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Comment

Interactive comment on “Seismic monitoring of geomorphic processes” by A. Burtin et al.

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The manuscript by Burtin et al. reviews the use of passive seismic observations in the field of geomorphology. This is certainly a topic of current interest to a wide audience, so such a review is timely. Most of the text is clearly written and easy to follow. My main criticism is that in the title and throughout the manuscript the authors suggest a comprehensive review that covers all or most seismogenic processes in geomorphology. However, technical details are then primarily provided for research, which the authors have previously been involved in. Other fields, such as rock falls, landslides, avalanches and the theoretical foundations of river noise generation are only discussed superficially. Moreover, the explanations seem too qualitative to address an audience with seismological background. At the same time, the present explanations seem too brief and abstract to target a reader who has not read the authors' previous publications.

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To my mind, the scope of the manuscript has to be redefined. I suggest something in the direction of "Seismic Monitoring of Torrential and Fluvial Processes". The comments on landslides, rock falls, avalanches, etc. could be moved towards the end of the manuscript, parallel to an outlook section. The explanations would benefit from several additional figures, including one or several cartoons as suggested below.

I have to admit that my review ended up quite lengthy. This is because I think that much of the seismological community still views the use of seismic methods for mass motion monitoring as an "exotic" application. This is unfortunate, because there is significant potential in this approach as the author's previous work has demonstrated. Therefore, I think that adjusting the present manuscript to make it accessible to a wider audience could lead to a well-cited milestone reference.

Fabian Walter.

GENERAL COMMENTS

It would be useful for the reader to have an overview of previous work at a single glance. For this, the authors could include a summarizing table specifying the monitored process types, signal duration, network aperture, network deployment period, network geometry, source-station distance, location technique and/or other important aspects of the studies they discuss.

From the start of the manuscript the reader wonders what is really causing the observed seismicity. The authors mention this at distinct parts throughout the text, but a single source mechanism section with some more details would help. I suggest summarizing the work of Tsai et al. (2012) and Gimbert et al. (2014) to provide insights on seismogenic processes like particle saltation and water turbulence. This does not have to be a lengthy discussion, but some insights would be important, such as PSD-frequency dependence and the hysteresis, which the authors already present. This source discussion could be centered on a cartoon that also discusses the differences between local and ambient measurements.

Section 2.1

This section is very general and I disagree with some statements. Also, several points (e. g. polarization, geometric spreading, instrument types, site affects, seismometer sensitivity) require references. Is such a lengthy explanation of seismic waves really needed? For example, the difference between surface and body waves is explained, but for most of the authors' seismogram examples the reader is left wondering if the presented signals are body or surface waves. To my mind, the theory of seismic wave propagation is too complex to fit into such a general, relatively short section. For instance, no mentioning of near field and far field effects is made, which may play an important role for relatively small source-station distances in torrent monitoring. If the authors decide to keep such a section, I propose to include a sketch of the different polarizations.

I am not sure that one can say that in general seismic energy on the horizontal components is larger than on the vertical one (Page 1223/ Lines 10ff). This really depends on the source-station distances, e.g. if surface waves have developed. If the authors want to stick with such a statement, it would be important to explain the critical depth/epicentral distance ratio at which surface waves form and medium influences on Rayleigh ellipticity. Since for geomorphic processes at the surface the source depth is negligible, it could be argued that surface waves tend to form even at close distances (I am not sure myself that this has been observed, but it could be an explanation).

The presentation of the different sensors would benefit from a table listing flat frequency range, sensor types and examples. By the way, I thought that seismometers and velocimeters refer to the same instrument. Also, is there really such a clean distinction between geophone and seismometer, or is this something that scientists use rather loosely? What are the differences between short period seismometers, short period sensors and geophones? A table and references could clarify this. It seems that in fluvial studies, acoustic sensors typical refer to "local" measurements (the author's terminology). This should be made clear, because some readers will likely associate

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acoustic sensing with sound waves in air.

I do not fully agree with the discussion on frequency excitation (Page1221/Lines 18ff). The reason is that there is a subtle but important difference for earthquake and landslide sources. For double-couple earthquake sources, one can define a rupture model, which gives rise to corner frequencies, which decrease with increasing moment, as the authors suggest. However, for landslides, it is the time scale of the centroid single force history (i.e. the acceleration-deceleration period of the moving mass), which determines the characteristic frequency of the landslide. From this perspective the involved mass is unimportant. So a general statement on seismic sources including earthquakes and landslides has to be made with care.

Sections 2.2 and 2.3

These sections would definitely benefit from sketches or cartoons showing the instrumentation, the river and the signal generation. Moreover, the local monitoring could use more detailed explanations, because compared to the ambient monitoring its discussion is rather condensed.

Section 3.1

This may be a misunderstanding on my side, but I was confused when I read this section for the first two times: The authors mention time-frequency analysis in the first sentence, but then focus on calculation techniques of the frequency spectrum. However, after the first sentence, the reader also expects some comments on the temporal resolution. As far as I know, the wavelet technique mentioned at the end has its strength in the fact that compared to conventional FFT's it improves resolution in both time and frequency space (this paper could be cited in this context: Tary, J. B., Herrera, R. H., Han, J., & Baan, M. (2014). Spectral estimation—What is new? What is next?. Reviews of Geophysics.). If it is not too much work, this section would be easier to understand if there were accompanying spectral plots showing the advantages and disadvantages of the discussed techniques.

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Section 3.2.1

As stated above, to my mind the discussion on landslides and rock falls is too short compared to the debris flow and bedload transport discussions. I recommend focusing the paper on these latter topics and moving the landslides and rock falls to an outlook or side discussion towards the end of the manuscript. Otherwise, topics like single force source mechanisms of large landslide seismograms probably deserve some more explanations. The non-expert will unlikely grasp this concept with the current explanations. By the way, the idea of landslides in glaciated regions is dated (Page 1230, Line 11).

Section 3.2.2

This section is very useful, because it describes the type of "noise" signal that long-term seismic monitoring usually captures. The earthquake signal discussion is somewhat blurry, because it is not clear to me where a general measure of event duration comes from. The signal duration depends on source-station spacing, whereas the rupture time depends on the earthquake's source time function. This should be made clearer.

Section 4.1

This is extremely useful information for newcomers to seismology and in particular seismic observations of geomorphological processes. The accompanying figure is well chosen. However, more references are needed, especially for the paragraph starting on Page 1234/Line 13. Again, a table, which summarizes trigger parameters of previous studies, would provide a concise overview for the reader. It may be interesting to mention other detection techniques, such as frequency domain triggering or automatic waveform discrimination (e. g. Hidden Markov Models: Hammer, C., Beyreuther, M., & Ohrnberger, M. (2012). A Seismic Event Spotting System for Volcano Fast Response Systems. Bulletin of the Seismological Society of America, 102(3), 948-960.).

Section 4.2

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To my mind, location methods for geomorphic events involve many subtleties, which conventional seismologists are unaware of. I therefore suggest reorganizing this section to make it more accessible. If the authors do not agree that this is an improvement, I leave it up to them to decide what to change.

I suggest starting the section with a quick overview of relevant location schemes: arrival time inversion (typical for earthquakes, only possible if individual impulsive arrivals are visible), polarization and triangulation from travel time differences (By the way, could signal attenuation also be a possibility, such as presented in this paper: Battaglia, J., & Aki, K. (2003). Location of seismic events and eruptive fissures on the Piton de la Fournaise volcano using seismic amplitudes. *Journal of Geophysical Research: Solid Earth* (1978–2012), 108(B8)?). Then I would focus on the triangulation approach based on inter-station signal coherence measurements (explaining the involved equations, which the authors present). Here, I think it is extremely important to state what is meant by coherence: If I understood correctly it does not necessarily mean that the signal phase at some time delay agrees for at least two stations. Rather, it is enough that individual seismicity bursts are measured throughout the network and thus that the signal envelope is coherent. A simple plot showing the cross-correlation of the raw signal and a signal envelope of an event would illustrate this nicely. In fact, the coherence issue may be important enough to have its own subsection. Finally, a subsection could be dedicated to the implementation of coherence-based triangulation: beamforming, time residual PDF's and the HI-Climb approach. However, all technique descriptions require some more details. For instance, for the HI-Climb approach, it is not clear to me what "locating the coherence" means.

Section 5

This section seems to discuss different network layouts rather than network geometries. In network geometry discussions seismologists usually comment on whether the stations are ordered in a cross, a circle, a line, a semi-circle, etc. So I would make it clear that the discussed layouts are geared towards different monitoring schemes: the

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"linear" network (which I suggest naming stream-parallel layout) can track the passage or debris as seismicity bursts on each station individually, the other layouts can locate mass motion via some coherence measurement. In the current discussion, it seems implied that the radial location uncertainty for sources outside the network is inherent to a certain network geometry. This is a problem for all geometries, including linear, circular, small, large. This should be made clear and the meaning of the first paragraph in Section 5.2 thus clarified. It would also help to provide quantitative estimations of the array resolution limits considering frequency content and apparent velocities. A brief summary can be found in Section 2.7 of this paper: Poggi, V., & Fäh, D. (2010). Estimating Rayleigh wave particle motion from three-component array analysis of ambient vibrations. *Geophysical Journal International*, 180(1), 251-267. Again, if this part of the paper included a sketch or cartoon, the reader could grasp the concepts behind location much easier.

Section 6.2

The Q-based determination of debris flow velocities is a great idea! For this discussion the reader needs to see the governing equation (and also some equivalent references). If I understood correctly, this is just the expression for seismic amplitude including anelastic damping and geometric spreading, right? This would facilitate understanding of the concept, whereas phrases like "minimizing the shape of the recorded seismic pulse" are unclear. Moreover, the reader most likely will wonder if there is agreement between measurements of debris flow using different stations. A set of seismograms supporting the statement "In the Illgraben, the different flow pulses of the debris-flow sequence showed similar behaviour; in each of the three pulses, seismic energy increased inside the catchment, implying addition of sediment by bed erosion and/or lateral input" would furthermore strengthen the discussion.

SPECIFIC COMMENTS

The units on the y-axes of seismograms should have physical meanings (e. g., nm/s).

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Page 1218/Line2: "the signal": specify Line 5: rewrite "led to develop" Line 9: Not clear what "its" refers to Line 11: "Aims to give" → gives Line 23: Which scales (time/space?)?

Page 1219/Lines 28-29: "can be used to illustrate" → illustrates

Page 1220/Lines 5ff: indicate observations directly in figure Line 8: "any process" seems too general, elastic wave generation has to be involved Line 21: "path-finding": unclear Lines 23-24: "the seismic monitoring" → seismic monitoring

Page 1221/Line 4: I suggest renaming Section 2 as "Seismic Monitoring" Line 13: "radially polarized": rather on the radial and vertical components Line 24: What are "process events"?

Page 1222: In the discussion on Q, it may be worth mentioning that the definition of this quantity aims to make it frequency independent.

Page 1223/Lines19-21: I suggest explaining site effects in more detail. A non-expert will not understand what this is or how horizontal component seismograms play a role with it. The concept of H/V ratios should be introduced in this context. Also, what is meant by "seismic amplification"?

Page 1224/Line7: specify "rates"

Page 1225/Line 7: specify what makes these sensors "less sophisticated" Line 10: What is meant by "process manifestations"?

Page 1226/Lines 4-5: specify hydrological parameters

Page 1229/Lines 10-13: Incomplete sentence? When discussing Figure 5, use figure markers to guide reader. When describing rock fall characteristics, providing only one reference (Deparis et al., 2008) seems not enough, unless this is a review paper. Why is the impulsive earthquake seismogram not expected for rock falls? For low-frequency landslide signals more references should be given (e. g. Kawakatsu 1989; Ekström

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and Stark, 2013; Allstadt, 2013).

Page 1239/Lines1-2: "Temporal normalization can be strong using 1-bit processing": Not sure I understand this. Perhaps: Coherence can be improved with 1-bit processing ...?

Page 1230/Line 18: specify "many" Line 20: specify "very large" Lines 23ff: I suggest mentioning the seismic signal of turbulence (Gimbert et al., 2014).

Page 1230/Line 5: "simple" could be specified (no lateral variations?, crust over mantle?)

Page 1231/Line 17: Leave out first sentence ("Tectonic events ...") Line 19: "instantaneous" -> impulsive Line 22: "scattered" -> scattering Line 26: some more references seem appropriate Line 19: reason for better coupling?

Page 1232/Line17: rewrite "It could be that"

Page 1233/Lines 3-4: Just a comment: I have struggled with anthropogenic signals in nearly all frequency bands, even at very low frequencies due to seismometer tilt (at 10's of seconds or longer). Line 6: It may be worth mentioning that geomorphic events are often only detectable at single stations, as well. Lines 20ff: I suggest making clear that the STA and LTA windows have to be successive.

Page 1234/Line14: "of" -> to

Page 1235/Line 5: What is meant by "absolute signal"? Line 10: rewrite "interest of a frozen LTA" Line 26: "time arrivals" -> arrival times

Page 1236/Line 1: "in" -> into Line 4: "but the procedure is different" is too general Line 9: "a" -> an Line 10: "frequency content of signal" -> signal frequency content

Page 1237: it may be worth discussing the meaning of the velocity parameter: is this an apparent velocity? A surface wave velocity? Line 14ff: Which wave phase does the polarization approach target? Line 20-21: Why does the polarization become highly

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variable at higher frequencies?

Page 1238/Lines 2-3: The statement " In contrast to hillslope processes, channel processes generate ambient seismic noise that overwhelms discrete sources." is exclusive enough to require references.

Page 1239/Lines 20-22: The sentences "This correlation ..." and "In the described method ..." are difficult to understand.

Page 1240: For the very interesting discussion on Q-based velocity measurements I was wondering if spatial variations in Q-value are a problem.

Page 1241/Lines 19ff (in particular the sentence "For location ..."): This discussion seems too qualitative. The location precision depends on the array response, which is not only a function of azimuth, but also of distance. This point should be discussed. Also, it is not clear what longitudinal and perpendicular means. A sketch would help.

Page 1242/Lines 3-5: This is not clear to me (e. g. "the azimuths of all the station pairs cover the entire range of possible fluctuation", fluctuations of what?) Lines 7-8: This statement about standard array aperture sizes needs a reference. For this discussion it may be interesting to mention the possibility to use several arrays. With more than one back-azimuth measurement, a source epicenter can in theory be located (e. g. Richardson, J. P., Waite, G. P., FitzGerald, K. A., & Pennington, W. D. (2010). Characteristics of seismic and acoustic signals produced by calving, Bering Glacier, Alaska. Geophysical Research Letters, 37(3).).

Page 1244/Line 20: The meaning of "coherent" in this context is not clear to me. Lines 11-12 vs. Line 25: Are "debris flow pulses" and "sediment-laden flows" the same? If so, I suggest using one terminology, only. Otherwise, the difference should be explained.

Page 1245/Line 19-21: This sentence needs a reference.

Page 1246/Line 2: This may be a trivial question as I am not an expert in debris flow processes: Do the authors imply that only suspended load was present in the debris

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flow? Is this typical? Lines 5ff: I suggest also stating that seismic monitoring provides unrivaled temporal resolution.

Page 1247/Line 22: "specificities"?

Page 1248/Line 22: rewrite "it looks possible"

Page 1249/ Line 7: "area" → areas

FIGURES

Figure 2: the orange dashed circles (labeled D_1 and D_2) need to be defined. Also, many readers will not be familiar with basket samplers (caption).

Figure 3 caption: "relative to the velocity" (of what)?

Figure 5: Panel labels are missing.

Figure 6 caption: "ratio figures": specify panels

Figure 7 caption: "located and in relation": rewrite

Figure 9: Specify location of station H0460 (country/mountain range/...).

Interactive comment on Earth Surf. Dynam. Discuss., 2, 1217, 2014.

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