Reviewer 1

We thank the reviewer for the very careful revision of our paper and the helpful suggestions. In the following, we respond to the author’s comments (blue) and explain changes and adaption we propose for the final article. All comments not specifically addressed in the following list will be adapted in the manuscript as suggested.

Title

As the paper essentially deals with the impact frequency of blocks and not the occurrence frequency of rockfalls, it is suggested to replace "occurrence frequency" by "impact frequency". Moreover, forest can’t influence rockfall occurrence because rockfalls initiate usually in the upper part of slopes, before the falling blocks can be influenced by forest.

In this study, we define the occurrence frequency as the product of the onset frequency of a block and its propagation probability to a certain position along the slope. The impact frequency is – according to our definition – the product of the occurrence frequency and the presence probability of the element at risk. We agree that the terminology is slightly confusing and, therefore, we try to make it clearer in the introduction. We decided to replace “occurrence frequency” by frequency in the title (see also suggestion of Reviewer 2) in order to avoid confusion.

Specific comments

1. The input data which are used for the simulation of blocks propagation are derived from Carrea et al. (2015). But Carrea et al. (2005) give the distribution of the volumes of rockfall events and not of the individual blocks. Similarly, the cited references (Dussauge-Peisser et al., 2002; Malamud et al., 2004) don’t deal with the distribution of block volumes. Studies on the distribution of block volumes can be found in the following references: Ruiz-Carulla, R., Corominas, J. & Mavrouli, O. 2015. A methodology to obtain the block size distribution of fragmental rockfall deposits. Landslides, 12: 815–825. Hantz D., Ventroux Q., Rossetti J-P., Berger F. 2016. A new approach of diffuse rockfall hazard. In: Landslides and Engineered Slopes - Aversa et al. (Eds). Associazione Geotecnica Italiana, Rome, Italy, ISBN 978-1-138-02988-0, 1063-1067. This confusion doesn’t call into question the results obtained because (a) the simulation has been made on a virtual slope which is not the La Cornalle slope, (b) the values used for the power-law parameters are plausible also for the distribution of block volumes. But the section 2.2 should be rewritten without mentioning the unsuitable references (Dussauge-Peisser et al., 2002; Malamud et al., 2004; Carrea et al., 2005).

Thank you very much for this valuable input. We changed the reference in chapter 2.2 as suggested and excluded the reference for the beta-exponent.

2. In the widely used terminology of landslides (Varnes, 1978; Cruden & Varnes, 1996), the word "rock" refers to the material which is implied in the movement and not to the fragments which propagate down the slope. The fragments implied in a rockfall can be called fragments, particles, projectiles (Bourrier, Dorren, Hungr, 2013), but the word "block" is more commonly used (for example, Ruiz-Carulla, Corominas, Mavrouli, 2016, Comparison of block size distribution in rockfalls). Then I suggest to replace "rock" by "block" in some places. Moreover, a rockfall event consists in two phases: The detachment of a volume of rock from a steep slope and its propagation down the slope (for example, Bourrier, Dorren, Hungr, 2013). When mentioning a frequency, it is important to precise if it is a detachment (or release) frequency or an impact frequency on an element at risk. The expression "occurrence frequency" used in the manuscript

is not explicit, so I suggest to replace it by "release frequency" or "impact frequency".

We agree on that and replaced rock by block as suggested. We further tried to clarify the definition of terms regarding the frequency (see also response 1).

Page 3, line 6-9 I don’t understand what are "reference situations". Could you explain?

We refer here to (hypothetical) comparative situations such as other forest scenarios or non-forested situations. We replaced this in the text.

Page 4, line 7 Could you please explain why it is necessary to randomly vary the slope angle of each cell?

We randomly varied the slope angle aiming at more realistic slope conditions. We agree, however, that this is slightly contradictory to the “controlled conditions”. Since we re-ran the simulations (see below), we decided to remove the random variation of the slope angle.

Page 4, line 9 It should be explained why a vertical fall height of 10 m has been chosen (it is not realistic).

Since a vertical cliff face is not represented in our virtual slope, we chose an initial fall height of 10 m which is representative for real rockfall slopes.

Page 4, line 10 The distances are different from the distances in the Figure 1. For example, the last line must be at a distance of 574-100=474 m from the release area (and not 530 m). This point must be clarified.

This is incorrectly written in the text: The distances are measured on the slope (and not horizontally). In Figure 1, we also report the distances along the slope (in the graph).

Page 5, line 31 The 49 scenarios should be explained: 4 forest types and 4 forest structures give 16 scenarios, but how can one obtain 49 scenarios? The number of slope scenarios doesn’t appear clearly in table 1.

Correct is a total of 48 scenarios: 4 forest types, 4 forest structures and 3 terrain scenarios (rough + soiltype 3, rough + soiltype 4, zero roughness + soiltype 3 → zero roughness was not combined with soiltype 4).

Page 7, line 2 Power-laws were fitted for the volume-frequency relation, but the powerlaw parameters (alpha and beta) are not given in the paper. It would be interesting to compare the beta-values obtained with the beta-value adopted for the initial distribution of block volume. Concerning the intensity-frequency relation, could you please indicate if the E95 values have been averaged over the 100 simulations to obtain the distribution shown in Figure 9? In other words, is the distribution obtained from 239 E95 values (478 m / 2 m) or from 23,900 values (dividing the number of each energy class by 100,000 years)? In my opinion, the most significant distribution in terms of hazard assessment would have been obtained by considering all the energies calculated in all cells rather than only the 95th percentiles (which doesn’t contain all the information about the extreme values).

The alpha and beta coefficients are given in Table 6. We replaced them by the coefficients of the cumulative intensity-frequency distribution allowing for comparison with the initial distribution of block volume (in the old version, it is the non-cumulative distribution). Concerning the intensity values: We averaged the E95 values over the 100 simulations, but we agree that it would be better to calculate the 95-percentile from all energy values yielded at an evaluation line. Since the second reviewer questions the statistical representativeness of the intensity calculation (due to small block number of large volumes), we decided to re-run the simulations with more blocks (whole release area) and calculated the 95-percentile of the energy values of all blocks per volume passing an evaluation line.

Page 7, line 15-20 The definitions of bA and cbA should be clarified. From the definition given in the line 17, bA is not an area, but a relative area which reflect the proportion of area which is occupied by trees. It should be called "relative basal area". As bA is dimensionless (m2/ha), it should be multiplied by an area to obtain a total tree area, which influences the impact frequency. I suggest to present the definition as follows: "The latter is defined as the product of the basal area (bA; m2/ha) by the area of the forested slope from the top of the release area to the respective EL, for a width of 100 m." And to define fsL after Equation (7) as the forested slope length. As cbA is an area, it must be expressed in m2, and not in m2ha-1 as written in Equation (7), page 11, line 6-7 and in Table 5. Moreover, in the third member of Equation (7) bA represents the basal area of individual trees and not the (relative) basal area as defined previously. I suggest to remove this third member which is incorrect and unnecessary.

It is true that the definition is slightly confusing. cbA is indeed the product of the relative basal area (corrected in the text), which is the sum of the basal areas of individual trees divided by the respective horizontal area (from the bottom of the release area to the respective area), normalized by a slope width of 100 m and multiplied with the forested slope length (measured along the slope).