Final response letter to the Editor and the Reviewers

Dear Editor and dear Reviewers,

My co-authors and myself would like to thank you very much for your time, efforts and contribution in helping us improving the quality of this paper. We are very thankful to Fabian Walter (Editor), Emma Suriñach (Reviewer #1) and Andreas Köhler (Reviewer #2) for reviewing our manuscript; we've also much appreciated a non-anonymous review!

In this letter we have responded the comments made on the revised manuscript. We've addressed each comment made by the two Reviewers individually in red and applied the suggested and related modifications on the revised manuscript version of March 20, 2018, using a track-change mode. Reference to page and line numbering is given in bold and correspond to the final version of the revised manuscript in track change mode.

We hope that our changes and clarifications, both in this letter and in the final revision manuscript text and figures, fully address the remarks made by the two reviewers.

Again, we would like to thank you for your time and efforts in helping us improving this manuscript.

With my best regards,

Naomi Vouillamoz

Associate Editor Decision: Publish subject to minor revisions (review by editor) (01 May 2018) by Fabian Walter

Comments to the Author:

Dear authors,

thank you for your revisions, which substantially improved the quality of your submission. Based on the additional round of external reviews, your manuscript can be published after minor revisions. Please read the reviews carefully and respond to them in the next iteration. They are mostly minor aimed at improving the readability and clarity of your work. However, I fully agree with Reviewer 1 who specifically asks for scatter plots of continuous parameters. This will be a concise way to illustrate and support some of your major points. Please pay particular attention to this suggestion.

Best,

Fabian Walter.

Report #1 from Referee #2 Andreas Köhler

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I think the manuscript has improve considerably and most of my previously comments have been addressed. I have just a few questions and suggestions:(1) I suggested to show scatter plots to visualize the distribution of event features and to evaluate how well defined the event classes are (clustering vs. end-members of a continuous distribution). I see from the new Fig. 14 that this is of course only possible for continuous variables like apparent velocity (3), duration (5), and frequency (7). In my opinion it would still be helpful to plot those features against each other (i.e, (3) vs. (5), (3)

vs. (7), etc.) using the measurements of all events. The variable "S" should also be included. However, maybe I misunderstood and those quantities are not stored, but only evaluated visually and qualitatively during the interactive processing?

As much as we agree that such plots would provide a great and elegant way to derive conclusions about our classification, we cannot generate such graphics at the moment and do not think they would necessarily help to better define the classes. Our arguments for this are manifold:

1) Those quantities are indeed not stored in a way we can produce continuous plots. Signal durations were attributed as a duration range for a given event (< 2 s; 2-10 s, > 10 s); dominant frequencies were defined also as a range. The related quantitative values that we have stored are the maximum station amplitudes of the event measured in the series of pre-defined bandpass waveform ((1-5, 5-20, 20-50, 50-100, 100-200 Hz) see the figure 1 below). Apparent velocity could not be derived for all events and in some instances also defined as a range, due to the general bad waveform quality and the corresponding uncertainties.
2) These attributes, especially the frequency content, are strongly dependent to the distance and the size of the source, meaning that one should expect important variations of some attributes for events of the same kind but recorded in different distances, as well as for records from one single event at different stations (i.e. different receiver-source distances).
3) The statistical significance in terms of high-quality end-members events of our dataset is unfortunately low, additional measurements and more complex waveform and spectral attribute shall be used to pursue and refine the classification.

We would like to stress that the method applied which consist in visual sonogram screening is de facto a PATTERN RECOGNITION method. The human eye still performs better than any currently existing algorithm. One will not miss a face, whatever its size, resolution or color in a bad quality picture, the best algorithms still do! As mentioned in part 6, further work for automated event detection and classification systems will require more complex waveform and spectral features as the ones used in this paper.

We added references in the method parts: Joswig, 1990,1995,1996 regarding sonograms and Vouillamoz 2015, Sick, 2016 regarding visual pattern recognition by sonogram screening.

The Figure 1 below displays data of the SZ10 catalog. For each event of the SZ10 catalog, the trace featuring the maximum signal amplitude was used. Then, maximum absolute 0-to-peak amplitudes were computed for the signal within the five defined bandpass filters (1-5, 5-20, 20-50, 50-100, 100-200 Hz) ($A_{max\text{-}bp}$) and scaled (as a percentage) to the maximum of the $A_{max\text{-}bp}$ of that trace. Except for the distant earthquakes which show a coherent pattern of higher amplitudes in the 1-5 Hz bandpass, the distinction of the other classes is not straightforward, showing many patterns overlaps and illustrating the complexity of finding an unequivocal classification based on simple seismic features.

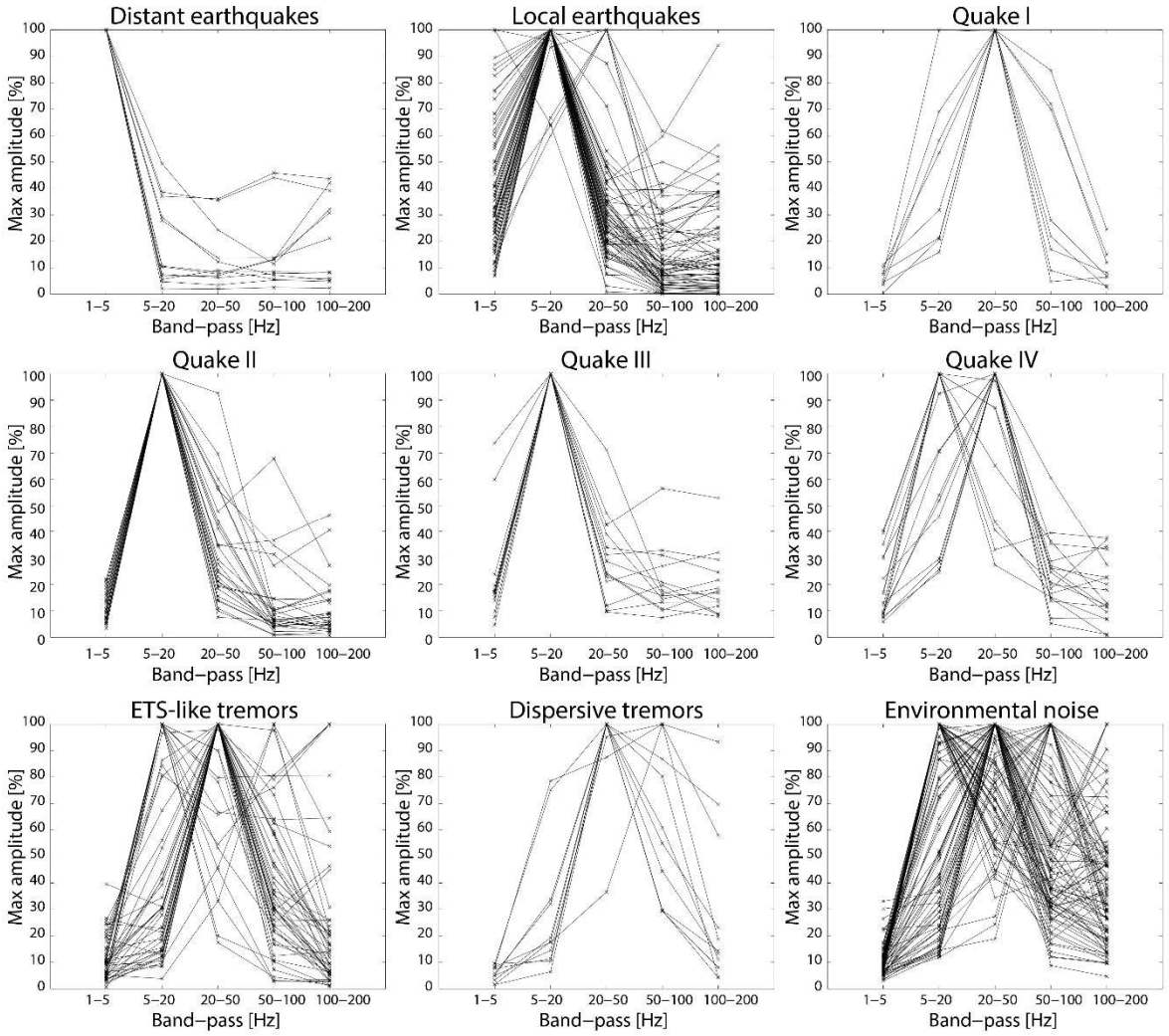

Figure 1.

(2) Array processing: I agree that classical beamforming (FK analysis) with non-planar wave fronts will probably still provide an idea about the source direction. Resolving the quadrant sounds reasonable. However, since the authors also use the apparent velocity: How much biased will this measurement be for sources very close to the array? Does it affect the classification?

No, we don't think this affects the classification.

We computed apparent velocity for those events displaying well defined and trackable wave packets, hence implying a source originated outside the recording network. Apparent velocity was not used for event location, we applied a pre-defined velocity model (following Tonnellier et al. 2013) and apparent velocity could not be computed for events originated within the network (no coherent solution was found using jackknifing as explained for example in Joswig, 2008 or Vouillamoz et al. 2016, Figure 5). As mentioned above, for some events, we ended up defining a range of apparent velocity. But clearly, a nearby source always features slow apparent velocity (< 2 km s^{-1}), hence the classification is not affected.

(3) Figure 14: I think I misunderstood the comparison of displacement rates and seismic emission rates previously. It seems that there is just a single average displacement rate in cm per day for each data set and not a displacement for each day Yes, as mentioned in the Data section (Why could the displacement not be measured with daily resolution? Moneywise and lack of human resources). So the correlation between seismic and displacement rate is based on three data points (three data

sets). Yes. In other words, also seismic rates are averaged rates over all days. Yes. Please make sure that this is clearly described in the text or clarify if I am wrong.

We specified **average** daily rates of landslide-induced microseismic events in Section 6.3. (now section 6.4)

(4) Earth surface dynamics allows to publish supplementary material / data. Have you thought about publishing the complete event catalog as supplement (not raw seismic waveforms, but plots like Fig.6 for each event and/or detection time, duration, frequency, apparent velocity, and "S" for each events)?

Yes, we have thought about it. However, as we still wish to publish further analysis of these data, we prefer to keep a track records of the dataset. As specified in the data and resources section, the data are of course available upon request to the authors (in SEG-2 or MSEED data format). All events traces are furthermore available in ASCII (*.dat) data format (1 file for one event and one station). All the plots (waveforms, spectrogram, bandpass filtered waveforms, amplitude spectrums…) are also available for each event and event trace.

Report #2 from Referee #1 Emma Suriñach

Comments on the new version of the paper

Characterizing the complexity of seismic signals at slow-moving clayrich debris slides: The Super-Sauze (Southeastern France) and Pechgraben (Upper Austria) case studies by Naomi Vouillamoz, et al.,

In this new version the authors have considerably improved the manuscript. The present version is much better organized and the aims have been clarified.

It is a shame that in the reorganisation of figure 14, the authors have omitted information on precipitation. I hope this will be explained in the future together with an explanation of the situation. As mentioned in the previous iteration, this is the subject of the PhD thesis of Sabrina Rothmund (submitted) and will be the subject of a new publication. Now, it is clear that the aim of the paper is only to classify and to catalogue the seismic signals.

As regards this, I suggest a change in the title of the paper.

We do not wish to change the title of the paper. We do characterize the signals. The paper also includes a 'location' and 'magnitude' section.

The incorporation of the S value in section 5.2, as suggested by Andreas Khöler, clarifies the results. Good suggestion.

I suggest that you should include a reference or a discussion on seismic array configuration optimization. This is well known in earthquake studies, but perhaps it is worth emphasizing this here.

We included a new section 6.1 "Passive seismic monitoring at clayey landslides" to discuss the seismic network.

Further comments

Pag 1 Line 25. I am not sure that this reference must be included here.

We moved the reference of P1 L25 at the end of the sentence.

Pag 2 Line 4. It is confusing. Perhaps: acoustic emission 10 -1,000 kHz (AE)

We modified P2 L3 to: "Seismic investigations of natural and artificial slope instabilities started in the 1960's with acoustic emission (10-1,000 kHz)"; as the acronym is not used further in the paper.

Pag 2 Line 15. Please check these two references.

The two references of Brückl and Mertl, 2006; Mertl and Brückl, 2007 are correct.

Pag 2 line 25. Perhaps explaining what the tripartite arrays are and method (or indicating a reference) could be informative to the reader.

We included a sentence. The text reads now P2 L25-30:

"This study aims at proposing a classification of microseismic signal types as recorded by tripartite microseismic arrays deployed at slow-moving clay-rich debris slides ("clayey landslides"). Tripartite microseismic arrays are suited for the determination of the back azimuth and apparent velocity of an incoming signal, hence providing key information about the signal source location (e.g. Joswig, 2008; Sick et al., 2012; Vouillamoz et al., 2016). The classification of microseismic signals is based on waveform and spectral attributes of the signals and uses microseismic observations reported by similar case studies as a benchmark."

Pag 3 line 2. Replace tele-earthquakes by teleseisms (I understand the context, but I think that the term tele-earthquake is not correct).

It is P4 now L5. Modified as asked.

Pag 4 line 4. I would stress the difference between the sites when beginning with Pechgraben. e.g. As regards PG….

We modified P4 L7-9 to: "At Pechgraben, due to the relatively large aperture (30-50 m) of the seismic arrays in the PG15 campaign, many near-source area microseismic events were recorded by less than three sensors. Consequently, a denser seismic network configuration was designed for the PG16 campaign."

Replace PG15 seismic arrays by seismic arrays in the PG15 campaign.

Responded by previous comment.

Pag 5 line 8. I would include a subsection entitled "classification" . The features you indicate are not simple waveform features: Apparent velocity of trackable wave packets is not a single waveform. You calculate the app. velocity with more than 1 waveform. Rewrite the sentence.

We included the section 3.1 Classification and modified P5 L18-19 to: "considering the following features:"

Pag 5 line 13. Indicate the software or method used to obtain the app. velocities.

The software is indicated P5 L4: HypoLine;

We added a reference to Vouillamoz et al. 2016, where the approach to compute the apparent velocity in HypoLine is described.

The sentence P5 L3-6 reads now:

"Each detection was first evaluated individually and interactively in HypoLine, where phases information were picked, and time offsets between array-correlated wave packets used to derive apparent velocity and back azimuth information following the approach described in Figure 5 of Vouillamoz et al., 2016."

Pag 5 line 16. Replace Unique versus multiple events by Number of events, Clustering of events or similar because this is not a feature name.

We modified to "clustering of events", also in Figure 14.

Pag 5 line 21. I do not follow this sentence. If the distance is short, then there is little attenuation. Attenuation is caused by geometrical spreading (distance) and by intrinsic attenuation. Although you explain the situation below, this specific sentence leads to a misunderstanding. Rewrite the sentence.

We agree, the paragraph was not clear and misleading. Attenuation is of course more important for larger distance. In our case, working at distances of less than a few hundred meters, we observed events originated a few hundred meters away from the networks showing waveforms being attenuated homogeneously resulting in all the waveform showing more or less the same amplitudes. On the contrary, a source originated within the seismic network displays a large range of amplitudes (several orders of units). We rewrote the paragraph P5 L29 P6 L6:

"The signals of landslide-induced microseismic sources are expected to be severely attenuated, mainly because of their propagation through heterogenous clay-rich soils of various water saturation (e.g. Koerner et al., 1981). Calibration shots and hammer blows carried out at Super-Sauze and Pechgraben showed that sources occurring within the seismic network feature prominent waveform attenuation across the seismic network, whereas sources originated a few hundred meters outside the seismic network feature waveforms being homogeneously attenuated, resulting in similar signal amplitudes across the seismic network. Therefore, only microseismic events featuring prominent and consistent attenuation of the signal maximum amplitudes across the seismic network are considered as a nearby source, potentially induced by the landslide dynamics."

Pag 5 lines 26-28. This is not part of classification. Don't think it is strictly method?

We moved the sentence to P5 L14-16: "Since the short receiver-source distances of the considered signals do not allow a clear separation of body waves and surface waves, amplitude information was taken as the maximum absolute 0-to-peak amplitude of the signal unfiltered vertical seismogram."

Pag 6 line 4. Replace teleseismic by Teleseisms

Modified P6 L18 as asked.

Pag 6 line 9. Specify that in Fig. 2 the layout is for an earthquake. You are in a section devoted to microseismic signals typology at clayey landslides. Perhaps a short introduction is needed to avoid confusion.

we specified P6 L23-24: "we apply the layout of Figure 2, which illustrates an earthquake signal…"

Pag 6 line 10. Split the sentence into two parts. …. ordinate. For Pechgraben NqF is 1.95…. Note that you are referring to the plots of an earthquake recorded in SZ10.

We modified to P6 L24-25:

a. Shows the signal sonogram (Joswig, 1990) up to the Nyquist frequency with a logarithmic ordinate, which corresponds to 1.95-250 Hz for Pechgraben data and to 3.91-500 Hz for Super-Sauze data.

Pag 6 line 14. At least indicate that the colours are in dB, if you do not show the colour scale.

Modified P6 L29 as asked.

Pag 6 line 21. …. Hz, defined as b1 to b5.

Modified P7 L2 to: "defined as bp1 to bp5"

Pag 7 line 21. I understand that seismograms are not displayed by distance with the result that no apparent velocities can be obtained from the plots of figure 3. It could be helpful to indicate this in the figure caption.

Even if the plots of Figure 3 were displayed as a function of source distance, one could not derive a decent apparent velocity based on the figure resolution!

Pag 7 line 25. Replace Fig. 4b and 4e, lower panel by Fig. 4b and 4e(lower panel). The same in the other cases below.

We modified this consistently in the document.

Pag 7 line 26. I would replace “consist of ” by “appear as ”. In fact, you have not modelled the wave field.

Modified P8 L8 as asked.

Pag. 7. It seems to me that the differences between Type III and IV are very small. Is the value of the duration indicated in Table 1 for Type III (moderate distance) correct? As showed, is the apparent velocity the only difference? I am not aware if this is sufficiently stressed in the text.

Text P17 L21-25: “Type III and type IV events feature S values which are below 200 % and must **represent a continuous transition of quake events recorded at larger receiver-source distances**. The higher apparent velocities of wave packets of type IV events and the consistent signal amplitudes of well distinguishable successive phases across the seismic network suggest a source origin outside of the landslide body in the host rock.

Pag 9 line 20. Mention Table 1 somewhere in the ETS-like signal and in the Confirmed rockfall events paragraphs.

We mentioned Table 1 at P9 L21.

Pag 10 line 25. I would number the recording stations to identify the seismograms in fig 7 and 8 so as to help the reader.

As much we are willing to help the reader, indicating a complete station numbering in Figure 8 results in a pretty bad resolution…

Pag 11 line 1-10. Same as above comment for figures 8 and 9.

Stations are indicated in the caption of Figure 9!

Pag 11 line 28. This link is not working at the moment!

It was working on September 13. The internet page was unfortunately removed.

We modified P12 L10-11 to: “developed by the LIAG, Leibniz-Institut für Angewandte Geophysik, Germany”.

Pag 11 source location. I suggest that you should include a reference or a discussion on seismic array configuration optimization in this section (or in 6.2). This is well known in earthquake studies, but perhaps it is worth emphasizing this here.

Answered previously in the non-numbered comments. We included a section 6.1 Passive seismic monitoring at clayey landslides to discuss passive seismic monitoring at active landslide.

Pag 12 lines 15-16 and 24. Perhaps I am lost, but don’t you think that there is a contradiction?

A source located within the seismic network is resolved without the need of array processing. Phase information are processed in network mode, providing the hypolines (see Joswig 2008; Vouillamoz et al. 2016 for details).

Pag 13 line 15. For me this is not a valid argument. Note that site effects are frequency dependent. Yes. The frequency content of teleseisms and distant earthquakes is lower than that of the events that you are considering. Yes. Fig 2 indicates f<10Hz. Moreover, note the frequencies indicated in Table 1. Table 1 is intended to give general characteristics of local and more distance earthquakes. For local earthquakes, we still have energies up to 20 Hz. Perhaps applying Kanamura method H/V would be useful. Of course, in an additional publication maybe?

Pag 14 line 8. I am sorry, but I do not follow you. Why do you not represent the values of the amplitudes in the plots in Fig. 12b? Perhaps an explanation is needed.

Local magnitude is still computed by many seismological institutions around the world by applying a Wood-Anderson filter to transform seismic records measured by a modern seismometer in nm s^{-1}

into “Wood-Anderson” records in mm as used in the original definition of the local magnitude by Richter.

It is however more convenient to work directly with the original data, as measured, in nm s^{-1} and get rid of all the filtering needed to convert and measure the peak-to-peak Wood-Anderson amplitude in mm (which is in addition period/frequency dependent). Other studies used such approaches, for instance: **Gaucher, 2015**; Earthquake detection probability within a seismically quiet area: application to the Bruchsal geothermal field, Geophysical Prospecting, doi: 10.1111/1365-2478.12270; or Edwards et al. 2015; Seismic monitoring and analysis of deep geothermal projects in St Gallen and Basel, Switzerland, Geophysical Journal International, doi: 10.1093/gji/ggv059.

Pag 14 line 11. This link is not working at the moment

This page has also been removed. We modified P14 L26 to: SISSY product information sheet.

Pag 14 line 16. Is the value 5e106 correct?

Yes, $5e10^6$ is the correct value! It could be discussed of course, but it was the best compromise we found between the different calibration datasets we have.

Pag 14 line 22. Complete the legend of Fig. 12b indicating this.

Indicated in the caption of Figure 12 as asked.

Pag 15 line 13. Replace this by near, local and regional earthquakes and teleseisms.

Modified P 16 L12 as asked.

Pg. 16 line 32. Schlindwein et al., 1995 is not indicated in the references.

We added the reference.

Pag 19 line 18. Note that the name of my department has changed. It is Department of Earth and Ocean Dynamics. Faculty of Earth Sciences. University of Barcelona (UB)

We corrected the name of your department.

Figures, Figure captions and Tables

- Is the value of the duration for Type III (moderate distance) indicated in Table 1 correct?

Yes! It is about 2 seconds.

- Figure caption 1. Please define PG15, PG16 and SZ10.

We specified.

- Figure 2. Please replace the title. Seismic features of an earthquake in different representations… You are using different representations to show the characteristics

Modified as asked.

- Fig 6a. Indicate bp1 to bp5 similar as in Fig 4a

We modified the figure 6 and 7 as asked

- Fig 8. b) You must add “at short distances” after human footsteps. This must be specified.

We agree and modified as asked.

- Fig 9. a) You must add “at short distances” after human footsteps. This must be specified.

Modified as asked.

- Fig 10. I would indicate the solution by an open circle.

There are several solutions, hence the uncertainties…

- Fig 12. In b) the extrapolated functions must be indicated. In line 5, is projection the correct word?

This is now indicated it in the plot of Fig 12b.

- Figure 15. I noticed that you have unified the slide quakes and dispersive tremors in landslide-induced tremors. This is consistent with the classification of your catalogue. Accordingly, I would replace Quakes by landslide –induced microearthquakes to be consistent.

Modified as asked.

- References

Aki and Richards 2002, is not mentioned in the text.

We removed the reference.

Joswig 2008. Please complete or correct (capital LETTERS) the reference: First Break (June).

We corrected the capital letters.