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

**Anonymous Referee #2**

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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.

In general, I am very skeptical about the purpose of the paper: to establish a standard typology of endogenous seismic sources. It is true that there has been a dramatic increase of monitoring/detecting seismic signals generated by different ground phenom-



ena 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. 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. For this paper, I suggest you only include the very significant seismic signals and avoid small events. 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. All efforts in this line should be devoted to monitoring one site with different instruments and to interpreting the events in coordination with different specialists. 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.

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 stablish a standard typology, consideration of the peculiarities (or not) of the site effects is very important.

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.

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. 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. 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.

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.

Pag. 11 Line 2. Please, indicate if the rock fall was monitored. Information specific on this events is necessary.

Line 5. What does it means.... energy below 10 Hz is present for....1 m3 ( Fig 3a). Is this your case, because you mention this figure here?

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.

Line 9. P- and S- waves are hardly distinguishable..... Is this in this specific case? You cannot generalize.

Line 11. First arrivals are mainly impulsive. At the scale of representation I have to



believe it.

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.

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.

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

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.

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.

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?

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?

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.

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. 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. Moreover, I do not understand why the harmonic signals are included in the discussion.

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 mis-location 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?

12) Papers under revision although they are public must not be cited, nor must papers in volumes without a standard scientific recognition.

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.

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 O ct. (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.

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 ˆx and not eˆx).

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