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Earth Surface Dynamics Discussions
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Interactive comment on "Dynamics of the Askja caldera July 2014 landslide, Iceland, from seismic signal analysis: precursor, motion and aftermath" *by* Anne Schöpa et al.

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

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Discussion paper

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.

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?

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. -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. -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?). -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. -pg6 L1 – CMM is not commonly known, it could use a brief explanation. -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. -pg 7 L7-10 – It would be helpful the frequency ranges were also given in period in parentheses beside each range. -pg 7 L24 – The authors range is not actually within the other range, as stated. Perhaps instead the authors should say the ranges overlap? -pg8 L15-16 How was mean frequency computed? -pg 8 L23 Citation or more background needed on this method. -pg8 L30 – Using cross correlation of envelopes or time series waveforms? -pg10 L25-26 Citation needed -pg 11 L2-3 Can the authors provide any explanation for why the two different gliding bands seem to have different stopping points? -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? -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 unobserv-

able discrete events leading up to this, meaning it must have started moving earlier than that. -pg14 L1 “accelerated” and “stable-sliding” seems like an oxymoron -pg14 L3 It is not clear why the authors cite other papers here for findings made regarding the present study.

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