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Interactive comment on "Seismology of the Oso-Steelhead landslide" *by* C. Hibert et al.

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Dear Editor and Referees,

We are very grateful for the reviews and comments you provided on our paper now entitled "Dynamics of the Oso-Steelhead landslide from broadband seismic analysis". In this final comment we provide answers to each point raised by the Referees and indicate how we changed the manuscript to take into account these suggestions. We also respond to the three main issues raised by Kate Allstadt in her short comment.

The answer to a specific comment is given after repeating the comment and is *italicized*.

Answer to Referee #1 comment:

This paper uses seismic signals and field observations of the Oso-steelhead landslide and estimated the volumes of two failures. This paper includes important information for the study of landslide dynamics and I think it is appropriate for publication of Natural Hazards and Earth System Sciences with minor modification. Here are some minor comments:

I suggest to change the title to more appropriate one to describe the point of the paper. The current title is so general that readers may have difficulty to get the main subject.

We agree and we change the title to: 'Dynamics of the Oso-Steelhead landslide from broadband seismic analysis'

Page 7310, lines 18-21 Add references.

We added Keaton et al. (2014) and Iverson et al. (2015).

Page 7312, lines 9-19 It is hard to follow which peaks you are mentioning here. Please mark in the figure, or write precise time of the peaks. The term of "closest stations" in lines 15-16 is confusing, since you are showing only one record here. I suggest to rephrase the sentence.

Following this comment we added some label on the figure to mark the second event onset. We rephrased as suggested: "On the closer station (JCW), [. . .]"

Page 7312, line 19 Are you using period shorter than 30s? Contradict with “we restrict our analysis to signals with periods longer than 30 s” at page 7313 line 22. Add a description of the filter you used.

This is indeed confusing, as we mention here only our analysis of the long-period seismic waves. We added precision on the frequency range used to filter the long-period signal.

Page 7312, line 23-24 I am not sure where are the onsets of the first and second events exactly, but both onsets seems to be emergent for me (especially in Fig.3).

Indeed, but the first event onset seems more emergent than the one of the second event, which is what we have written.

Page 7313, line 24 “partially overlapping” How many seconds do you overlap the triangles? Please specify the shifting time.

We overlap the triangle by their half duration, i.e. 10 seconds. We added the precision in the text.

Page 7314, lines 3-5 As you used isosceles triangles for a source time function, it seems the end of the force component is always zero. I do not understand why you are adding an extra constraint here. If I do not understand correctly, please rephrase the sentence so that it is clear for readers.

At the moment mass stops to slide, i.e. is not accelerating or decelerating, there is no

force exerted by the mass on Earth and therefore the force should be zero.

Page 7316, lines 13-15 Not clear in the Figure which two peaks you are talking about.

We modify figure 3 to mark clearly the two peaks aforementioned.

Page 7317, lines 4-5 I suggest to specify that the amplitude is that of the first event: “The amplitude of the long-period signal from the first event”

We agree with this suggestion.

Page 7317, lines 16-end The authors estimate the volume of the second event from the high-frequency seismograms here. However, there are some points which are not logical for me. First of all, it is not very clear for me whether these events are rock falls or not, and the mechanisms to produce the high-frequency seismic waves are same with these events and rock falls of which the earlier studies show the empirical relationship between seismic energy and volumes. Authors used the results of earlier studies to validate the volume scaling to the seismic energy, but they did not show the correlation in this limited frequency range. I suspect the seismic energies are depending on the type of events, mechanisms, and size of the events. Therefore, the reasons written here does not support the volume scaling very well.

The previous studies, on which we based our analysis, studied rockfalls that have a dominant mechanism which is granular flowing, similar to that of large landslides. We considered that we would be able to use the same approach for the Oso-Steelhead landslide if the dominant mechanism for the high-frequency seismic waves generation *is the flowing of a granular mass for both landslides. Hence, the volume scaling will work only if the dominant physical process and the source geometry of the second event is the same as for the first one (i.e. granular flow), with slope parameters that are roughly the same. If the source geometry of the second event is not the same as the first one, this seismic energy comparison is indeed not working. We modified this part to make this point really clear.*

Page 7318, line 4 :The locations for the departure zone are rather assumed by authors than identified.

Yes indeed. We modified the sentence to make this clear.

Fig 3: Can you mark onset time on Fig 3? (17:37:22 and 17:41:53)

We added the onset time on Fig3.

Fig 5: I do not understand the meaning of red curve (product of the opposing force and the normalized moment) and possibly was not mentioned in the text. Please add an explanation in the text.

The red curve gives the direction of the acceleration with respect to the path traveled by the sliding mass. If this curve is positive, the acceleration is going in the direction of sliding, thus promoting movement along the path. If its value is negative, it is going in the opposite direction of the movement and thus is causing the mass to decelerate along the path. This is indeed a precision that must appear in the text and that we added.

Answer to Referee #2 comment:

The work will attract also landslide specialists, who may have limited knowledge of seismology 101. Therefore, some terms like, for example, short- and long-period seismic waves (or seismic signal), should be clarified (associate T and frequency band). Provide some basic background information regarding the local geology (in particular, lithologies involved) and landslide mechanism/type.

We added the information on the period and frequency range of the long-period seismic signal we use and provided information on the lithology involved.

p. 7310 line 16 – ground observations – not entirely clear
p. 7312 line 19 – long-period surface waves ($T<30s$)?
p. 7313 Eq. 1 – explain symbols
p. 7314 line 12 (and Fig. 5) departure zone or departure area (p. 7318) – I suggest to use “source area”
p. 7316 line 15 – 45 s? perhaps 35 s
p. 7316 line 26 – Multiple time-overlapping breakaways – not entirely clear

We revise the manuscript by taking into account all these suggestions.

Answer to Referee #3 comment:

Major comments:

The volume of the second event is estimated based on the comparison of the energy of the 2 signals in the frequency band 3-10 Hz. I have two comments regarding this process:

The authors assume the proportion of potential energy dissipated in the form of seismic energy is constant. Various studies (Deparis et al., 2008, Hibert et al., 2011,...) indeed tried to fit the observed or modeled potential energy with the seismic energy by a linear fit, but the dispersion of the data around this fit is important.

Hibert et al. (2011) show that the seismic energy is linearly proportional to the potential energy, based on observed and modeled scaling laws between the duration of granular flows and their seismic energy and potential energy loss. A similar correlation has also been observed for granular flows occurring on the Soufriere Hill volcano (Montserrat) by Levy et al. (publication submitted). The dispersion of the data comes from two sources: i) the large uncertainty on the parameters used to compute the seismic energy which is really sensitive to the velocity and the attenuation, for which no accurate model exists for high-frequency seismic waves. ii) The difference between each event source dynamics as well as the topography along the path taken by the granular flow (the numerical modeling presented in Hibert et al. (2011) shows this topography effect). Therefore, for the comparison of the seismic energy of the two Oso events to provide relevant information on their respective potential energy loss, the two events should be granular flows, share a similar run-out path geometry and average slope angle, and occur close to each other so the seismic waves propagate within layers with the same properties. If these three assumptions

are not verified, the inference of the volume of the second event might indeed be incorrect. As asked by Referee #1 and K. Allstadt we added this precision to the manuscript.

The authors claim the 3-10 Hz frequency band is less sensitive to the topographic effect than the 1-3 Hz, based on a previous study (Hibert et al., 2014) realized over another site. First of all I don't see in the mentioned publication where does this come from. Second, all previous studies on that subject show that the whole 1-8 Hz band is affected by the topographic effects (Spudich et al., 1996; Bouchon and Barker, 1996; Buech et al., 2010; Maufroy et al., 2014). The choice of the bandwidth must be clearly justified. I would suggest making a sensitivity analysis of the volume estimate to this bandwidth choice.

The studies cited by Referee #3 present results on the sensitivity of propagating seismic waves to the topography (amplification, scattering, etc), which is not what we meant by topographic effect. In our case we talk about the effect on the seismic signal of a source propagating on a rough topography. We refer to our study of the Bingham Canyon Mine collapse (Hibert et al., 2014) because in this study we have shown that a second high-amplitude arrival is observed when the sliding mass has hit the mine pit walls. We observed that this second burst of energy has slightly greater amplitude with respect to the rest of the signal in the 1-3 Hz than in the 3-10 Hz. For the Oso-Steelhead second landslide we show that the two amplitude peaks observed in the 1-3 Hz frequency band disappear in the 3-10 Hz. We do not know the source of these two peaks, but one of our assumptions is that they might be generated by the mass flowing over some topographical features. Because our analysis of the seismic energy is based on the assumption that the dominant source of the seismic signal is the flowing of granular material, we choose to compute the energy in the 3-10 Hz, as in this band these two peaks, that are probably not related to this process, disappear. We revised the text to make this point clear.

No uncertainties is given on the inversion of the time history of each force component, and on the resulting trajectory and volumes. How sharp is the cost function (p5, lines 1-3)? Are there secondary peaks? A figure showing the cost-function versus the estimated volume would help the reader estimating the uncertainties.

Uncertainties are difficult to quantify, since the inversion is regularized and data selection is an integral part of the analysis. Different choices of regularization and data selection will lead to somewhat different solutions. The result presented in the paper is representative of the class of well-fitting solutions. Based on comparisons of inversion results with ground-truth data for tens of landslides, we find that the direction of sliding, the maximum force, and the duration of sliding are well-constrained and robust parameters. Small details in the time histories are not.

The Figures do not always illustrate the methodology used: Some Figures do not represent the signal in the bandpass used in the methodology (e.g. Figure 2d, Figure 5a). Why such a narrow bandwidth (0.03-0.04 Hz) is used in the Figure 2d? Figure 4a mentions 'long-period', but does not precise the bandpass. The Figure 4a does not show all the seismograms available (station D04D).

We modified the figures to better illustrate the text as suggested. We also indicate the filter used whenever necessary.

Minor points:

Page 1, line 22: the distal deposits traveled more than 1.1km. Please clarify.

We agree and we modify this sentence accordingly.

Page 2, line 27: how do you define "strong"? Does it mean that a previous signal also exists, as proposed by the study of Iverson et al., 2015?

We do not understand to which "previous signal" Referee #3 refers to. There is no mention of a signal prior to the one generated by the first event in the study by Iverson et al. (2015). If Referee #3 refers to the first force peak which is aseismic in the high-frequency, we discuss this point below.

Page 4, line 1: It is not clear that the part 3 "landslide force history" only refer to the first event. Make it clearer in the section title.

We revised the section title as suggested.

Page 4, lines 2-4: could you give a reference for that sentence?

We added references.

Page 4, lines 23-24: this sentence requires more explanations or at least a reference.
Page 4, lines 24-25: Where do these values (8 triangles, 10s) come from? Did you try different values?

Yes, we usually test manually different initial model by adjusting the number and the *half-duration of the isosceles triangles used. We added a reference to Ekström and Stark (2013) and we specify how we proceed to obtain the best inversion of the force history*

Page 6, line 22-24: Authors mention that the 'interpretation is not sensitive to small variations in the assumed propagation velocity'. Is the 1.8 km/s found by Iverson et al.,2015, compared to the 1.1 km/s found here considered as a 'small' variation?

The distance between the landslides and JCW is approximately 12 km. A velocity of 1.8 km/s instead of 1.1 km/s would shift our force history by approximately 4.2 seconds. We considered that 4.2 seconds compared to the total duration of the short-period seismic signal (100 s) and the force duration (90 s) is small, and will not impact significantly our joint interpretation of the force and the short-period seismic signal.

Page 7, line 19-22: I am not sure that what has been observed for one site study can be transposed to other areas. Page 8, lines 19-21: The choice of this 3-10 Hz band is not convincing. See the major comment.

(See answer to major comment b)

Figure 4b: could you discuss why the forces estimated have amplitudes 3 times greater than in the study of Iverson et al., 2015?

(See answers to Kate Allstadt's comment)

Answer to Kate Allstadt comment:

We answer thereafter to the three most important issues raised by Kate Allstadt in her comment.

Initial failure sequence:

The reconstructed force history of the Oso-Steelhead failures presented in Iverson et al. (2015) (Figure 5) shows a first cycle of 60 seconds of horizontal acceleration and deceleration without generation of short-period seismic waves. Iverson et al. (2015) interpret this first aseismic cycle as the acceleration of a relatively coherent mass of material. While it has been observed that the slow emergence of the short-period seismic signal occurs during the time the landslide reaches its maximum acceleration (Allstadt, 2013; Hibert et al., 2014), which is thought to be related to the progressive fracturing of the mass, there is for the moment no observations that corroborate a strong horizontal acceleration of the sliding mass without any short-period waves generation. In the work published by Schneider et al. (2010), a correlation has been found for two rock-ice avalanches between the amplitude of the envelope of the short-period waves and the basal friction rate, and to a lesser extent with the center-of-mass momentum. A similar observation has been made by Levy et al. (paper submitted) for rockfalls that occurred in Monsterrat, on La Souffrière volcano, and for at least 14 large landslides we are currently working on (paper in preparation). Dammeier et al. (2011) and Hibert et al. (2011) have also shown that the energy of the short-period waves generated by rockfalls increases with their volume. As described by K. Allstadt, a small event whose size is estimated to be orders of magnitude smaller than the first Oso-Steelhead failure has generated short-period waves recorded at station JCW, located 12 km from the source. It is therefore difficult to understand why the hypothetical first horizontal acceleration cycle described by Iverson et al. (2015)

would not have generated any short-period waves, as a large mass having a horizontal acceleration-deceleration cycle that lasts 1 minute would be able to generate much stronger short-period seismic signal than a smaller event, according to all the studies aforementioned.

As described by K. Allstadt in her comment, the discrepancy between the force history of the landslide presented in Iverson et al. (2015) and ours may come from the different methods used but also from the different frequency band considered. While Iverson et al. (2015) have used seismic signal filtered between 30 and 60 seconds, we choose to work with seismic signals filtered between 40 and 150 seconds. Our choice is guided by the duration of the loading and unloading cycle of the Earth by the sliding mass. This duration of the source is generally exceeding a minute for large landslides. Several studies have also shown that the duration of the short-period seismic signal is correlated to the propagation time of the mass (Surinach et al., 2005; Deparis et al., 2008; Dammeier et al., 2011; Hibert et al., 2011), which, in the case of the first Oso-Steelhead landslide, would imply a duration of the source of approximately 100 seconds. Choosing to filter the seismic signal in a period range that does not encompass the whole loading-unloading cycle duration will prevent capturing this cycle in the inversion process and therefore can lead to spurious results. To demonstrate this, we were able to simulate the analysis performed by Iverson et al. (2015) and to retrieve the force history produced by their inversion method. We show that the first force cycle of 60 seconds, interpreted as the first stage of the sliding by Iverson et al. (2015), is actually an artefact (Gibbs phenomenon) related to the narrow frequency band used (T=30-60s) to filter the seismic signals. Our analysis also explains why the maximum force found by Iverson et al. (2015) is one fifth of the maximum force given by our inversion. This shows that the issue on the frequency band choice is critical for landslide seismic source inversion and should be brought to the attention of the readers, so we decided to integrate this analysis into the manuscript as an appendix. The details of our analysis are provided therein.

Our solution for the force history, showing a 90-s loading-unloading cycle, is consistent with the short-period waves records for the first event, in terms of timing (it starts at the same time) and duration, but also regarding the correlation between signal envelope amplitude and inferred kinematics parameter of the centre-of-mass.

Interpretation of the second high frequency signal:

In Iverson et al. (2015), the signal is filtered between 30 and 60 seconds, while in our study, we considered seismic signal filtered between 40 and 150 seconds. This may explain why we do not observed long-period seismic signals for the second event. The seismogram recorded at A04D illustrates well the fact that we do not observed long-period signal in the frequency band we choose. We have added the period range we used to filter the signal in the revised manuscript.

K. Allstadt states that the deconvolved force history for the second landslide presented in Iverson et al. (2015) shows more vertically oriented forces. However the force history of the second landslide presented on Figure 5 in Iverson et al. (2015) has an amplitude at the noise level and therefore proposing an interpretation for the second event dynamics based on this result is difficult. Nevertheless, the force history presented by Iverson et al. (2015) shows that the north component as almost the same amplitude as the vertical one, and that the east component of the force has a peak amplitude greater than the vertical one. If this inferred force history is correct, this would favour the assumption of a more horizontal acceleration and not a vertical one as stated by K. Allstadt. The run-out of the deposit identified as the one of the second landslide by Iverson et al. (2015) suggests that this sub-event did not have a significant horizontal movement, which is in contradiction with their force history. We think that this deposit is the result of other late small events, as the one filmed and mentioned by K. Allstadt, and is not at the origin of the second high-amplitude signal of the Oso-Steelhead sequence.

Keaton et al. (2014) present strong evidences based on the morphology and the lithology of the deposit and on the displaced trees orientation that the overall deposit was the result of two successive slope failures. Uncertainty remains on where the second event initiated. As stated by K. Allstadt and also by Referee #1, if the geometry or the dominant seismic source of the second event differs from the one of the first event, our approach based on the seismic energy is indeed invalid. However, the morphology of the second event deposit identified by Keaton et al. (2014) supports our assumption that the dynamics of the second slope failure is close to the one of the first landslide. We have added text in the manuscript stating the assumptions under which the volume estimate of the second event is valid.

Trajectory of the landslide:

Determining the true initial and final positions of the centre of mass and its position over time is not trivial and depends on which scenario the analysis is based. Our estimation of the centre-of-mass positions is also based on satellite imagery and post-failure Lidar acquisition. We do not have access to the data computed by David George to which K. Allstadt refers, and therefore we cannot argue on whether this estimation of the run-out distance of the centre-of-mass is valid or not. However, we were able to extract the digital deposit thickness models from the figure 8 presented in Iverson et al. (2015) (Figure 1). From this model we computed the position of the centre-of-mass of the deposit (Figure 1 A - white diamond), which is close (approximately 100 meters) to the one we choose as the terminal point of our inverted trajectory (red curve on Figure 1 C). From this model we do not understand how the

run-out distance of the centre-of-mass could be 400 meters as stated by K. Allstadt. Our estimate of the run-out distance is also supported by the fact that we found a mass in agreement with the values obtained by Iverson et al. (2015) using other methods. The discrepancy between the maximum amplitude of the force presented in Iverson et al. (2015) come from their choice of the frequency range used, as discussed above and demonstrated in the appendix added to the manuscript.

Figure 1 caption: A) Oso-Steelhead deposits thickness model extracted from figure 8 published in Iverson et al. (2015) and estimated center-of-mass of the deposits (white diamond); B) Residuals of the least-square method used to extract the thickness model from figure 8 in Iverson et al. (2015); C) Georeferenced original figure 8 from Iverson et al. (2015) with the trajectory inferred from our inversion of the first Oso-Steelhead landslide force history (red curve), and the computed center-of-mass (white diamond).

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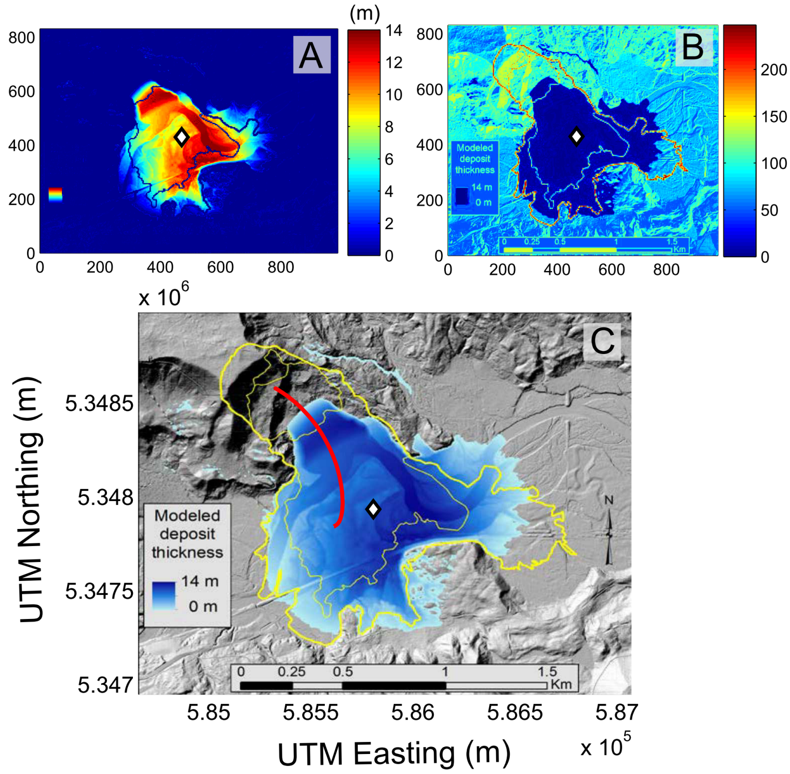

(m)
A
B
C
Modeled deposit thickness
14 m
0 m
0.25
0.5
1
1.5
Km
x 10^6
UTM Northing (m)
UTM Easting (m)
x 10^5
N

Fig. 1.