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Earth Surface Dynamics Discussions

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.

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

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.

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.

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. 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.) 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. . .”) 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. 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. 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? 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”? 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. 7. Page 13, line 24: This line about the 38 seismometers within 30 km is repetitious. 8. Page 13, line 25: Unclear what the authors mean by “activated”? 9. Page 13, line 29: Is this just saying that there is an asperity on the failure plane?

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