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Comment on esurf-2022-21

Anonymous Referee #2

Referee comment on "Modeling deadwood for rockfall mitigation assessments in windthrow areas" by Adrian Ringenbach et al., Earth Surf. Dynam. Discuss.,
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This paper presents a tool to investigate the effect of deadwood on rockfall hazard. This tool consists of both an automatic deadwood generator (ADG) and a block propagation model integrating forest and deadwood. The approach proposed consists in comparing several scenarios (forest without deadwood, forest with "fresh" deadwood, forest with "decayed" deadwood, no forest).

The main interest of the paper is the ADG which is clearly presented. Its accuracy is thoroughly analysed on two study sites. On the contrary, the study related to rockfall simulations is less rigorous. First, the model used is not described in details (although it was presented in other papers) and therefore essential assumptions are not clearly stated (models of block interaction with trees and deadwood, in particular). In addition, as regards to the block propagation simulations on the two study sites, the authors do not clearly state an essential point (the small size of the block) and there is no comparison with field surveys or experiments, which reduces a lot the impact of the results. This is not a problem by itself but it has to be clearly stated.

Consequently, I recommend publication of the paper only if the paper is substantially improved on these points.

I have additional comments that also have to be taken into account :

1) p.3 l. 30: "likely:" □ typo.

2) p. 3 l. 41: the term « absorbed rockfall energy » is not correct in my opinion since the energy of the block is not absorbed, it is converted into kinetic energy of the tree, in particular. This term is thus misleading on the process. I would prefer « block energy reduction ».

3) p.4 l.74-80 : The section should be rephrased so that the reader could clearly understand that : the ADG allows to reconstruct deadwood spatial distribution directly after windstorm and also few months after and the rockfall model, depending on the assumptions (modelling of deadwood breakage depending on the level of decay, in particular) allows to assess block propagation in different configurations (i.e. with and without deadwood). In the present form, the sentence could let the reader think that the assessment of rockfall hazard is mainly depending on the ADG, which is not the case. It should be clearly stated that it depends also on the level of accuracy of the block propagation model.

4) p. 4 l. 84 : « software » In my opinion, it is not a software, more a python script

5) p. 4 l. 94 : the variables pw1 and pw2 should be presented here. In addition, I think that, for more genericity, only a variable pw should be presented.

6) p. 4 l. 96 : can you justify the values 1.5DBH and 0.65DBH ?

7) p. 5 l. 108 : can you justify the values 10 % to 20 % : what is the effect of this assumption ?

8) p. 5 l. 109 : the H_min values are depending on each study site. What is the effect of this assumption ? Here, it should be stated that there is a potential effect of this value.

9) p. 5 l. 119 – 122 : this section let the reader think that the breakage of deadwood is modelled into the rockfall simulations. Is it the case ? If so, the integration of the breakage has to be explained in details.

10) section 2,3 : this section has to be substantially improved in order to explain in a more detailed (even if detailed equations are not necessary) way the assumptions associated to the modelling (rigid blocks with a given shape, non-smooth mechanics, rigid trunks and rigid logs,...). In particular, the parameters leading the rebound on the soil have to be explained. The modelling of the impact on the trunk and on the logs is not explained and the associated parameters are not presented. It has to be integrated.

11) p. 9 l. 180 : the block size is very small. It is a very important assumption as it strongly influences the efficacy of deadwood. This point has to be clearly presented here but also in the introduction, discussion and conclusion so that the reader can understand the limits of the study.

12) p. 9 l. 186 : « exemplary visualize » : I don't understand

13) p. 9 l. 196 : « 22/79 » means « W1 : 22 , W2 : 79 » ? - to be clarified

14) p. 9 l. 197 : « 2,7-4,9 » : it is not clear that it refers to the slope of the regression – to be clarified

15) p. 9 l. 198 : « equitations » - typo

16) p. 9 l. 199 : At first sight, it is not clear that the range of the regression coefficients are the same (it is a matter of units, I guess) – Am I right ?

17) p. 9 l. 204 : Finally, you conclude that it is not necessary to use a regression since the accuracy is low and that a fixed value set at $RP_{\phi}=3$ DBH is sufficient. Is it correct ? If so, it should be more clearly stated.

18) p. 11 l. 214-215 : this sentence should be moved to a discussion or conclusion section.

19) p. 11 l. 219-234 : This section is difficult to follow . Could you improve it ?

20) p. 15 l. 270-271 : This statement has to be more justified, or explained at least.

21) Figure 8: two last sentences : this point is essential and has to be explained in the main text.

22) p. 15 l. 272-288 : It could have been interesting to compare the distributions of the velocities and energies at given locations of the site (evaluation screens). Indeed, it would give a more straightforward view of the effect of deadwood.

23) p. 15 l. 280-288 : the analysis is based on « energy absorption » which is not relevant , in my opinion. Indeed, as the effect of deadwood depends on many parameters (relative size of blocks and logs, boundary conditions of the logs, incidence of the block), this analysis is too simplified. It is maybe better to suppress it ?

24) p. 16 l. 291 : I would replace « highlight » by « illustrate » since the genericity of the present study is limited.

25) section 3,5 and conclusion : in these sections, the limitations of the study have to be clearly presented and discussed. In particular, the fact that the block size is small is essential to discuss. In addition, the assumptions of the block propagation model, and their effect on the results, have also to be discussed. Finally, I think that an analysis of the effects of the simulation parameters (that are not presented in the paper) on the mitigation effect of deadwood is missing. This point has to be specified as a perspective , for example.