Authors' reply to comments of the reviewers

Original title:	Dynamics of the Askja caldera July 2014 landslide, Iceland, from seismic signal analysis: precursor, motion and aftermath
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We thank the reviewers for their constructive comments and suggestions. Please find in the following the reviewers' comments followed by our reply.

Review 1 by J. Caplan-Auerbach

Received and published: 5 January 2018

This well-written paper describes a prolonged seismic signal associated with a large landslide in Askja caldera, and uses those data to describe the failure sequence. The authors use a variety of techniques to analyze the dynamics of the slide, including a precursory sequence and series of "afterslides". Overall, the paper provides an interesting description of this event, and much of the analysis is compelling. The identification of this signal is itself an important contribution, particularly given recent events such as the 2017 Nuugaatsiaq landslide/tsunami. However, I believe it requires more explanation and justification before it can be accepted for publication.

The landslide signal is preceded by a prolonged tremor sequence, which exhibits harmonics and apparent gliding. Immediately prior to the slide itself, the tremor stops and there is a period of quiescence. This is reminiscent of the signal recorded prior to eruptions at volcanoes such as Redoubt, and thus it is no surprise that the authors invoke a similar mechanism for the tremor signal (repeating, stick-slip events that occur at regular intervals). Furthermore, tremor-like signals have been recorded prior to other landslides, notably in Alaska. That said, I found the analysis of the tremor and gliding to be somewhat weak. The authors use changes in the timing between stick-slip events as an explanation for the observed gliding. To explain the fact that both increases and decreases in frequency are observed, they propose that there are two discrete patches of slip that behave differently (one accelerates while the other slows). I don't deny that this is a possible model, but I don't find the justification that compelling. First, it isn't clear to me that the observed frequencies are actually harmonic. There is clear upgliding, but it is only a single frequency, and the downgliding is subtle and not obviously showing overtones.

Reply: We added a zoom-in to Figure 6 and Figure 8 showing the up- and down-gliding spectral lines in more detail.

The modeling shown in Figure 8 does indeed confirm that repeated similar events can present as gliding, but they bear little resemblance to the gliding observed in the precursory tremor. Are the frequencies of observed and synthetic signals the same? Nowhere do the authors state which frequencies they believe represent up- and down-gliding, so it's difficult to tell.

Reply: We've modified our Figure 8 (and also our parameter choices, see Table 1) to highlight the similarities between the modelled and observed tremor. We believe that the up- and down-gliding tremor are now much more clearly shown.

The synthetics have many more overtones than the observed signals. Can the authors explain this?

Reply: It is true that some overtones that are not clearly visible in the data. The overtone labelled with a number two (Figure 8b and c) is less strongly observed than others, for example. We believe that the simplest explanation for this is that our simplified model of wave propagation fails to account for certain propagation phenomena that may diminish wave amplitudes. Wave propagation in the complicated, 3D, layered, attenuating media surrounding the Askja volcanic complex is far richer than we have attempted to capture.

The authors indicate that the model replicates the observed aseismic portion of the signal, but without knowing at what time the landslide would initiate in this model it's hard to tell if this model fits the data.
Reply: Our simulations have not attempted to couple the seismic tremor to the force-balance of large-scale land sliding motion. We find this to be a fascinating idea, although well beyond the scope of the present modelling efforts. We do believe, however, that our revised Figure 8 now more clearly emphasizes how the ways in which our model does fit several prominent aspects of the seismic data.

I understand the rationale for the two slip-patch model, but I'm not sure I buy it. That two patches could generate events similar enough to generate harmonics, and that both of those patches would experience regular acceleration or deceleration simultaneously seems unlikely. If we had evidence of strain on the order that is required for this behavior, perhaps it would be plausible. But simply saying that this could be observed with "sufficiently high spatial resolution geodetic observations" is unsatisfying.
Reply: This comment pushed us to rethink our explanation of down-gliding tremor. We agree that deceleration before the landslide is a physically unrealistic explanation. We find that we can equally well fit the data if the second tremor patch gradually expands in size. This explanation has an additional benefit as well. Since the seismic moment (and therefore the far-field seismic particle displacement amplitudes) is proportional to the patch area, this model is capable of explaining the increased tremor amplitudes before the quiescent period. We describe these improvements in greater detail in Section 4.

Another concern that I have with the modeling has to do with the force history analysis. The authors describe their modeling and describe their results, but we never see the results of the modeling (other than the location and history). The analysis describes the direction of motion, but this isn't presented; we only see the eastern component of velocity based off of the high frequency data. This needs to be much more thoroughly presented.
Reply: We added the results of the modelling, namely the force-time history, the velocity vs. time, the displacement vs. time, and the directions of motion vs. time plots to Figure 5. We also now present the waveform fits of the recorded and synthetic waveforms in Figure S3 of the supplement.

Note that a revised version of this paper should also cite Poli, 2017 (Poli, P. (2017), Creep and slip: Seismic precursors to the Nuugaatsiaq landslide (Greenland), Geophys. Res. Lett., 44, 8832–8836, doi:10.1002/2017GL075039) as it relates very directly to these processes.
Reply: We now cite Poli's work.

It might also be useful to read Kilburn and Petley 2003: (Kilburn, C. R., & Petley, D. N. (2003). Forecasting giant, catastrophic slope collapse: lessons from Vajont, Northern Italy. Geomorphology, 54(1), 21-32.)
Reply: We also refer to Kilburn and Petley (2003) in the revised version.

These are my broad concerns. Smaller issues within the text are enumerated below:
1. The abstract can be significantly shortened. There is a lot of detail within it that is unnecessary for an abstract: there's no need to include the motivation for the study, and much of the text can be cut out (e.g. change "The excellent seismic data quality and coverageof the stations of t the Askja network made it possible to jointly analyse. . ." to "we jointly analyzed. . .")
Reply: We shortened and condensed the abstract.

2. Page 2, line 4: "often" seems like a bit of an overstatement here. Tsunamigenic landslides on volcanoes have certainly occurred, but they are not common.
Reply: We deleted the word "often".

3. Page 2, line 25: I'm not sure there's any need to discuss iceberg tremor here; it's not relevant to the study.
Reply: We deleted the sentence about iceberg tremor.

4. Page 6, lines 24-25: While it's true that high frequencies attenuate more rapidly than low frequencies, I'm not sure that this is the reason for the shape of the spectrogram (it could also be a source mechanism). Perhaps the authors could comment on whether this shape is dependent on the distance to the seismometer?
Reply: We changed the wording of the sentence and now also refer to source effects.

5. Page 10, line 25: The authors describe 3.5 km as a long distance for seismic energy to be recorded. This actually strikes me as pretty close. Perhaps the authors could comment on what distance they consider "close"?
Reply: Seismic studies we are aware of (Amitrano et al., 2005; Zeckra et al., 2015, Yamada et al., 2016) that report on cracking before slope collapses had instruments located less than a kilometre away from the collapse area. Relative to this, we considered >3.5 km as long. We clarified this in the manuscript.

6. Page 13, line 6: I recommend citing Norris, 1994 (Norris, R. D., 1994, Seismicity of rockfalls and avalanches at three Cascade Range volcanoes; implications for seismic detection of hazardous mass movements: Seismological Society of America Bulletin, v. 84, p. 1425–1939) as one of the earlier publications describing the appearance of seismic signals associated with landsliding.
Reply: We added this reference.

7. Page 13, line 24: This line about the 38 seismometers within 30 km is repetitious.
Reply: We rephrased this sentence and deleted the repetition.

8. Page 13, line 25: Unclear what the authors mean by "activated"?
Reply: We meant excited and deleted the word "activated".

9. Page 13, line 29: Is this just saying that there is an asperity on the failure plane?
Reply: Correct. We now use the term asperity.

Review 2 by Anonymous Referee #2
Received and published: 9 January 2018
This paper shows the precursory seismic signal of the Askja caldera landslide. It is well-written, and shows an interesting observation. Authors found that there were up- gliding and down-gliding signals in the seismic data before the landslide, and explained they were accelerating and decelerating stick-slip motion preceding the landslide. They reproduced this phenomenon by numerical modeling. The interpretations are interesting, but they are based on relatively strong assumptions. It is fine to use assumptions and make an interpretation, but in the discussion, the assumptions were treated almost an accomplished fact. The proposition of landslide early warning is too optimistic after finding only one example of post-report. Please explain the mechanism more carefully or tone down the succeeding discussion.

[Major comments]
One of the assumptions I was not very convinced was that they treated the tremor as continuous stick-slip events with little intervals. It may be true, but the mechanisms of tremors are still debating and there are many other interpretations.
Reply: This comment has motivated us to greatly rethink and rewrite Section 4 of our manuscript. The principal change has been to provide a clarified justification of our reasoning concerning the mechanical origin of the observed seismic tremor. As a general comment, we believe that the field of tremor source process modelling has greatly advanced in the fifteen years since the discovery of subduction zone episodic tremor and slip (i.e., Rogers and Dragert, 2003, Science). This discovery lead to a dramatic increase in the number of mechanical modelling studies of various tremor source processes. We have provided a streamlined review of this (and other) literature with emphasis on how the tremor observed at Askja may be generated.

Another brave assumption was that the frequency change was caused by the change of loading velocity. It was not easy for me to imagine the acceleration and deceleration of the velocity occur simultaneously at a single body (which is the assumption when you performed long-period inversion).
Reply: Prompted by this comment, as well as a similar comment from Referee #1, we have greatly rethought our explanation of the tremor frequency change. In particular, we no longer favour the explanation of local deceleration prior to the landslide. As we wrote to Referee #1, "We agree that deceleration before the landslide is a physically unrealistic explanation. We find that we can equally well fit the data if the second tremor patch gradually expands in size. This explanation has an additional benefit as well. Since the seismic moment (and therefore the far-field seismic particle displacement amplitudes) is proportional to the patch area, this model is capable of explaining the increased tremor amplitudes before the quiescent period. We describe these improvements in greater detail in Section 4."

Many sentences in the discussion (section 6) were an interpretation based on the assumption, but they are discussed without considering assumptions (e.g. page 13 line 23-24, 26-28, page 14 line 17-18, page 14 lines 31-32). I think interpretations and observations should be classified.
Reply: We changed the wording of this section and pointed out the assumptions and interpretations more clearly.

In section 6.2, authors are discussed the possibility of potential landslide early warning system using seismic signal. The idea is interesting, but the way of writing seems to be too optimistic. At this moment we are drawing the target around the arrow. For example, the first sentence in

page 15 says the precursory tremors should be dectable, but as authors may know, there are many landslides which do not generate precursory signal. For practical purpose, the success rate also should be investigated.
Reply: We have modified our discussion of the potential to use tremor as an early warning system. In particular, we have changed the tone of our argument to be more modest. We simply wish to imply that the feasibility of such as system deserves further study.

[Minor comments]
Page 4, section 2.1 and 2.2 The geological setting and seismicities are difficult to configure without map. Readers may not be familiar with the area. Since it seems they are used in the later interpretation, please add figures to explain them.
Reply: The inset of Figure 1 already shows the location names mentioned in section 2.1 but we added more references to this figure in the text. We added the location names mentioned in section 2.2 to Figure 1.

Page 6 lines 1-7 Please add a map to show those events.
Reply: We added two maps to the supplement, one showing the earthquakes in the month before the landslide and one with the earthquakes occurring one month after the landslide (Figs. S1 and S2).

Page 6 line 18 It is not clear for me what is cigar-like shape. Could you rephrase it? (same in page 13 line 3)
Reply: To show the shape of the waveforms in detail, we added a zoom-in of the landslide signal and of one afterslide signal to Figure 4. We now use the term spindle-shaped.

Page 7 section 3.2 Please show the force-time history and waveform fitting (possibly in appendix)
Reply: We added the force-time history to Figure 5 and present the waveform fits in Figure S3 of the supplement.

Page 7 line 13 Remove a big space between CMG-3Ts and instruments
Reply: We removed the space.

Page 7 line 21 Inversion would give a force (mass times acceleration). How did you compute the mass?
Reply: We assumed a constant value for the landslide mass (*m*) and calculated the acceleration time-series by dividing the force results by the mass (a = F / m). Then, the displacement (*d*, the trajectory of the sliding mass) can be computed from the second integration of the acceleration. Thus, we can estimate the mass (*m*) from the resulted force time history that is also able to explain the run-out distance (~ 1000 m) and the run-out trajectories of the event inferred from remote sensing images and field observations.
Chao et al. (2016, Scientific Reports) present the estimated mass of 10 large-scale landslides and more information related to mass computations can be found there.

Page 8 line 9 There are multiple lines and these three frequencies are not easy to identify. Could you zoom-in and add lines on the figure? How about the yellow curved line between 10-15 Hz?
Reply: We added a zoom-in to the figure showing the up- and down-gliding spectral lines in more detail (Figs. 6 and 8).

Page 8 lines 13-17 Please show the spectrograms of other stations (e.g. DREK, GODA, HOTT, JONS, KLUR, MOFO, STAM, VADA) in the appendix.
Reply: We now present the stacked and single-station spectrograms in the supplement (Figs. S4 and S5).

Page 8 lines 18-19 I am not sure why this implies the surface wave. Other phases may give a large H/V.
Reply: We acknowledge that a large H/V ratio may be produced by several phases and changed the wording of this sentence.

Page 8 lines 22-27 Please show this amplitude ratio on a map.
Reply: We added a map showing the amplitude ratios to the supplement (Fig. S6).

Page 8 lines 28-32 Please show the time window used for this localization.
Reply: We added a sentence to the manuscript saying that "We used a frequency range of 1.5-3 Hz as this frequency band shows the highest tremor energy and time windows of 1 min starting at 22:54:00 UTC."

The word of "migration" sounds confusing for me. In general, if you say migration for tremors, the source location will migrate as a function of time. If the location is fixed when you invert the location from the cross correlation time shift, I suggest to use gridsearch.
Reply: We are sorry for the confusion. The term "migration" is taken from the original paper presenting the location method saying that "the migration of these observed time delays, that is, the conversion from time to distance, can be used to retrieve the origin of an event" (Burtin et al. 2014, Earth Surface Dynamics). To avoid further confusion, we changed the term in the manuscript.

Page 10 lines 3-10 It would be helpful if you add a geological section of the landslide to understand this description.
Reply: We changed this section substantially and clarified the language. We added a speculative cross section to Figure 7 showing the hypothetical failure planes.

Page 10 lines 8-9 Why does the higher energy transmit if the stick-slip motion happens at the sliding surface? That is not intuitive.
Reply: If the landslide mass is more damaged than its foundation, we expect it to act as a low velocity waveguide and hence transmit less energy into the solid Earth. We've modified the language on this point.

Page 10 line 12-17 This is quite strong assumption, and I was not convinced that it was the only one possibility to explain this phenomenon. You wrote, "we deduce that the individual, repeating stick-slip events occurred very close together in time from the start of instability." On the other hand, "individual stick-slip events before the Askja landslide may not have been detectable kilometres away and that the events must occur already very close in time and transmit enough energy that they can be seen from a longer distance, likely as a continuous tremor signal." If you treat these tremors as summation of individual events, why the attenuation can be different? You wrote the individual events were not detectable farther away but tremor signal could transmit energy. That sounds contradictory for me.
Reply: These sentences were written in a confusing way. We meant to say that we can distinguish the presence of the seismic tremor versus its absence. The tremor itself, as we

have shown in numerical simulations, may be explained as due to many repeating velocity pulses that are blurred together due to a combination of effects. Hence we cannot discern individual slip events within the seismic tremor. We've heavily modified the writing surrounding these sentences.

Page 13 line 11-13 Please show the location of afterevents on a map.
Reply: We added a map with the afterslides to Figure 9 showing the best fit locations of the small slope failures.

Page 28 Figure 4 Why you use vertical component in Fig. 4 and EW component in Fig. 6?
Reply: We changed Figure 4 and present the EW component there as well. This component shows the highest amplitudes for the landslide and the tremor.

Page 30 Figure 5 Please show the date of the photo (a) taken.
Reply: We added to the figure caption that the background image from Google Earth was taken on 7 August 2012.

Page 31 Fig. 6 Why are there strong signals with frequency <1Hz after the bandpass filter between 1-45Hz?
Reply: We apologise that the figure caption was misleading. The spectrogram is not filtered whereas the waveform is. We clarified this in the figure caption.

Page 32 Figure 7 Please add a scalebar for the likelihood. Please add the definition of likelihood in the main text.
Reply: We added a scale for the likelihood in the figure and now mention in the text that "we used the location procedure of Burtin et al. (2013) to locate the tremor signal on a DEM grid. This statistical approach assigns a probability to each grid point that it is the source of the signal based on cross-correlation of the waveforms at different stations. The resulting probability density function is normalised to its maximum value giving this grid point a likelihood of 1 to be the source location of the signal (Burtin et al., 2014)."