the frequency of spectral maximum energy decreases when the block mass increases (Farin et al, 2015; Burtin et al, 2016; Huang et al, 2007".*

Line 7. The study of Farin et al., (2014) is an experiment in lab. and cannot be extrapolated to nature as it is observed (the high freq. disappear). This contribution is no relevant here.
OK. We removed this statement.

Line 9. P- and S- waves are hardly distinguishable. Is this in this specific case? You cannot generalize.
The statement is supported by different references.

Line 11. First arrivals are mainly impulsive. At the scale of representation I have to believe it.
We removed statements concerning the impulsive nature of the signals as it may vary from site to site.

Line 12. Figure 4 is incomplete. Information is required. If the signal belongs to a publication, the references must be included. Otherwise a comment is necessary.
The reference of the dataset from which the presented events are taken from are added in the caption when the presented (or similar events) have been published.

Line 13. Why do you suspect that the signals could be different if what you are recording is the movement of the mass falling down the slope? Normally, there is a time lag between ground and blast signals and the signals of the rock fall as observed in earlier publications.
We meant that natural rockfall are often composed of several falling blocks subject to break-up. In the case of the Riou-Bourdoux experiment only single block falls were monitored. It is true that in other studies when the rockfall is triggered from the rock cliff, very similar mechanisms and signals can be observed. We hence rephrased the sentence accordingly.

Lines. 18 and 20. Le Roy et al. 2017and 2018. Complete appropriately these references

OK. We have corrected the references.

Line 22. Burtin et al, 2016. This paper is devoted to torrential process. By the way, the reasoning in the outlook section is of interest.

Fig1. of Burtin et al, 2016 shows the influence of the block mass on the frequency content even so, it is not discussed in the text. We added Huang et al, 2007 that discusses the same experiment.

Line 24. You mention "[...] may be emergent due to simultaneous arrivals of the waves". Explain this better. Do you mean that it could be interference between the impulses? What happens with the wave field? It also depends of the frequency content.

OK. We rephrased the sentence as: " [...] may be emergent due to simultaneous arrivals of waves generated by impactors of different sizes impacting the ground at closely spaced time intervals".

6) Figure 13 is perhaps one of most interesting figures but it must be better explained. As I mentioned before, small events must be avoided.

We enriched the discussion of this figure (P.16 l34 to P.17 l.7). We already respond to the "small events" issue in a previous comment. Basically, we selected events clearly above the noise level.

7) Pag. 16 Line 3-4. The authors justify the differences in the frequency content mentioning attenuation because of large distances, but this is not the case here because it is indicated in the paper and in the abstract that the signals are from events at r$<$ 1 km. Is this consistent?

Attenuation is function of the distance and the wavelength of the seismic waves observed. c.f. previous response to comments of Reviewer #1. At our scale, "large distances" ranges from 100 m and more, depending on the magnitude of the source and the network geometry. One can clearly observed the influence of the distance in most the presented examples (Figures 3 to 11), even if the location of the source is not computed. Moreover, this comment is in contradiction with all the previous comments concerning the influence of the wave propagation on the recorded signals as a strong limitation of our study.

8) The term seismologically is not used correctly in the text. Replace it by seismic instruments. What does seismologically instrumented mean? In the world of seismologists the instruments are seismic instruments or not. They could have different resolution, characteristics, etc: : : "Seismologically" refers to a discipline, but not to the installation. Basically, the parameters you are considering are devoted to data processing signals and signal characteristics and not to wave transmission which is the subject of seismology. And as regards the installation, what does a non-seismologically installed seismic instrument mean?

OK. We corrected accordingly.

9) Pag. 16 line 19 and below. All this information devoted to harmonic signals in the discussion section is out of place. Moreover, it does not correspond to the data presented by you.

We present data from our gathered dataset. Except the one recorded at the Slumgullion landslide (Gomberg et al.), none of these signals have been published before. We discuss why we do not refer to these signals in the proposed classification.

10) Discussion. From line 19 to the end. The information provided does not correspond to a discussion of what is presented in the paper. It mainly concerns previous results without comparing them with the data presented in the manuscript.

We have thoroughly rewritten the discussion section.

Most of the sentences in the discussion could be included in the introduction, because the information is previous to the results presented in the paper and with little relation to them, at least in the present form.
We have rewritten the introduction and the discussion to take into account this comment and the previous ones.

Moreover, I do not understand why the harmonic signals are included in the discussion.
We discussed the harmonic signals in the Discussion section as we are not including them in the proposed classification whereas they have been presented in other studies. We find surprising that, on the one hand, reviewers reproach us not to compare our data to other and then, on the other hand, find the paragraph where we do this comparison not relevant.

11) As the authors mention on pag. 4 citing (Walter et al., 2017) in MS processing chains (by the way, I do not understand why you include this information in this section): "Many studies approximate the media attenuation field and/or the ground velocity, or do not take into account the topography, leading to mislocation of the events that prevents for accurate interpretation of certain sources and leads to false alarms". Is this the case of the data presented here?
We talked about location of the source which is an important information to associate the recorded signals to slope deformation. However, we mentioned here that location using attenuation law and assuming a homogeneous attenuation factor may lead to mislocation of the seismic events. Consequently, if the location error is of the same order of the distribution of geomorphological structures, it can be difficult to interpret the source of the recorded signals.
In the present study we did not locate the events and focus on the signals features

that can be related to the seismic source mechanism. The later is discussed in each sub-classes presentation with reference to studies that modeled the seismic sources from the seismic signals or to studies that observe similar signals in different context (e.g. glacier motion).

12) Papers under revision although they are public must not be cited, nor must papers in volumes without a standard scientific recognition.
We cite posters and abstracts only to present the monitoring sites and/or the datasets (Table 1 and Figure 2 to 10). We removed reference to posters/abstract when supporting statements in the text. For the papers under revision, we let the editor decides whether they should be included in the reference list (most of them being today accepted).

Some comments on the analyses of data. 13) As regards the tremor-like slope-quake (you do not mention this in this way in the title of figure caption of Fig. 12), the PSD is in the range of 8-13Hz, (not 10 Hz as mentioned) and the mean frequency of 20Hz is not clearly deduced from the plots.
OK. We corrected the description of this class and the caption.

14) Slope-quake with harmonic coda (H-SQ). I do not only observe the coda in the Chamousset signal (fig.10a) (note there is an error in this figure) of 08 August, but also in that of 6 October (fig. 11c). Super-Sauze site slope-quake signal of 24 Oct. (Fig. 10b) and the rock fall signal of 5 Nov (Fig. 4d) also present this behavior. This harmonic coda is present in different events. I think this is significant, and perhaps this is not related to the source but to the site effect for specific frequencies.
We agree that for this particular case, wave propagation could be a better explanation for the signal feature. Consequently, we removed this class from the classification and we discuss this signal feature in the new version of the discussion.

15) As regards all figures, but specifically Fig. 3 since it is the reference. Please, indicate the information contained in the plots in the figure caption. What is Amax? Is the parameter defined in section 4? It could be informative to show the maximum amplitude in ground speed units. What are the different traces in different colors shown in plot a? Indicate correctly the power of 10 (10 ËĘx and not eËĘx).

OK. We indicated that A_{max} refers to the maximum amplitude (nm/s). The different traces in different colors correspond to the other sensors present on the site, we added this comment in the caption. We modify the power accordingly.