

Author's response to the Interactive comment of Franck Bourrier, Referee #1, on "Full scale experiments to examine the role of deadwood on rockfall dynamics in forests"

Dear Franck,

We thank you for the in general positive conception of our submission and your suggestions for improvements. We amended the manuscript with respect to your advice and enriched the abstract and conclusion with further information about the rock dimensions and their effects on the non-broken deadwood logs and living trees.

Please find below the response to your remaining criticisms. A manuscript including the open points would be ready but can only be submitted after the editorial decision has been made.

1) p.2 l. 44, 45: I don't understand the sentence.

We rephrased the sentence to clarify the introduction of deadwood logs into the three-dimensional rockfall model.

2) p.3 l. 63: the reference is strange

We amended the reference.

3) p.3 l.71-73: how were the SDE, SDE_{la} and SDE_{sa} calculated?

The SDE, SDE_{la} and SDE_{sa} were calculated using the standard python matplotlib library, including Carsten Schelps' function "plot_confidence_ellipse.py". The two-dimensional calculation is based on a particular case to obtain the eigenvalues of the 2D dataset. The original gist.hub repository is now linked as a source in the manuscript.

4) p.3 l.71-73: given that the distribution of the deposit is not gaussian, can you justify the use of the SDE indicators instead of "limits of a given percentile of the distribution of the deposited blocks in 2D (X,Y)" ?

Thank you for raising this interesting question. The null hypothesis of the normal-distribution test (scipy.stats.normaltest) for the x- and y- deposition coordinates of the barely disturbed ORG and CLR states (after the rotation of the principal components) cannot be rejected (alpha =0.05). Therefore, from a statistical point of view, we cannot say the deposition pattern does not correspond to the normal distribution, which permits the use of the SDE. Solely the longitudinal component of the DW-set up follows a non-normal distribution, mainly due to the outlier, stopped at the release and the one which surpassed the DW area.

However, choosing a given percentile of the distribution of the deposited blocks in 2D could be a viable option. As stated in the manuscript, experimental boundary conditions led to fewer deposition points in the deadwood state. Inclusion or exclusion of single deposition points heavily alters the percentile number, and single outliers would be unduly weighted. Due to this concern and – as stated above – only one axis could benefit from a refined statistical analysis, we opted for a normal distribution.

We enhanced the manuscript with explanatory sentences about the premises and results of the statistical analysis.

5) p.3 l.81-82: *I don't understand the sentence.*

We rephrased the sentence with descriptive details about the used data analysis methodology.

6) Table 1: *It can be interesting to discuss more the discrepancies between the experiments and simulations in terms of velocities for ORG and CLR and rotational velocities*

We feel that the presentation and discussion of the translational velocities regarding the stopping criterion are already present in the submitted manuscript. However, we enriched and clarified the discussion about the translational velocities and enlarged the discussion of the rotation velocities. It emerges that, in particular, the *maximum* rotation velocities show a greater variability between the individual runs, as the *mean* rotation velocities. We conclude that local soil and tree conditions are responsible for this. Since such local disparities are not represented to this level of detail in the simulations (partly to prevent overfitting), the simulated maximum rotation velocities are also lower.

7) p.6 l.123: *“ the surpass the DW” : missing word?*

We adjusted the attached manuscript: ORG and CLR surpass the DW state.

8) p.7 l.125 -126 : *Could you present more details about these results (quantitative comparisons between simulations for different values of the parameters, for example) ?*

In order to be able to compare the discrepancy between the input soil parameter pairs, we enhanced the results section by inserting the values OF_{3000} of the 10th placed parameter pair. Additionally, we discussed the meaning of the observed variabilities in the discussion.

9) p.8 l. 146 – 149 : *in my opinion, the differences with the results from Bourrier et al . (2012) are mainly due to the sizes of the blocks used in the simulations. In Bourrier et al., the blocks were large enough to break the trees which completely changes the processes as well as the efficacy of the protective measure.*

Unfortunately, nothing about tree breakage was stated in Bourrier (2012), therefore we concluded that the lower stopping capacity of the deadwood was mainly due to rolling over and overjumping the stems with diameters in the same range as the rock diameters. But we enhanced the manuscript with the sentence: “The ratio between rock and deadwood log diameter plays an important role, both in terms of hindering rolling over of the obstacle as well as in terms of breaking prevention “.

Nevertheless, this underlines the importance of stating the apparent (low rockfall energies, no tree, nor deadwood breakage) in abstract and conclusion, as you proposed in your overall review.

10) p.8 l. 151 : *“Olmedo (2015)” - it can be useful to cite also “Olmedo, I., Bourrier, F., Bertrand, D., Berger, F., Limam, A. Dynamic analysis of wooden rockfall protection structures subjected to impact loading using a discrete element model*

(2020) European Journal of Environmental and Civil Engineering, 24 (9), pp. 1430-1449.

We have expanded our bibliography with the here suitable and suggested literature.

11) p.9 l. 168 : “underline” instead of “underlines” ?

We corrected the verb conjugation.

12) p.9 l. 169: “The here” - missing word?

We resolved this issue due to the rephrasing of the entire paragraph (see your item 13).

13) p.9 l. 168-184: this section is not clear: it can be improved,

We have amended the relevant section and clarified our statement about the absolute and runtime-relative number of frontal impacts per forest state. The section has been enriched with additional content (see explanations to item 6)

14) p.9 l. 182: “m. s-2” instead of “m.s-1”

We adjusted the wrong units in the attached manuscript.

15) p.10 l. 202-204: I don't understand

We enhanced this section and clarified our statement about the stopping criterion. As mentioned in item 6) the discussion about the associated disparities between the mean translational velocities of the simulation and experiments, is added to this paragraph.

16) p.10 l. 215 : “trees Bourrier et al. (2012)” : typo ?

We adjusted the typo within the source directly in the attached manuscript.