Dear Editor,

Please find with the following letter our reply to the reviewers of our manuscript "*Seismic monitoring of geomorphic processes*" (Burtin et al.), submitted for publication under the reference esurf-2014-43.

We compiled all the reviews in a single rebuttal letter as the major comments were quite similar and we tried to answer each point highlighted by the reviewers.

We hope that our suggestions and the proposed improvements of the manuscript will correspond to your expectations, and that we could submit a revised version fitting the standard of papers published in Earth Surface Dynamics.

With kind regards,
Arnaud Burtin

Interactive comment on "Seismic monitoring of geomorphic processes" by A. Burtin et al.
F. W. Walter (Referee)
fabian.walter@wsl.ch
Received and published: 27 January 2015

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. 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.

[1] As it was suggested by all reviewers, we chose to add a longer section describing slope processes in the revised manuscript. We think it is important to keep such a section in the article since these geomorphic processes are linked, and sometimes coupled, with channel processes. To illustrate our focus on the channel processes,

we also changed the title to better reveal the content of this work. Finally, we would like to mention that the main goal of this article is to provide information about the seismic monitoring of geomorphic processes to a community of geomorphologists, non-seismologists but certainly aware of acoustic monitoring. Therefore, some sections might appear less detailed with technical aspects but we tried as much as we could to add references where the readers can find full additional information.

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.

[2] To review channel processes and the associated seismic signal, we moved the description of work by Tsai et al. (2012) from the outlook section to an earlier point in the manuscript. We also introduce the theoretical work by Gimbert et al. (2014) describing the seismic signal originating from turbulent water flow. Finally, we now discuss some interpretations made in this study on the fluctuating hysteresis with frequency.

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.

[3] We agree with the reviewer that a full theory of seismic wave propagation is not appropriate to fit such a section. Furthermore, we think this content is not entirely justified for the readers we are aiming at (see [1]) and we give references for full developments of the theory. However, we expanded the description of the seismic wave signal to introduce the near- and far-field, the wave types with sketches, and anelastic attenuation including frequency dependence and medium heterogeneities.

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, if 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).

[4] This argument is given from the observation made from several study sites and for the recording of river activity. Among which, the Hi-Climb dataset is a good illustration. Spectrograms for each component (Z, N, E) are displayed in Burtin et al. (2008) and exhibit a systematic larger energy on the horizontal components. To our knowledge, we have not seen any study showing an opposite pattern yet. To provide general comments in this section, we removed the statement about the seismic energy on the horizontal components.

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 acoustic sensing with sound waves in air.

We made some clarifications to our statements in the revised manuscript.

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.

We agree with this comment and we made a clearer statement about the observation of low-frequency content due to large landslides.

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.

[5] To also reply to a previous comment, we introduce the “local” term because (1) the measurements are usually in-stream estimates, close to the studied process, and (2) the measurements are spatially punctual estimates (integration of processes

occurring upstream). For local (in-stream) monitoring, the developments, procedures and strategies of study are different and are already exposed in a special issue that was following a workshop on assessment of bedload monitoring (Gray et al., 2010). We only briefly review the contents of this manuscript and refer the readers to this publication. *"International Bedload-Surrogate Monitoring Workshop, April 11-14, 2007, St. Anthony Falls Laboratory, Minneapolis, Minnesota, United States."*

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.

For clarification, we modified the text when we introduce the time-frequency analysis. We also mentioned the reference quoted by the reviewer on the interest of wavelet techniques but we do not further expand the discussion since to our knowledge there currently is no direct application for the monitoring of channel processes.

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).

[6] To continue with comment [1], we think it is important to keep a description of slope processes in this manuscript, even more as we open a discussion about catchment monitoring that would include them. Therefore, to answer this comment, we added a longer discussion on slope processes (section 3.2.1.1) with appropriate references.

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.

We think there was a misunderstanding between the event duration and the source duration (or rupture time), when we introduced the signal duration. We comment on the signal duration, which is the duration of the observed signal on the seismograms. We do not characterize the source time function there. However, we also agree with the reviewer that the signal duration of an earthquake is dependent of source-receiver distance as the signal is composed of waves travelling at difference velocities and paths, as suggested in the text on teleseismic event. To clarify, we modified the text in the revised manuscript.

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.).

We added a description of automatic event classifications from HMM methods in the revised manuscript.

Section 4.2

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.

In the descriptions of the location methods, we tried to be as concise as possible since these approaches were well described in the referenced publications. We do not think it is necessary to describe the approach in every detail, as the reader knows where to find the relevant information from the references. If the Editor wants a clear development, we can expand this section in the revised manuscript.

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"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.

We are not sure we correctly understand the reviewer's comment, but we interpret layout and geometry to be equivalent terms. We do not suggest that event location is difficult with a certain network geometry, when sources are outside a network. We say that it is for any network geometry, as suggested, but we also say it is difficult for transverse location with a source inside a linear network (Burtin et al., 2009). We hopefully clarified these points in the revised manuscript.

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.

We added further details of this application in the revised manuscript.

SPECIFIC COMMENTS

We took into account as much as we could the minor comments of the reviewer.

Interactive comment on "Seismic monitoring of geomorphic processes" by A. Burtin et al.
Anonymous Referee #2
Received and published: 28 January 2015

Overall Comments

The paper reviews the recent achievements in the field of seismology applied to geomorphic processes, and in particular to fluvial processes, debris flows and hill-slope processes such as rockfalls and landslides. The authors provide in the introduction a qualitative discussion on the basics of seismic monitoring as well as on the relation between the seismic signal and an excitation applied on the Earth's surface. Then, the authors describe the techniques associated with the detection and processing of seismic signals, as well as detail suited designs of seismic arrays for geomorphologic purposes. Finally, a taste of using the seismic technique for geomorphologic purpose is given at the scale of a catchment in section 6, before perspectives are provided in the outlook section.

In the context of our growing use of seismic approaches to study Earth surface processes, I strongly encourage such a review effort. However, I think that the paper here proposed does not fulfill the requirements of a review paper for two main reasons:

- First, the authors aim to provide a review paper on studying a variety of geomorphic processes from the use of seismology. While the review is somewhat complete regarding fluvial seismology and debris flows, I think that it is largely incomplete regarding rock falls and landslides, which are fields where numerous key advances are not mentioned (I think for example about the work of Mangeney, Larose, etc...).

See comments [1] and [6].

- Second, even when considering this paper as being more a review paper on "Seismic monitoring of fluvial and debris flow processes" (which I think would in itself justify a review effort given the growing interest of the scientific community in that topic), I think the authors only partially succeed in providing to both seismologists and geomorphologists a comprehensive view on the main questions that they raise in their introduction, and that a non-specialist would hope to find answers to, which are: (1) how can the seismic signal be exploited in order to extract reliable information on the various geomorphic sources and what does it rely on regarding our knowledge in seismology, i.e. the effect of ground properties, heterogeneities, wave types, etc...?; and (2) what type of key physical quantities can be accessed from the seismic signal and could not (or at least less easily) be accessed using other measurement techniques?

See comment [3].

For these two main reasons, I do not recommend this paper to be published in the present state unless the authors (1) refocus on the fluvial seismology and debris flow side of the work (which does not mean removing rock falls, landslide and earthquake examples, which are useful for pedagogic comparisons), and (2) provide major changes in the manuscript related to the criticisms listed below, which apply in view of a review paper being more focused on fluvial and debris flow seismology.

Major Comments

While I find that section 1 (introduction) nicely exposes the potential of seismology to study surface processes (and orients the reader to the two main questions listed above), I am disappointed by the answers provided in the rest of the paper, which almost entirely focuses on the very technical side of the bibliography, while sometimes omitting to underline the physics involved as well as to highlight and replace in a broader context the main scientific achievements acquired in previous studies. More specifically, my main criticisms are threefold:

1-First, very little sense is provided to the reader on the role of seismic wave propagation (and subsequently ground properties) in affecting the seismic signal, and thus in modifying the source characteristics. I think that this point is a crucial point, since it is always the main source of worrying for people who do not know much about seismology, but are interested in the application. How good is our knowledge on ground properties, how well can we account for these effects when aiming to retrieve the source from the seismic signal? Green's function formulations exist in seismology, and I suggest that the authors clearly show these formulations and discuss more specifically the role of the different physical terms and our knowledge of them. In particular, an extensive discussion is provided in section 3.2. on the seismic signature of the various seismic events provided in Figure 5, but little is discussed on the effect of wave propagation. The authors should acknowledge that, while the signature of the source affects the seismic signal, a large part of this seismic signal is also affected by the propagation of surface waves into the ground, and thus by ground properties. As an example, the spectrum associated with rivers (and not necessary entirely bedload as suggested on Figure 5...) is much higher frequency than another river signal reported for example by Burtin et al. (2008), which would look more like the rockslide or car signals also shown on Figure 5. Why is that? Theoretical concepts must be provided to convince the reader that seismology provides solid grounds and methods to quantitatively analyze the signal, which goes beyond being able to qualitatively discuss the apparent relative variations of the signal in space, time and frequency.

[7] See comment [3]. We chose not to express the Green's functions theory in this manuscript. We think it is not appropriate for a study mostly addressing a geomorphic community. For Green's function expression, we refer the readers to works cited in the manuscript.

2-Second, I think that significant theoretical progress realized recently in the field of fluvial seismology are not properly discussed and in an incomplete manner. In particular, the reader has to wait until the perspective (outlook) section to realize that modelling approaches have been done in the past to translate the seismic signal in terms of a bedload source (see line 5 of page 1248). Does it mean that from these modelling approaches we understand the quantitative relationship between the river spectral features discussed previously on Figure 5 in terms of source properties and their modulation by wave propagation? If yes, it would be worth mentioning earlier. To me, these modelling approaches are essential to identify the sources and have access to the physical properties to which geomorphologists are interested in. For these reasons, they are worthwhile dedicating a section to them, or at list a section that talks about the physics. As an example, the authors mention numerous times throughout the manuscript that larger grains or rocks impacting the ground generate lower frequencies: I think the reader would like to know what is the physical reason for that? The authors do the analogy with earthquakes (i.e. larger earthquakes generate lower frequencies), but the source mechanisms associated with grain impacts and earthquakes are different. Then what is the argument in both cases? Is it a similar argument? All this is not trivial and needs to be addressed. Also, while the authors discuss the possibility of water flow to generate seismic noise, they omit to acknowledge recent theoretical work on that topic (Gimbert et al., 2014), which provides a physically-based work that sometimes supports their statements, and sometimes contradict them. It would be necessary to have a discussion about this, the physics that we understand, and the physics that we do not.

See comment [2].

3-Finally, I think that too many technical details are provided on the instrumentation (example: seismometer versus geophone versus velocimeter... not sure there is fundamental differences between those, if yes it is not made clear) as well as on signal processing (Welch's versus other techniques, spectral whitening for cross correlation techniques, etc...). While these details may be helpful for people who start doing seismic monitoring of geomorphic processes, I think that they are rather of "practical" order, but are not necessarily helpful scientifically as compared to providing a more physical sense on seismic wave propagation and source mechanics (cf my previous comments 1 and 2).

From our experience and during presentations of our deployments, we were often asked about the technical aspect of the different seismic instruments, how they compare to other acoustic devices, the frequency bands, use of three components... Thus, we think it is important to give these details as a first indication for the readers.

Technical Comments

As with reviewer 1, we took into account as much as we could the minor comments of the reviewer.

Interactive comment on "Seismic monitoring of geomorphic processes" by A. Burtin et al.
Anonymous Referee #3
Received and published: 2 February 2015

General comments:

This manuscript offers a useful overview of a growing field that is much in need of a summary review. It is well written and should be valuable to anyone interested in exploring seismic monitoring approaches, especially of fluvial processes. However, the manuscript falls a little short of its ambitious potential as a truly comprehensive reference on the theory, background, and methods for seismic monitoring of geomorphic processes (as its title and subsection titles seem to promise). The discussion seems too general or qualitative in many places, and often lacks physical explanations or relevant details that could make discussions of fundamental concepts or methods more clear or practically applicable. In other places, discussion is too narrow and only applicable to specific cases, where a more generalized explanation would be more helpful. Where relevant technical details are included, while usually immensely useful and generally well-explained, they are often limited to one or two specific approaches, sometimes drawn from a single or few example(s) of prior research, and sometimes with no examples cited. The coverage of fluvial processes also heavily overshadows the discussion of other types of geomorphic processes, which are not covered in nearly as much depth or detail.

See comments [1] and [6].

Overall, I think the manuscript would benefit from the following:

1) Either expand the text significantly in scope (specific suggestions for doing this below), or change the title/subsections and redefine the manuscript's focus to an overview of a few selected studies' approaches to monitoring fluvial processes, perhaps with other processes discussed in brief where appropriate. Personally, I would much prefer the former, as I think such a comprehensive review would be a huge benefit to the community, as well as a landmark citation, but obviously this would require significantly more work.

See comments [1] and [6].

2) I think more comprehensive citations in almost all sections of the manuscript are necessary, regardless of which approach is taken in (1).

OK

3) As noted in later comments on section 3.2.1, I recommend either the addition of an introductory section briefly but comprehensively discussing the background/prior seismic monitoring work for each geomorphic process. Additionally, as your audience may include seismologists without geomorphic backgrounds, I'd suggest a simple overview of what each process is, its basic mechanics and relative role/importance in shaping landscapes, and the mechanism by which it generates seismic waves and specific distinguishing seismic characteristics (with mechanistic explanations for those characteristics).

As mentioned in comment [1], we think the first aim of this paper is to address monitoring solutions or ideas to geomorphologists. We chose to focus on the seismic signal characteristics for each geomorphic process rather than their relative role in landscape dynamics. All along the paper, we added some details to give possible explanations for the observed seismic characteristics.

4) Provide a thorough background with in-depth physical/mechanistic explanations for basic seismic wave characteristics and how they pertain to geomorphic processes... for example, Green's functions are not mentioned anywhere in the text (probably belongs in the background discussion of seismic wave generation and propagation, and also under methods), which seems like a significant oversight, as signal attenuation is a significant issue in any geomorphic monitoring study. As mentioned above in (3), physical explanations for the frequency characteristics of different processes should be included. Other specific examples are included in the comments below.

See comments [3] and [7]. As suggested by reviewers, we added more details on the seismic signals, including basic equations of ground motions to introduce the near- and far-field, the wave types with sketches, anelastic attenuation with the frequency dependence, but we do not express the Green's functions.

5) In many sections (e.g., methods, detection and location of processes, etc.), the manuscript would benefit from the inclusion of a broader and more thorough range of specific

approaches/techniques and examples where they have been used in research.

In this manuscript, we tried to give a wide sample of the use of seismic monitoring for geomorphic survey with a link to fluvial and torrential monitoring, although we discussed largely slope processes. As suggested later, we included some details about coastal geomorphology and snow avalanches. We do not think that we now omit a crucial process in our manuscript.

6) Some reorganization of the entire manuscript might be helpful. More systematic organization of the discussion of each process within each section would make the manuscript more navigable as a reference and might make it more clear to you where more information should be provided on a given process. The overall order of some of the sections is a little confusing as well; for example, array geometry seems like it might go better before detection and location, and might tie in more intuitively following the discussion of wave components excited in section 2.1. It might make sense to order the manuscript in the order a research might need to reference topics, for example: general seismic wave and geomorphic processes background, detailed background in seismic signals of specific geomorphic processes, set up and installation of arrays, seismic data pre-processing and spectral processing methods, event detection methods, event location methods, monitoring catchment dynamics. The authors can decide what makes the most sense, but I personally find the current organization a little unintuitive.

The array geometry section helps to introduce an example of a catchment size monitoring. Detection methods and signal processing approaches are independent of the array geometry and density, we chose to introduce them before what influences the nature of the monitoring and then we give an example of such a monitoring.

Specific comments:

2. Seismic signals and their monitoring
I think the titles for these sections are a little misleading, as the discussion focuses very heavily on fluvial bedload monitoring, without nearly as much detail on geomorphic processes such as landslides, debris flows, rock falls, etc.

The new section title is “Seismic monitoring”.

2.1 Signal generation, propagation and recording
It would be helpful to reference the relationship between seismic wavelengths and their minimum scattering length scale for heterogeneities (pg. 1222, lines 13-15). This is especially relevant when considering, for example, the effects of source-receiver distance in fluvial monitoring, since alluvial river beds/banks are often composed of alluvium with characteristic grain sizes, in contrast to the more homogenous substrate of bedrock rivers.

OK

Define “natural frequency” (pg. 1223, line 5).

OK

I think the statement that the seismic signal will be larger on horizontal components since they reflect both Rayleigh and Love waves, while the vertical component only records Rayleigh waves (pg. 1223, lines 12-16) needs a bit more discussion and may not be fully justified... for instance, Tsai et al (2012) assumed Rayleigh waves would be the dominant wave type generated by fluvial bedload sediment impacts, since they assumed roughly vertical impacts, and therefore analyzed only the vertical component of their signal. Roth et al (2014) also observed the largest amplitudes in the vertical component of displacement for bedload sediment transport.

There is no discussion about larger amplitudes on the vertical component by Roth et al. (2014). In the article, the authors chose to focus on the vertical component; no comparison is shown with the horizontal component. Along the Trisuli River, Burtin et al. (2008) showed that the horizontal components have larger amplitudes than the vertical. The choice of Tsai et al. (2012) to use only vertical impacts is a first order simplification in their modelling approach. In the future enhancements of the model, the authors clearly indicate that more realistic impacts (non-vertical) could be taken. It would mean the generation of S_h-waves and Love waves that are not recorded on the vertical.

This section would benefit from some concrete examples of geomorphic processes and the types of waves they are expected to excite (with relevant citations). Also, a discussion of instrument distance from the intended source (e.g., river, debris flow channel, etc.) with respect to the resulting near- and far-field effects would be very helpful.

We chose to provide details and examples of geomorphic processes in a later section.

Why are Green's functions never mentioned here? This seems highly relevant. I would recommend including an in depth background explanation of attenuation theory, as well as theoretical versus empirical Green's functions, and challenges such as how the Green's function is likely to vary in different geomorphic settings, local inhomogeneities, time evolution of substrates in dynamic environments, etc.

See comments [3] and [7]. This discussion about theoretical and empirical Green's functions could be theorized in a separate "seismology" study and is not appropriate for this special issue.

2.3 Ambient monitoring
The discussion of hysteresis in seismic signals of bedload sediment transport (page 10) is also supported by Roth et al (2014). It is also worth mentioning that hysteresis in the relationship between bedload sediment transport and water depth or discharge is a well established phenomenon in gravel bedded rivers this is perhaps at least as relevant as the suspended sediment hysteresis you already mention. See, for example, Allen (1974), Nanson (1974), Walling (1977), Dunne and Leopold (1978), Ried et al (1985), Dietrich et al (1989), Milhaus and Klingeman (1992), Moog and Whiting (1998), and Humphries et al (2012).

A mechanistic explanation should be provided for the inverse relationship between impactor size and frequency (pg. 1226, lines 25-end). It may be useful to cite Hertzian impulse theory and the relationship between contact time and impactor mass (see, for example, McLaskey and Glaser, 2010).

We included some of the suggested references to highlight the hysteresis in the relationship between bedload sediment transport and hydrodynamics. We also added some details to explain a relation between frequency content and grain size.

3.1 Characterization of seismic signals: Methods
Discussion of other methods, or analysis methods once data is in spectral form, would also be helpful. For example, it might be worth mentioning here that several studies (e.g., Hsu et al, 2012; Roth, 2014) have found qualitative results (ie, detecting bedload transport) using only seismic amplitude data (hourly Hilbert envelope within a given bandwidth).

Yes, we do agree. This was actually the case, and as mentioned in the manuscript, for the first study illustrating a possible survey of bedload transport with seismic instruments (Govi et al., 1993).

This also seems like an important place to discuss the Green's function, or other approaches to empirically dealing with attenuation. Tsai et al (2012) would be a good reference to cite here. Also, for geomorphologists who may be entirely unfamiliar with the processing of seismic data, things like instrument response removal may be worth a brief mention. The uninformed reader may otherwise get the impression that you can just taper and FFT raw seismic data from a seismometer.

Processing that is relative to seismometry, like the removal of instrument response, is not crucial to demonstrate the interest of seismic monitoring of geomorphic processes. Therefore, we do not choose to develop the principle of deconvolution as most seismic books have a seismometry chapter that gives all the details of it, but we now mention this stage and some references in the revised manuscript.

3.2.1 Rockfalls, landslides and river channel processes
Pg. 1230, lines 25-26: Citing Huang et al (2007) here seems a little disingenuous, as that study specifically referred to the signal generated by debris flows and rocks dropped on dry stream beds (not active fluvial transport of coarse bedload grains). Should cite Gimbert (2014) in the discussion of water signal spectra.

The citation Huang et al. (2007) is here to mention the relation between frequency characteristic and grain size. We think that a debris flow is a good representation of an active transport of coarse grains. We added a discussion about a water signal spectrum, see comment [2].

As this is your first detailed discussion of the range of different geomorphic processes that have been seismically monitored, each topic really warrants a much more comprehensive list of citations. I would suggest adding a section (or including in the introduction) with more general comprehensive background and citations for each process mentioned. For example, the following citations might be appropriate to include: debris flows (LaHusen, 2005; Suwa et al, 2003; Arattano, 1999; Marchi et al, 2002); landslides (Sutherland, 2002); bedload transport (Tsai et al, 2012; Gimbert et al, 2014). The bedload transport citations listed here are especially relevant in this section given your discussion of bedload and water frequencies. Only one paper is cited for rock avalanches. Snow avalanches (Vilajosana et al, 2007; Cole et al, 2009) might also be worth mentioning.

As suggested, we added more references to describe this section.

3.2.2 Tectonic and meteorological events and anthropogenic acticity
You might also want to mention the signal generated by ocean waves (Adams et al, 2002), which is important in coastal or near-coastal environments.

OK

4.1 Detection
The final discussion of setting STA and LTA for debris flows or floods (pg. 1235, lines 18-22) could be expanded to provide more concrete details for different kinds of geomorphic processes. Also, giving specific numbers as examples (ie, 1-2 minutes for the STA and 30-90 minutes for the LTA) is unhelpful given the large variance possible in real-world floods, which can last from hours to multiple days. Stating example values normalized by or in terms of characteristic flood time scales (e.g., total flood duration or the time between base level excursion and flood peak, etc.) would be more generally useful.

This suggestion for STA/LTA detection for debris flows or floods was an illustration of different signal lengths that require a different parameter. In the case of such long duration event, a simple threshold of amplitude for a given duration is enough. This is how a warning system is triggered in debris-flow catchment.

Overall, this section is very helpful, but could use more citations (ie, specific examples of studies that have used this approach), and could be much more expansive in scope. If you're going to call the whole section "Detection and location of geomorphic processes," it would be nice to see a broader collection of detection methods/examples. What about unique frequency signatures? Even the hysteresis in bedload transport studies might be considered a detection method...

We do not want to introduce a discussion about the detection of hysteresis curve in this section. Our purpose is to detect geomorphic events that are not an integrative process of geomorphic activity.

4.2.1 Discrete geomorphic events
pg. 1237, lines 19-21: Physical explanation for why polarization becomes highly variable for high frequencies. The last point discussed here (pg. 1237, lines 22-26) on impulsative first arrivals, should be discussed in a little more depth, perhaps through a brief summary of the actual migration technique used in the cited study.

We added new references to illustrate these points.

4.2.2 Continuous geomorphic events
pg. 1238, line 13: How do you locate the coherence?

We refer the readers to the study of Burtin et al. (2010) for a detail description of the method.

5.3 Array geometry: local to regional 2-D array geometry
pg. 1243, lines 18-22: Shouldn't all of this depend on the frequency you're trying to capture? Giving aperture sizes with respect to the signal you're interested in monitoring, with physical explanations, might be more helpful than absolute values.

We chose to illustrate our descriptions with observations from several seismic networks that allow us to give first order values for catchment size and number of stations. Of course the frequency content and the wavelength have an influence, but in the different studies (with different apertures) that we mentioned, the frequency content is similar.