Authors' reply to comments of the editor

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 editor for her constructive comments and suggestions. Please find in the following the editor's comments followed by our reply.

Comments by K. Allstadt (Editor)

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The authors presented a well-written and scientifically interesting case study with compelling findings. The two reviews were generally positive. While both reviewers did ask for more explanation and justification, the authors seem to addressed most of those concerns rigorously in their response to the interactive comments. I strongly encourage the authors to submit their revised manuscript. However, before doing so, I encourage the authors to ensure that they sufficiently address two of reviewer 2's comments that I did not feel were satisfactorily addressed in the response, as explained below:

Regarding page 7, line 21, reviewer 2 asks the authors to explain how they obtained the mass from the force history, and pointed out that you must know the mass to get the trajectory or vice versa. In their response, the authors give an explanation of their methods, which involves an iterative process to match the trajectory to the observed runout distance. As the authors acknowledge in their original manuscript, the trajectory corresponds with that of the center of mass, not the overall runout distance. However, in Figure 5 of the initial submission, the authors place the first dot at the very top of the landslide, which is certainly not the initial location of the center of mass. Correcting for this would substantially shorten the trajectory and change all of the derived values. I also recommend the authors give some more explanation about how well they actually know the runout distance of the center of mass and how the uncertainty of that is reflected in the estimates they derived from the force history inversion. The location of the center of mass before and after the landslide can be challenging to estimate with just satellite imagery and field observations and a rigorous assessment requires some assumptions about the location of the failure plane. Is the uncertainty in the before and after location of the center of mass the source of the uncertainty ranges given on the estimated mass and other seismically-derived values in the original submission? No explanation is given but it is needed.

Reply: The editor's point is well taken. In this study, we performed an inversion of the long-period seismic signals between 0.02-0.08 Hz based on a model where the landslide mass is treated as a single block. The spatial scale of the landslide event is small enough compared to the wavelength of above filtered seismic waves to satisfy the block model approximation (i.e. seismic point source). Assuming a constant value of landslide mass over time, we can estimate the acceleration time-series by dividing the inverted forces by the landslide mass. Then, we put the initial position of the block mass at the top of the landslide area and find the mass by ensuring that the block-mass trajectory inferred from acceleration time-series matches the run-out path from satellite images and field observations. Here, the landslide mass for the grid-search scheme ranged from 1×10^{10} kg to 2×10^{11} kg.

Uncertainties in the inversion result mainly rely on the data quality of observed seismograms. Chao et al. (2016, 2017) demonstrated that only few stations with good signal-to-noise ratios (SNR) are sufficient to produce a reliable inversion result (i.e. waveform fitness value is larger than 0.75). In fact, the SNR of the waveforms depends on the frequency range of the band-pass filter. In order to test the sensitivity of the waveform inversion to the chosen frequency band, we used three frequency ranges of 0.02-0.05 Hz, 0.02-0.08 Hz and 0.04-0.08 Hz. The most likely run-out path of the landslide event is obtained from averaging over all the inversion results taking the standard deviation into account (see Fig. 5e). This is reflected in the range of values giving for the horizontal displacement of 1260±250 m, the vertical displacement of 430±300m and the landslide mass of $7\text{-}16\times10^{10}$ kg. We have revised the manuscript to clarify the above statements.

Additionally, regarding page 10, line 12-17, Reviewer 2 states "You wrote the individual events were not detectable farther away but tremor signal could transmit energy. That sounds contradictory for me." – I agree with reviewer 2 that *it is contradictory to say that discrete repeating events are too weak to be recorded as individual events*, but are strong enough to be recorded once they are closely spaced enough together to appear as tremor. Most if not all of the other studies that invoke repeating quakes as a source of gliding spectral lines have observed discrete events that become more frequent and grade into tremor. For stick-slip sliding, as the recurrence interval decreases, one might expect the subsequent earthquakes to become smaller rather than larger (slip- predictable behavior). The authors responded to this comment by saying that they cannot discern the individual slip events within the tremor, but that does not address the actual topic of concern of reviewer 2, which was that no discrete events were observed BEFORE the tremor. *Can the authors provide any more potential explanations for why no discrete events were ever observed if this tremor is, as they propose, generated by closely spaced repeating events that become more and more frequent?*

Reply: Our original presentation of this idea was in need of improvement. We have now included supplemental Figure S7, which shows a sample seismogram, and its Fourier transform. This seismogram shows that individual events are in fact discernable, but that they are rather jumbled in appearance and difficult to interpret. The Fourier velocity spectrum, in contrast, shows clear spectral peaks whose interpretation we give in the main text.

There is a one-to-one correlation between the spectral peaks due to a repeating signal, and the repeating signal itself. It is not possible to have one without the other. However, because we infer the presence of multiple superposed signals, the interpretation of the time domain signal can be difficult. It is possible, for example, that a positive velocity pulse from one tremor patch and a negative velocity pulse from another, distinct tremor patch arrives at the same time and cause destructive interference. Other complexities are also possible. We have revised the main text to emphasize that while individual events

are possible, we find that the time averaging afforded by a spectrogram facilitates interpretation of the observed seismic signal.
Several other studies of seismic tremor due to repeating stick-slip have also noted this time domain/frequency domain phenomenology. Dmitrieva et al. (2013) specifically noted that no individual events were present in their study on Mt. Redoubt Volcano. In studies of the Whillans Ice Plain, Lipovsky and Dunham (2016) and Winberry et al. (2013) observed some locations that had clear individual events and other locations that had tremor signals where individual events were blurred together. We have now also noted this in the text.

I also have a few minor comments that were not already addressed by reviewers that should be addressed in the revision:
-Avoid overusing intensifiers like “very”, “excellent”, “exceptional” – they add little meaning.
Reply: We went through the text and deleted those adjectives where appropriate.

-pg3 L14-17 Where did the steam cloud come from if the event was a landslide? Mention hydrothermal depressurization up front, right now it’s buried toward the end of the manuscript.
Reply: We rephrased this sentence and now refer to the depressurization of the hydrothermal system. We now also mention that the cloud could have also contained a considerable amount of dust from the landslide.

-pg5 L20-24 Landslide triggering often depends on more than just a few days weather beforehand. It might be useful to put the weather data shown in Fig 2 in context of typical weather (i.e. was the entire period warmer than usual?).
Reply: We computed a mean July temperature for the year 2014 of 8.1°C for weather station Kárahnjúkar, and now mention in the text that “the area around the Askja central volcano experienced a period of warm weather in July 2014 with a mean monthly temperature of 8°C, 2 degrees higher than the long-term average of the mean July temperature.“

-pg5 L29 Is this seismic data openly available? If so, where? The authors can refer to a data and resources section or acknowledgements with details, but it’s useful to state somewhere in the text.
Reply: We added a data availability section saying that “the seismic dataset is available upon request from Prof. Robert White, Bullard Laboratories, University of Cambridge, Cambridge CB3 0EZ, United Kingdom.”

-pg6 L1 – CMM is not commonly known, it could use a brief explanation.
Reply: We added a short description of this earthquake location algorithm saying that “this method combines seismic imaging and travel time inversion to determine the locations and times of earthquakes from seismic data recorded continuously on a sparse local seismometer array. Data inversion is done as a 3D subsurface grid search over the data and network of trial locations for likely locations and origin times of seismic events

(Drew et al., 2013)."

-pg6 L12 – Ensure it is clear that the description here refers to the seismic signal from the catastrophic failure part of the landslide sequence, not the precursory tremor.
Reply: We changed this sentence and now refer to the "high-amplitude short-period signals generated by the catastrophic failure part of the Askja landslide sequence".

-pg 7 L7-10 – It would be helpful the frequency ranges were also given in period in parentheses beside each range.
Reply: We added the period rages to the frequencies.

-pg 7 L24 – The authors range is not actually within the other range, as stated. Perhaps instead the authors should say the ranges overlap?
Reply: We changed this statement and now speak of overlapping ranges.

-pg8 L15-16 How was mean frequency computed?
Reply: We clarified this procedure in the manuscript by saying "first, we stacked the spectrograms of the eight closest stations, which where computed with the same specifications like time window length and fraction of window overlap, etc., by adding their energy values per frequency and time step. Then, we divided these sums by the number of stations to obtain the mean energies for the frequencies and time period of interest. The result shows that the gliding spectral lines of the tremor are clearly visible as sharp bands of higher energy values and did not become blurred in the stacked spectrogram (Fig. S5)."

-pg 8 L23 Citation or more background needed on this method.
Reply: We added more detail to this method and now state that "we computed the ratios of the mean envelope amplitudes of 1 minute of the tremor to 3 minutes of background seismic noise for all stations of the network. First, we removed the instrument response, the mean and the trend, and band-pass filtered the signals between 1-45 Hz. Then, we computed the envelopes for 1 minute of the tremor starting at 23:17:00 UTC, 21 July 2014, and for 3 minutes of background seismic noise starting at 00:10:00 UTC of the same day for the E components. Next, we calculated the mean amplitudes of the envelopes for these two time windows and determined their ratio."

-pg8 L30 – Using cross correlation of envelopes or time series waveforms?
Reply: We used signal envelopes for the cross-correlation. We clarified this in the manuscript.

-pg10 L25-26 Citation needed.
Reply: We changed this section completely in the new version of the manuscript.

-pg 11 L2-3 Can the authors provide any explanation for why the two different gliding bands seem to have different stopping points?
Reply: In the new version of the manuscript, we favour the explanation that the up- and down-gliding spectral lines of the tremor are caused by an accelerating (visible in the up-

gliding) and a growing (down-gliding) stick-slip patch. As these two patches seem to be controlled by different mechanisms and move independently, we envisage that the patches transition into a state of seismically non-detectable movement due to different reasons, which might happen at different times. In the manuscript, we explain the disappearance of the tremor as follows:
"These simulations additionally predict the disappearance of the tremor signal shortly before the landslide. We suggest that two different mechanisms are responsible for this behaviour in our case. First, the patch that experiences accelerated loading eventually crosses the stability threshold and begins to start sliding stably ($R < Rc$ in Eq. 1). This behaviour is consistent with the theoretical prediction of a transition from stick-slip to stable sliding at high loading rates (Rice et al., 2001; Gomberg et al., 2011). In the simulations, this can be traced by the up-gliding spectral lines whose energy contents decrease with time until they fade into the background at 13 minutes (Fig. 8c). Second, the patch with growing area experiences a commensurate increase in recurrence time (recurrence time and patch size are proportional, see Eq. 2); eventually the recurrence time becomes so large that a quiescent period ensues. This can be seen in the simulations of the down-gliding spectral lines that disappear at 12 minutes (Fig. 8d). "

-pg12 L26-28 This comparison does not seem useful as the two seem unrelated and likely have different mechanisms. Are the authors implying there is some link between the stick-slip events related to landslide motion and hydrothermal activity?
Reply: We concur with the editor that the stick-slip events and the earthquakes of the hydrothermal system likely result from different physical mechanisms. Therefore, we changed this paragraph.

-pg13 L23-24 Can the authors actually be sure the landslide didn't start moving before the observable tremor? The authors state earlier that they think there were unobservable discrete events leading up to this, meaning it must have started moving earlier than that.
Reply: We agree that the landslide might have moved before the observable tremor and clarified this in the manuscript.

-pg14 L1 "accelerated" and "stable-sliding" seems like an oxymoron
Reply: We changed the wording of this sentence. It now reads "Through acceleration and growth of the sliding planes, the stick-slip sliding transitioned into an aseismic, stable sliding period".

-pg14 L3 It is not clear why the authors cite other papers here for findings made regarding the present study.
Reply: The concept of distinguishing the phases of landslide motion based on seismic signals was introduced by Hibert et al. (2014) and Chao et al. (2016). We changed the sentence in the manuscript to "based on combined inspection of the high- and low-frequency signals generated by the Askja landslide, we distinguish three phases of landslide motion, initiation, propagation and termination as proposed by Hibert et al. (2014) and Chao et al. (2016). "