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Earth Surface Dynamics Discussions

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.

Tim Greenfield (Referee)

tg286@cam.ac.uk

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The authors comment on a recent (2018) paper by Schöpa et al. on the dynamics of the July 2014 landslide in Askja caldera, Iceland. Two criticisms of the paper are presented. The first is that the landslide volume as calculated by Schöpa et al. (2018) is too large compared to detailed field surveys and tsunami modelling presented in an earlier paper by Gylfadóttir et al. (2017). The second is that the maximum average velocity calculated by Schöpa et al. (2018) is an order of magnitude smaller than the results suggested by Gylfadóttir et al. (2017).

Schöpa et al. (2018) note in their text the many assumptions made in their modelling

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procedure. They identify the length of the run-out as a key parameter in estimating the mass (and therefore volume) of the slide. Given the slide entered the lake and the difficulty in determining the basal slip plane (identified by the authors of this comment), there is significant uncertainty in this parameter. I note though, that Schöpa et al. (2018) do misquote the field and geodetic based estimates of volume from Gylfadóttir et al. (2017) in their text and these could have been used to estimate the run-out from the Schöpa et al. (2018) force time history. It is also interesting to note that another application of the Ekstrom & Stark (2013) technique in Greenland (Chao et al., 2019) overestimates the volume compared to accurate field and satellite-based surveys. This could suggest a weakness of the Ekstrom & Stark (2013) technique when a slide enters the water.

The velocity of the slide is quoted as an average maximum sliding velocity (7 ± 0.7 m/s) and does, at first glance, appear to be slower than expected. This value follows directly from the estimation of the slide's mass and so low values may be expected given Schöpa et al. (2018) overestimate this parameter. Schöpa et al. (2018) note the discrepancy between their value and the 30 m/s estimated from tsunami modelling (Gylfadóttir et al., 2017). They highlight three simplifying assumptions in their modelling which may account for the difference between the two values. I believe however, that part of the misunderstanding comes from Schöpa et al. (2018) quoting an average velocity rather than a peak velocity. I calculate (from figure 5b in Schöpa et al., 2018) a peak velocity of 18 m/s (Figure 1). This is of the right order of magnitude and should be highlighted by the authors of this comment. It would be interesting to see whether the velocity-time history could produce the observed tsunami run-ups.

The text is fairly well written, although the second paragraph took me a couple of read throughs to really understand what the authors are trying to get across. I would suggest simplifying and clarifying the first two sentences, which are quite long.

I recommend publication of the comment after minor revisions to the text are completed.

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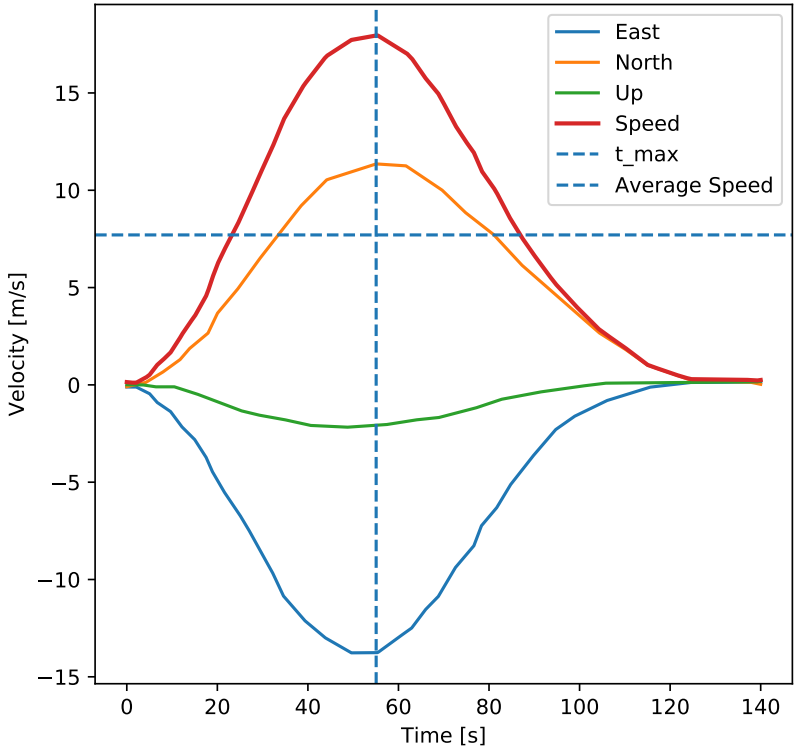

Fig. 1. Velocity-time history from Schöpa et al. (2018). The digitised east, north and up components are shown by the thin lines and the overall speed by the thick red line.

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