Earth **Surface**
Dynamics

Discussions

EGU

Interactive comment on "Comment on: Dynamics of the Askja caldera July 2014 landslide, Iceland, from seismic signal analysis: precursor, motion and aftermath" *by* Tómas Jóhannesson et al.

Anonymous Referee #2

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Review of Esurf manuscript "Comment on: Dynamics of the Askja caldera July 2014 landslide, Iceland, from seismic signal analysis: precursor, motion and aftermath by Tómas Jóhannesson et al."

The comment-manuscript addresses several quantitative disagreements, for the same landslide, of the Schöpa et al. relatively to the Gylfadóttir et al. (2017) paper, respectively. It questions the strength of landslide parameters that are derived from seismic signal analysis (e.g. Schöpa et al. 2018).

First, from geometry patterns, the submitted "comment" point on (i) a false citation when reporting on the landslide volume as referenced by another study (i.e. Gyl-

fadóttir et al. (2017). "Referring to Gylfadóttir et al. (2017) as the source, Schöpa et al. state that the volume of the slide was 12–50 million m according to geodetic surveys. . .whereas the referenced paper quantitatively points on a 20 million m value". Moreover, (ii) the volume and centre-of-mass displacement estimated by Schöpa et al. correspond to average debris thickness, which is more than an order of magnitude less thickness of the field measurements for the debris tongue Gylfadóttir et al. (2017).

Second from kinetic approaches, the submitted comment-manuscript points on the velocity disagreement between the 7 m/s sliding velocity from seismic signal analysis (Schöpa et al. 2018) and the 30 m/s impact velocity (as tuned to fit the observed tsunami run-ups around the lake by Gylfadóttir et al. 2017). All these points are well grounded when the comment authors should also explicitly state the Gylfadóttir et al. velocity estimate is an indirect measurement.

Before the comment to be accepted, I suggest some sentences should be added to the text for the comment-manuscript to go beyond the Gylfadóttir et al. 2017 versus Schöpa et al. binary analysis. e.g., What are the lessons to be learned for the landslide community from the discrepancies between the two studies? It may point on a necessity to switch from the deterministic outputs of both the Gylfadóttir et al. 2017 and Schöpa et al. 2018 studies to a more probabilistic approach where ensemble solutions are provided explicitly for geometry and kinematic of landslides.

specific comments: "A maximum velocity of only 7 m/s (corresponding to the potential energy of an object raised 50 vertically by 2–3 m) seems unreasonably low since this would imply a delicate local balance between frictional forces and the potential energy released at each instance during the fall, which does not seem likely" The above comment is qualitative in several parts. A more quantitative version is expected.