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Discussions

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

Anonymous Referee #3

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General

In this study, the authors focus on the signal generated by the catastrophic Oso landslide, USA, to reconstruct its volume and kinematics. The topic is of interest for both the landslide and the seismological communities. In particular the seismograms exhibit 2 successive different signals and their comparison is a good case-study for retrieving the properties of the 2 different flows. The volumes of the 2 flows are estimated based on the analysis of both the long- and short- period signals, that provides another point of view than a recent study by Iverson et al. (2015). I also feel the manuscript is concise, well written and organized, even if I suggest some minor points in the following to improve the reading.

Despite all these positive points, I have a major concern coming from the volume estimation of the second event. Indeed this estimate is based on not very convincing

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assumptions and the arbitrary choice of some parameters. I really feel this study can be suitable for publication once the technical details clarified, and the results discussed in comparison with the previous study by Iverson et al. (2015).

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:

a- The authors assume the proportion of potential energy dissipated in the form of seismic energy is constant. Various studies (Deparis et al., 2007, 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.

b- 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.

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.

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

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4a mentions 'long-period', but does not precise the bandpass. The Figure 4a does not show all the seismograms available (station D04D).

Minor points

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

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

Page 3, line 12: how do you define 'strong'?

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.

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

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?

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?

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.

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

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 7309, 2014.