Nat. Hazards Earth Syst. Sci. Discuss., 2, C3274–C3283, 2015
www.nat-hazards-earth-syst-sci-discuss.net/2/C3274/2015/

Natural Hazards and Earth System Sciences
Discussions

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

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Received and published: 3 February 2015

In this study, Hibert et al. use the seismic waves generated by the disastrous 22 March 2014 landslide near Oso, Washington to investigate the dynamics of the event by combining long period source inversions and short period analysis. Overall the paper is concise and well written and takes an innovative approach by integrating both high- and low-frequency seismic data together in the interpretation. It also demonstrates the useful prospect of resolving a trajectory of the center of mass of the landslide from a few traces of noisy seismic data.

However, the main point of discussion that I would like to address stems from differences between the findings by Hibert et al. and the findings of a recently published multidisciplinary paper on the Oso landslide (Iverson et al., 2015). I am a co-author on the paper and performed the seismic analysis, which was similar to that of Hibert et al.,

yet the interpretation of how the event unfolded differs significantly between the two. In my opinion, this stems largely from a difference in information, some of which Hibert et al. unfortunately did not have access to prior to submission because it was just published in Iverson et al. (2015). However, differences also seem to stem from technical details of the methods and data used. Typically, such details are only of interest to seismologists, but in this case they make a substantial difference in understanding how the event unfolded and how it became so disastrous. For that reason, they are worthy of discussion here.

The crux of the difference lies in two points 1) Iverson et al. (2015) found a multi-stage initiation of the main event, while Hibert et al. did not and 2) Iverson et al. (2015) found the second high frequency event to be from a significantly smaller debris fall whereas Hibert et al. estimate it to be a substantial mass. I detail these two points below, then follow with an additional comment regarding the trajectory estimation made by Hibert et al. and some minor comments.

Initial failure sequence:

Regarding the initiation sequence, in their long period seismic inversion, Hibert et al. found a single long-period cycle of acceleration/deceleration associated with the first high frequency seismic signal whereas Iverson et al. (2015) found two shorter period cycles, the first starting tens of seconds before the high frequency signal emerged from the noise that was interrupted by another stronger cycle of acceleration/deceleration and was accompanied by higher frequencies. This two-stage initiation was corroborated by geologic evidence and an account from an eyewitness standing on the western edge of the slide as the event unfolded (Iverson et al. 2015). Additionally, the undrained loading from the compressional force of a second event onto an already mobilizing slope is a potential explanation for why this event was so mobile and disastrous (Iverson et al., 2015). Missing this multi-stage initiation means missing a potentially crucial aspect of event dynamics.

The two different inversion results are not necessarily contradictory. Hibert et al. used less and different seismic data than Iverson et al. (2015), but there are also methodological differences that influence what is resolvable. Hibert et al. modeled the forces using overlapping isosceles triangles with coarse time resolution ($\sim$20 s triangle width overlapping by 50%), while Iverson et al. (2015) effectively deconvolved the forces from the seismic data and used a higher sample rate (2 s). The latter approach has the advantage of resolving details such as the occurrence of sub-events (e.g. Allstadt, 2013), but can also be more sensitive to noise, and consequently Iverson et al. (2015) were not able to resolve signal at periods as long as the Hibert et al. solution for the Oso event. As was suggested to me by Ekstrom in a discussion we had a few weeks ago, in a spectrally complete solution, the shorter period solution that is resolvable by the methods used in Iverson et al. (2015) could actually be superimposed on the longer period solution that is resolvable by the methods used in this study. In fact, this superposition was exactly the case for a much larger event, the 2010 Mount Meager landslide: there were three smaller subevents closely spaced in time that showed up in the seismic inversion when a broader band of frequencies was used, but were not distinguishable as distinct events if only the longest period seismic waves were used (Allstadt, 2013). However, if this is also the case for the Oso landslide, it means the validity of the assumptions behind the trajectory calculation performed by Hibert et al. (single failure, constant mass, sliding block approximation valid) would need to be reevaluated for an accurate interpretation of what it actually reflects.

Interpretation of second high frequency signal:

Hibert et al. state that there were no distinct long period signals associated with the second conspicuous high-frequency event and therefore they were not able to estimate the forces of it with their methods. However, there were resolvable long period signals visible above the noise on some stations and the forces were resolvable using the deconvolution method and computing the uncertainties due to noise to distinguish the forces from the noise (Iverson et al., 2015). These forces were much shorter timescale,

smaller amplitude, and more vertically oriented than the forces associated with the first event, which, along with other evidence, led to the conclusion that the second event was generated by a secondary debris fall that left deposits found high in the source area (labeled on Figure 1 in Iverson et al., 2015). Though these deposits are very small compared to those of the main event, the debris that formed the deposits fell almost vertically off the headscarp and may have had some component of free-fall, which would mean a very high acceleration during the fall and a high deceleration upon impact. Thus, even if the mass were small, the force would still be significant. Moreover, the event had a high degree of disruption based on the deposit morphology, which could be responsible for the strong high frequencies.

The interpretation of the second high frequency event of Iverson et al. (2015) explained above differs from that of Hibert et al., which closely follows the main hypothesis included in Keaton et al. (2014). Their idea, as I understand it, is that the material above the "collision" zone (lower edge marked by B on Hibert et al.'s Figure 5) was associated with the second high frequency signal. However, this interpretation is not consistent with the size of the respective deposits, nor with the seismic signals or the long period forces inferred by Iverson et al. (2015) from their inversion. The bulk of the failure mass is above the lower edge of the collision zone, and this material would have had to come down second, yet the second high-frequency event generated much lower amplitude yet shorter timescale forces than the first, implying that the first event had a much larger volume and more significant coherent movements.

As supporting evidence for the Iverson et al. (2015) interpretation that the second high frequency event was much smaller, about a month after the main event the Snohomish County Engineers captured a video of a piece of the headscarp on the order of 10 meters across breaking off with a significant free-fall component. The energy (integral of the envelope) of the resulting 1-10Hz seismic signal recorded at the JCW station was about 1/60th that of the main event on March 22nd and about 1/30th that of the second high frequency event. The volume of the collapse caught on video was far less than 1/60th the mass of the main event but could be close to 1/30th of the mass of the secondary debris fall deposits, which would have had very similar failure dynamics. This example illustrates that the style of landsliding is very important in determining the seismic signature and that the energy in the high frequencies does not scale linearly with mass if the style of the event is different. Norris (1994) also found this to be the case - high frequency energy only scales linearly with mass for similar styles of landslide occurring on similar slopes. It is unlikely that the landslides associated with the two high frequency events met these characteristics. For these reasons, I don't agree that the method used by Hibert et al. to estimate the volume of the second event is valid in this setting.

Regarding the trajectory of the landslide:

The center-of-mass trajectory computed by Hibert et al. by double integrating the horizontal components yields a center of mass (CM) path that is significantly longer than what is supported by ground and satellite observations. Much of the landslide mass remained stranded in the source area so the CM didn't move nearly as far as the thinner distal deposit. David George (personal communication, 2015) computed the location of the CM of the displaced mass before and after the slide using pre- and post-event digital elevation models assuming the failure surface from Iverson et al. (2015). He found that the CM stopped just north of the location of the channel before the slide blocked the river. This CM path from remotely sensed topographic observations is about 450 meters long, while Hibert et al. found the CM path length to be 800 m. If estimating the trajectory through double integration is valid here, then either the amplitudes of the horizontal forces are too high by about 80%, or the mass assumed in the trajectory calculation would have to be about 80% larger for the seismically-derived trajectory to be comparable to the ground observations since at long periods the force scales like Force = -Mass * Acceleration. However, it is unlikely the uncertainty of the mass is this great. Iverson et al. (2015) estimated the error in the volume estimate to be about $\pm$13%.

Additionally, as mentioned earlier, Iverson et al. (2015) also included an inversion of the long period seismic waves as part of the analysis and found the peak amplitude of the forces to be much lower than the results of Hibert et al., at 4.7 x 10ˆ9 N. The result fits data from the same stations used by Hibert et al. plus 14 more (18 total stations, 20 channels) with a variance reduction of 74%. Iverson et al. (2015) also computed uncertainties due to noise to infer a peak confidence interval of 3.8 – 5.4 x 10ˆ9 N. It's possible that the difference in methods described earlier is a source of some of the discrepancy.

Specific suggestions regarding the seismic inversion:

-Include more channels of data in the inversion and show how the result is affected and whether it is stable.

-Determine whether the force history from the best solution can reproduce data that were not used in the inversion.

-Calculate the uncertainties of the result (e.g. I use a modification of the jackknife technique, though it won't work well for only 5 channels of data).

-Decrease the half width of the triangles and see if shorter period details such as from sub-events can be resolved in the result.

-Include enough details on the math behind the inversion methods and the constraints that are applied for them to be reproducible. To my knowledge, sufficient details of the methods used have never been published.

-Show the vertical trajectory

Other comments:

Figure 1 – I suggest changing the legend to show which stations are broadband and which are short period since the broadband instruments recorded both the short period and long period signals. Also, many more stations recorded the landslide than are

shown, so the selection shown on the map seems arbitrary as most are not used in the study.

Figure 4 – The seismic signals on Figure 4a start ∼40-50s before the force history on Figure 4b starts – this seems longer than what could be attributed to an acausal zero-stage filter. Is there an explanation?

Page 7310:

L10-11 – I don't agree with this reasoning. Just because there are three pulses of energy at high frequencies doesn't mean the second failure was more complex. It may just be that more was going on simultaneously during the first event so discrete pulses were not identifiable.

L11-12 – There were long period waves recorded during the second event, they just aren't visible above the noise level on all stations.

L13-15 – As mentioned earlier, this approach to estimating the volume is not valid unless you know the landslides had similar failure styles.

L21 – Reference is needed for the six episodes of collapse since 1955 statement.

L21 – Reference is needed for the statement about the slide being preceded by several days of rain. Iverson et al. (2015) includes a detailed statistical analysis of the preceding precipitation.

L25 – Incorrect reference: Keaton et al. (2014) did not compute the volume of the 2014 slide. Rather, they obtained and referenced the number in their report from the USGS website, but now the appropriate reference for this is the Iverson et al. (2015) paper. This should be changed throughout the manuscript.

Page 7312:

L20-22 – This statement is not accurate, the seismic signals from this event were recorded on more than 5 broadband stations further than 140.8 km away. See Iver-

son et al. (2015). Also, there was a distinct long-period signal associated with the second event, as mentioned earlier.

Page 7313:

L3 – Clarify that this long period signal is not observable for all landslides, just those with sufficiently large masses and sufficiently fast accelerations.

L7 – Define Fs as the forces on the slide more clearly.

L25 – By how much do the triangles overlap?

Page 7314:

L1-3 Quantify the fit of the solution (e.g. variance reduction or rms difference)

L1-3 Need more detail on how the data were chosen and how it was processed. Specifically, elaborate on why just 5 channels from 4 stations were used when the signal was recorded on at least two-dozen broadband instruments.

L7 – Specify that this is large-scale sliding, movements that do not generate strong coherent forces could still be occurring.

L10 – The trajectory does not fit the field observations (see main comment 1).

L17-18 – Change reference to volume. See above.

L19-26 – Need to reassess these numbers given that the trajectory is not scaled to fit ground observations (see main comment 1).

Page 7316:

L22 – This statement requires justification. I do not expect the exact frequency band generated by different processes to be transferable so directly between sites. The frequency band recorded at a station depends on many factors such as attenuation/subsurface structure, how far from the source you are, the scale of the actual topography barriers being overridden and at what velocities they are being overridden.

Page 7317:

L13-15 – The peak acceleration of the second event is likely higher than the first event because the evidence suggests it was a more vertical collapse (see main point 2).

L16-19 – I don't agree that this is a valid statement or approach to estimating the mass of the second event, see earlier comment.

Page 7318:

L5-6 –There aren't any obvious geologic deposits that support a departure area of a slide from the top edge of the slump block (labeled departure area B). This proposed departure area should be clarified and potential deposits should be identified.

L14-16 – See earlier comment regarding the second event.

References

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