This manuscript reports an interesting attempt to define a model to predict area affected by earthquake-induced landslides, outlining distance from earthquake source, within which major effects are expected, on the basis of seismological parameters. While the basic ideas developed to simplify the calculation of such distance appear smart, some aspects of model implementation seem to me unclear or questionable and should be better justified or reconsidered.

A first problem concerns the equation (3) used to define the relation between the seismic moment *Mo* and the fault rupture length *L*, i.e.:

$$L=\frac{Mo^{\frac{2}{5}}}{\mu C_1^{\frac{3}{2}} C_2} \qquad \text{[R1]},$$

where μ is the rigidity modulus of the faulted rocks and C_1, C_2 are empirically determined coefficients.

The authors declare to have derived it from the paper by Leonard (2010). However the cited paper does not report a relation *L*(*Mo*) in this form, and, if equation (3) was derived from the results presented by Leonard, it is incorrectly written.

Indeed Leonard, starting from the well known general equation

$$Mo=\mu L W \acute{D} \qquad \text{[R2]},$$

where W is the fault rupture width and $\acute{D}$ is the mean dislocation along the rupture fault, proposes two equations relating W and $\acute{D}$ to L, in the forms

$$W=C_1 L^{\beta} \qquad \text{[R3]},$$

$$\acute{D}=C_2 (LW)^{\frac{1}{2}}=C_2\left[C_1 L^{(1+\beta)}\right]^{\frac{1}{2}} \qquad \text{[R4]},$$

from which one can obtain

$$Mo=\mu C_1^{\frac{3}{2}} C_2 L^{\frac{3}{2}(1+\beta)} \qquad \text{[R5]}.$$

Leonard found that, for almost all kinds of fault, β can be set to 2/3, which implies

$$Mo=\mu C_1^{\frac{3}{2}} C_2 L^{\frac{5}{2}} \qquad \text{[R6]},$$

with the exception of strike-slip faults exceeding a length of 45 km, for which β should be set to 0 and consequently.

$$Mo=\mu C_1^{\frac{3}{2}} C_2 L^{\frac{3}{2}} \qquad \text{[R7]}.$$

From these equations, one can derive that, for most of faults,

$$L=\left(\frac{Mo}{\mu C_1^{\frac{3}{2}} C_2}\right)^{\frac{2}{5}} \qquad \text{[R8]},$$

(which differs from [R11]) and, for strike-slip faults longer than 45 km,

$$L=\left(\frac{Mo}{\mu C_1^{\frac{3}{2}} C_2}\right)^{\frac{2}{3}} \qquad \text{[R9]}.$$

Additionally, Leonard derived different values of C_1 and C_2, for different type of faults, i.e., C_1 = 17.5 and C_2, = $3.8 \cdot 10^{-5}$ for dip-slip inter-plate faults, C_1 = 15.0 and C_2, = $3.7 \cdot 10^{-5}$ for strike-slip inter-plate faults and C_1 = 13.5 and C_2, = $7.3 \cdot 10^{-5}$ for intra-plate earthquake (stable continental regions). The value assumed for C_1 in the present manuscript (16.5) does not correspond to none of the values proposed by Leonard and also the value assumed for μ (3.3 GPa) is incorrect (it should be 33

GPa). If the errors in equation form and in parameters were due to misprints, they should be corrected, but if these formulae were actually used in calculations, the results would be totally inconsistent with the seismological model and should be recalculated.

>> The derivation reminded here is exact and we have followed the same. The script we used to compute rupture length indeed use EQ 8 and EQ 9 (where C2 = 17km in EQ 2). Indeed, (Mo^2/5) / µ C2 C1^3/2 would have given ridiculously small length on the order of some meters…
→ We have added the missing parentheses before the exponent in the equation 3.

For the parameters, we used indeed µ=33 Gpa, and the dot is a typo → corrected.

For C1, 16.5 is an intermediate value between 15 and 17.5 (and well within the 1sigma of both estimation that spans approximately between 10 and 25) that allow to collapse strike-slip and reverse fault before they reach the seismogenic depth. We note that using 15 or 17.5 or strike slipe dip slip would change by +6% / - 3% (respectively) the rupture length prediction having a quantitatively negligible effect. Similarly we use a single C2 value 3.7.10^-5 and not 3.7 and 3.8.

We somehow overlooked that and did not state it in the text. We are sorry about that and now have expanded the sentence after Eq 3:

"with μ the shear modulus, assumed to be 33 GPa, and C_1 and C_2 empirical constants. Although Leonard have fitted indepedently strike slip and dip-slip fault he obtained very close values for each cases, C_1 = 15 [11-20] and C1=17.5 [12-25] , and C2 = 3.8 or 3.7 10-5, respectively. For the sake of simplicity and to have a single prediction for strike-slip and reverse small and intermediate faults we choose a single, intermediate value for C1=16.5 and C2=3.7.10^-5, and note that predicted length differ only by a few per cent from what would be obtain with the best estimate proposed by Leonard and reported in the previous sentence."

Another puzzling question is relative to the equations (4), i.e.

$$b=b_{sat}\exp\left[e_5\left(M_W-M_h\right)+e_6\left(M_W-M_h\right)^2\right] \qquad (\text{for } M_W \leq M_h) \qquad [R10]$$

$$b=b_{sat}\exp\left[e_7\left(M_W-M_h\right)\right] \qquad (\text{for } M_W > M_h),$$

which were used to define the peak ground acceleration (PGA) expected at a distance of 1 km for an event of magnitude M_W. This acceleration value, in turn, is used to derive the distance R_{HMAX} within which the ground acceleration is not less than a_c (assuming that ground motion attenuation depends only on geometrical spreading), according to the equation

$$R_{HMAX}=\sqrt{\left(\frac{b}{a_c}\right)^2-R_o^2} \qquad [R11].$$

The authors declares to have based their calculations on the ground motion prediction equation (GMPE) proposed by Boore & Atkinson (2008), but they adopt an arbitrary value of 4000 m for b_{sat}, which properly should be defined as the acceleration at a distance of 1 km for an event of magnitude M_W equal to the magnitude "hinge value" M_h = 6.75.

Preliminarily, I observe that it is quite puzzling to propose, for an acceleration, a value measured in meters. Probably the misunderstanding about the meaning of b_{sat} derives by the fact that b is used to calculate the distance where acceleration is reduced to a_c, exploiting the inverse proportionality between wave amplitude and distance. Actually, following the GMPE model by Boore & Atkinson, b_{sat} should be defined as the acceleration expected for $M_W = M_h$ at a reference distance R_{ref}, which Boore & Atkinson set to 1 km. Indeed, the complete expression of Boore & Atkinson's GMPE would include a factor depending on distance R which becomes equak to 1 when $R = R_{ref}$. Thus, to avoid a dimensional inconsistence, [R11] should be written as

$$R_{HMAX} = \sqrt{\left(\frac{b}{a_c} R_{ref}\right)^2 - R_o{}^2} \qquad [R12].$$

Numerically [R12] gives the same result as [R11] only if distances are expressed in km, but in any case the equation [R12] is dimensionally correct, assuming that both b and a_c represent accelerations. It is however unclear while, adopting the Boore & Atkinson's GMPE, the authors did not simply derives b_{sat} from it. Indeed, this GMPE provides the element to calculate b_{sat} for different type of faults, in terms of expressions like exp(e_1) for unknown type, exp(e_2) for strike-slip, exp(e_3) for normal faults and exp(e_4) for reverse faults, where the coefficients e_1, e_2, e_3 and e_4 are reported in Table 7 of the cited paper.

>> What is above is correct but not contradictory with our approach, although it may have been awkwardly presented.

1/ On the unit/definition of b:

b and a_c should be non-dimensional acceleration, normalized by the gravitational acceleration, g. Thus we should get a normalized ground acceleration at the surface when dividing b by a depth, for example, b/R0 = 0.4 , with b=4000 m and R0=10km. That is 40% of g.

Anyway for clarity, we rewrote Line 27 Page 3 with the equation suggested and with: " with b the non-dimensional near-source acceleration at a reference distance from the seismic source, Rref = 1km, and R0 ... "

2/ On the magnitude scaling

The main misunderstanding is that we do not aim at reproducing Boore and Atkinson 2008 prediction, which depends on a number of term that we do not consider and/or cannot constrain, such as Vs30 for site effects of non linear attenuation terms.

This should be better stated on the text, but we simply aim at ground shaking model where reasonable scaling for magnitude, fault type and attenuation are used.
For fault type: based on Boore and Atkinson shaking is ~ 30% lower for Normal fault than for strike-slip/dip-slip; As stated in the text.
For Attenuation: for frequency around 1Hz, geometrical spreading will dominate on the ~30 first km, that are the typical distance from the rupture where landslide occur. As stated in the text.
Then for Magnitude we aim at reproducing a increase of shaking and then saturation of the shaking consistent with seismological observations for earthquakes.

Line 14 Page 4 we added: "We refrain from using b_sat values as derived by Boore and Atkinson (2008) because they should be included within a model accounting for site effects and non-linear attenuation, effects that are beyond the scope of this work. Therefore, as in Marc et al. 2016 we use ..."

Furthermore, the author, using equation (4) ([R11] in the present comments), report to have set coefficients e_5 = 0.6728, e_6 = −0.1826 and e_7 = 0.054, assuming that these provide ground acceleration at 1 Hz. Actually, these coefficient values appear derived from those reported by Boore & Atkinson for 5% damped pseudo-spectral accelerations at a period of 1 s (apart from a slight error in e_5 which actually is 0.6788: see Table 7 in Boore & Atkinson, 2008). These coefficients are relative to GMPE that does not predict ground motion, but the response of a one degree-of-freedom oscillator whose base is fixed to soil and forced to move by seismic ground motion. This shaking parameter is used to evaluate the response of engineering structures (which can be assimilate to an oscillator of given eigen-frequency and damping) in terms of maximum acceleration induced by seismic shaking to the oscillator. It seems to me hardly justifiable to assimilate slope material behaviour to an oscillator with eigen-frequency of 1 Hz and damping equal to 5% of the critical values (which is typical for quite elastic engineering structures). Thus, I wonder why it was not simply used the coefficients provided for PGA in the same Table 7 (which, actually, predict a saturation for $M_W \geq M_h$ as resulting from being e_7 =0)?

3/ On the scaling parametrization

First of, the wrong value of e5 is a typo, and we used the correct one for our calculations. Then we rewrote: "e5,e6 and e7 are empirical constants for 1Hz Pseudo Spectral Accelerations."

Then we agree that PSA are not likely a correct mechanical description for hillslopes failure but they have the advantage to be available at different frequency. Because different frequency-dependent processes modulates the ground shaking (source spectra, non-linear attenuation, cf Boore 2013), we do see different behavior before and after saturation (when M<Mh or M>Mh) for PSA at different frequency (Boore and Atkinson 2008). Thus we prefer to use a scaling consistent with the 1Hz frequency, while PGA scaling is closer of PSA ~5-10Hz, with much less decay between PGA at saturation and PGA for a Mw = 5. As we currently state in the text, typical frequency of resonance of hillslope are around 1 Hz (Meunier et al., 2008) while smaller structures prone to failure maybe most sensitive to higher frequencies, up to 5-10 Hz (Line 34 page 3, Marc et al 2016b)

Therefore we added Line 15 Page 4 : "We prefer to use PSA because it allows to focus on ground motion around a specific frequency, and therefore sensitive to specific frequency-dependent modulation of the ground shaking, even if the PSA scaling is obtained with mechanical assumptions (elastic oscillator with based fixed on the ground and 5% damping) not necessarily relevant to hillslope failure."

Other minor comments relative to specific points of the manuscript can be found highlighted in the enclosed pdf copy.

>> These minor comments will be addressed in the final reply.