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Interactive comment on "Effects of finite source rupture on landslide triggering: The 2016 M_W 7.1 Kumamoto earthquake" *by* Sebastian von Specht et al.

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Dear Mr. Setiawan,

thank you for your comments and constructive suggestions, which we considered in detail to improve the presentation of our study. Please find below our point-by-point response to the *original comments*:

1. *page 8, line 29: The safety factor of $F_S < 1.5$ for unstable hillslopes, is this statement applied for seismic induced, or rainfall-induced or both in general?*

   In this particular case we refer to the work by Chen et al. [2017]. They mentioned

   both rainfall and earthquakes for their safety factor definition.

   Changes in text (p. 9, l. 3):

   Chen et al. (2017) characterized unstable hillslopes, related to both rainfall and earthquakes, by a safety factor of $FS < 1.5$. Rarely is the limit equilibrium at $FS = 1$ considered as a reliable metric in engineering geology.

2. *page 9 line 7–8: please check the equation of Arias intensity, is it $\frac{\pi}{2g}$ or $\frac{2}{\pi g}$, see reference i.e Jibson (2007), USGS (1993) or Stafford et al (2009).*

   This was a typo in the fraction indeed.

   Now changed in text (p. 9, l. 14):

   $\frac{\pi}{2g}$

3. *Page 10 line 6: please add remark M_0 for the seismic moment directly. Some equations remarks also should be checked and added (if not yet mentioned).*

   Done and changed in text (p. 10, l. 11):

   where $\Delta\sigma$ is the stress drop, μ the shear modulus, and M_0 the seismic moment.

4. *page 13, line 1: "...since energy is proportional to the seismic moment M_0 (Eq.9)..." this should be (Eq.10)?? (Hanks and Kanamori, 1979).*

   Yes, changed to Eq. 10. on p. 10, l. 10

5. *Page 13 line 12: "... and θ_E and θ_E are the azimuths of the maximum." This should be θ_E and θ_I*

   Yes, changed to θ_I on p. 13, l. 29

6. *Figure 14 indicates that mostly landslides concentrated in the aspect of about 120 degrees, south-east, with distance for the rupture approximately within 1–2 km, which from location densely surrounding Aso caldera. Besides rupture*

effects, does distinctive lithology condition in Aso caldera itself also contribute to this finding?

The lithology (or at least the nominal descriptions of dominant rock types) does not show any distinctive directional properties. While it is reasonable to assume that landslides occurred along the weak zones (such as the Halloysite layers we refer to), no preferred orientation has been reported for these shallow layers. (Paudel et al., 2007, 2008; Sato et al., 2017).

7. *Does the rupture propagation energy also (at the end) include the compressional waves (page 9 line 24) in the Aso caldera, south-east side, where the landslides densely concentrated as described in your finding? What is your opinion as an additional explanation in the Discussion? Since your manuscript only applies the shear waves only for estimating the energy in the model.*

   The exact calculation of radiated seismic energy is quite complex, which is the main reason why we only consider the shear wave velocity at a site. Our assumption is that we treat the entire waveform as if it arrived at a constant velocity at a site when estimating the radiated seismic energy. This assumption results in an underestimation of the energy of 2.6% at longer distances and 7% at the fault. Compared to other components of the energy estimation procedure, e.g. the assumptions for the geometrical spreading, the usage of the shear wave velocity only introduces a minor error at most sites.

   Changed in text (p. 10, l. 11-16):

   Since most seismic energy is released as shear waves, we apply the shear wave velocity at the recording site (v_S) to the entire waveform, i.e. we assume that all waves arrive with velocity v_S at a site. This assumption has the advantage that it does not require a separation of the record into P- and S-waveforms, simplifying the computation. In the Appendix we show from a theoretical perspective that using a uniform v_S has only a small impact on the overall energy estimate.

   We added the detailed description of the appendix.

8. *Related to questions 6 and 7, after your findings, do the normal faulting component should be accounted into your model? For example, if we look both at strike-slip and normal components. Does it significantly affect the spatial pattern, asymmetrical distribution or landslides depth?*

   We showed that the fault-normal/fault-parallel ratios are consistent with Somerville et al. (1997), who also formulated their fault-normal/fault-parallel ratio as a term that can be easily plugged into a GMPE. Somerville et al. (1997) also provide model coefficients for fault-normal/fault-parallel ratios for dip-slip events. They observed a similar behaviour for strike-slip and dip-slip events with the dip-slip events exhibiting lesser amplitude variations of fault-normal/fault-parallel ratios and directivity. The formulation of the FN/FP term as an additional (optional) term for GMPEs is common practice (Somerville et al., 1997; Spudich et al., 2004, 2013). Any impact of single components from strike-slip and normal faulting cannot be quantified here as we investigated only a single earthquake. Concerning the landslide depths, please see the next comment.

9. *Landslides aspects and asymmetric spatial distribution are well described in your manuscript. Do the depth variability of those recorded co-seismic landslides also can be related with the rupture propagation processes and can be explained through your physical-based ground motion model?*

   Unfortunately, we do not have depth measurements of the landslides. We only know that the coseismic landslides were shallow (Song et al., 2017; Sato et al., 2017; Hung et al., 2017), thus we cannot make a detailed statement about the relation between landslide depth and rupture processes. We could use an empirical scaling between landslide volume and area, but that would introduce additional (and unnecessary) scatter to our model.

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