Third review of van Zadelhoff et al. for NHESS by David Milledge

My major concern with the previous version was in the calculation of basal and lateral root reinforcement. The method of calculating each is now clear with lateral reinforcement a depth integration of reinforcement per unit area then basal reinforcement simply reinforcement per unit area at the basal depth.

There is only one minor outstanding issue here. You were unable to reproduce my depth dependent lateral and basal root reinforcement values though they had been generated using your equations and parameters. You suggested that "while creating the figure provided by the reviewer, the values of the coefficients for the calculation of the basal and lateral root reinforcement were mixed up." However, this is not the case.

Instead, I think the mismatch in our root reinforcement-depth functions is related to how we are parameterising the gamma function. I have previously implemented your equations in Excel and have now repeated the exercise using the Matlab function (gampdf(x,alpha,beta)) using exactly the parameter values in your R script. In both Excel and Matlab I get the same result but this differs from your result generated using the R function dgamma(x, shape, rate = 1, scale = 1/rate, log = FALSE). I'm not an R user so I can't check this in R, and I had to look the function up online. I found it here: https://stat.ethz.ch/R-manual/R-devel/library/stats/html/GammaDist.html

In all cases (excel, R, Matlab) there is agreement with your equation (9) both in relation to the functional form of the gammapdf and in how the parameters α (shape) and β (scale) are defined. However, I strongly suspect that the syntax involved in calling the dgamma function in R is leading to the scale (β) parameter being miss-defined as the rate (λ=1/β) parameter in the R function. I suspect this because when I use your beta_2 value as a rate (i.e. λ=3.688) then convert to scale (i.e. β =1/ λ = 1/3.688 = 0.2711), I can reproduce the reinforcement curves in your response.

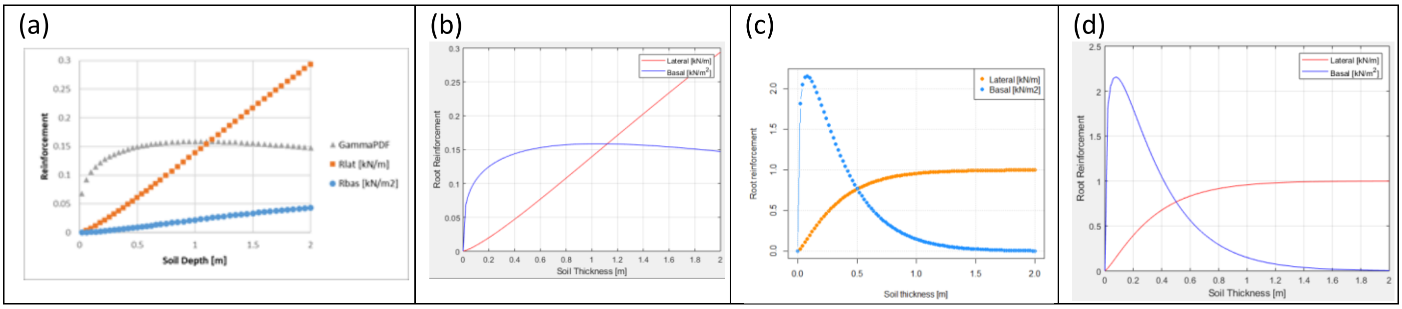

Fig. 1: a) my excel implementation of your previous equations using α=1.284, β=3.688; b) my Matlab implementation of your current equations using α=1.284, β=3.688; c) your R implementation of your current equations using α=1.284, β=3.688; d) my Matlab implementation of your current equations using α=1.284, β= 0.2711 (i.e. 1/3.688). Note the similarity between GammaPDF in a and basal in b (which is expected given the RRmax coefficient =1) and the similarity between Rlat in a and Lateral in b (which is expected since the functional form is unchanged in the new version). Note also the similarity in both Lateral and Basal curves in c and d when the second parameter is adjusted in Matlab to account for the rate-scale mismatch.

I'm not a frequent user of gammaPDF functions so I was concerned that I could be getting something wrong but the results I can find online seem to support my implementation and suggest that the error is in the R implementation.

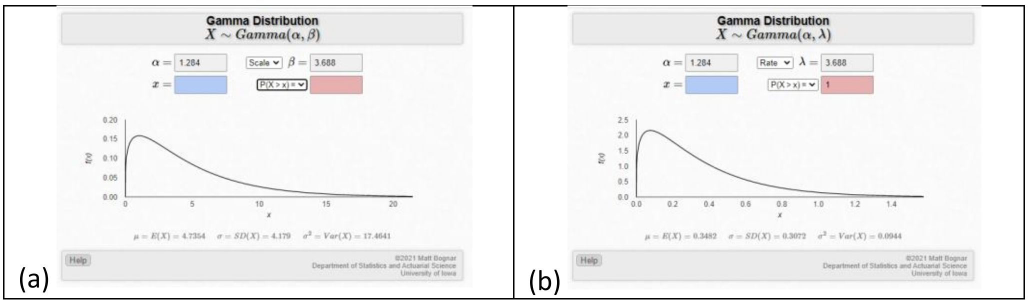

Fig. 2: Gamma distributions generated from an online calculator using: a) your parameters as shape and scale parameters; b) your parameters as shape and rate parameters. The online calculator is available here: https://homepage.divms.uiowa.edu/~mbognar/applets/gamma.html

Minor Comment 1: The need for a clearer justification for your choice to neglect passive resistance. I asked that you clarify the term: "second stage of the activation phase"; and provide citations to support your argument that passive resistance can be neglected in this phase. In response you added the following text: "The forces assumed in SlideforMap are typical for the second stage of the activation phase: the displacement at which lateral root reinforcement is maximised under tension along the tension crack and at which passive earth pressure, lateral root compression and lateral soil cohesion are assumed to not be fully mobilized (Cohen and Schwarz, 2017) and we choose to neglect these." It is problematic to neglect the resistance on the basis that it is "not fully mobilised" because that gives no indication of its relative or absolute magnitude. It could mean (in relative terms) that the resistance in this phase is 90% of the maximum (fully mobilised) resistance; alternatively, even if the resistance were only 10% of the maximum this could still be larger than the resistance provided by roots. In fact, Cohen and Schwarz (2017) say: "The largest force that contributes to slope stability is soil compression in the area above the landslide toe (p472)"; and "soil compression, due to its magnitude, dominates and controls the slope stability and its time to failure. (p472)"; their Fig 3 suggests passive earth pressure >5 kPa for most of your second phase displacement range (0.01-0.1 m). I don't think you can use this argument to justify your choice. You might instead neglect passive resistance because it is too difficult or complex or uncertain to model but if that is the case you should say so. It would also be helpful to explain your choice to model the second activation phase rather than first or third. Why is the second phase the most important phase to model?

We indeed choose to neglect passive resistance. We argue that adding all lateral forces (assuming both activation phases simultaneously) as done in previous models is too optimistic for slope stability. Therefore, we add just one. We argue not all resisting forces are activated at once as shown by the results in Cohen and Schwarz (2017). We focus the modeling on the phase where lateral root reinforcement reaches the maximum values. Our focus on the detailed inclusion of vegetation contributes to this choice. In this phase, we argue that it is more conservative (safer) to neglect passive earth pressure forces, because they are not fully activated and the magnitude considerably changes depending on the stiffness of the landslide material and the dimension of the landslide. Further research and work on this topic is definitely required. We improved this argumentation in the paper (L154-159).

This is a good response why not include it more completely in the paper? You make several points here very clearly that you don't really make in the paper itself. You recognise here that you are choosing to represent one particular phase of the failure process and choosing to do so partly because that is the part that interests you most as opposed to based on some argument about which stage is the limiting stage in generation of landslides that run out rapidly over long distances causing considerable damage. I would like to see you move this text into the paper in full, I think it will help readers understand your motivation and approach, but that is only a suggestion.

Minor Comment 2: Some statements in the paper have not been updated to reflect new results and are now incorrect. For example you say "The mean angle of internal friction shows a high variation, from a very low value for StA to close to the maximum tested value for Trub" (L498-9); but with the new results, variation is much reduced, Trub is far from maximum and StA is now the second largest value. On L527 you say "This is unique for shallow landslide probability models"; but this is contradicted in your discussion of PRIMULA as a probability model with a treatment of basal and lateral root reinforcement (L607-13).

Thank you for pointing this out: we read the paper thoroughly and believe that all results now correspond to the current submission.

This has now been addressed.

Minor Comment 3: Recognising similarities between your 'no vegetation' model and SINMAP. I suggested that in the discussion of similarities to other models (Section 5.4) you explain that your 'no vegetation' case differs from SINMAP only in: 1) the form of the distributions that you use to sample c and phi (log-normal for you, uniform for SINMAP); and 2) the treatment of soil depth, which is spatially uniform in SINMAP and spatially variable for you. This means that the AUC values for 'no vegetation' are indicative of those that you would expect from SINMAP. This was not addressed in your response but I think it is important that you do add this discussion because it has implications for the trade-off between model complexity and model skill. The no veg case represents a simple (low parameter) infinite slope model very similar to SINMAP that could be widely applied. If it works almost as well as the more complex model then that is an important finding worth reporting.

We thank the reviewer for the suggestion of putting the SlideforMAP "No vegetation" scenario in direct comparison to the well-established SINMAP model. We added an overview of the complexity/model skill trade-off and put this in connection with SINMAP and SlideforMAP (L590- 593) as we also believe this would be helpful to users. We did not test SINMAP, therefore we think AUC similarities are speculative. The differences between the model results due to parametrizations from different distribution and the uniform vs. adapted soil depth can be significant. It is not the aim of this paper to compare in detail the results of other models.

Thank you, I think you could perhaps have pushed this further, but I think it is suitable and useful as it is.

A summary of each of your vegetation scenario models, and what they mean for model structure would slot nicely into Section 3.4.3 'Vegetation parameter scenarios' (L425-30). Section 5.4 would be the place to add any interpretation of the performance of different parameter scenarios in terms of their implications for the value of different model structures i.e. what is gained by representing lateral reinforcement in general then what further gains are available when you use single tree detection. In Section 5.6 (L601) you say: "In comparison to more simple models based on infinite slope calculations (Pack et al., 1998; Montgomery and Dietrich, 1994, SINMAP,SHALTAB), SlideforMAP considers the effect of lateral root reinforcement". This is the perfect place to point out that SfM in the 'no veg' case becomes an infinite slope calculation that does not consider the effect of lateral root reinforcement. You could then report performance of this version (which neglects lateral reinforcement) and comment on the performance improvement associated with representing lateral reinforcement. My suggestions in relation to Minor Comment 3 are only suggestions though, not requirements, I'm happy for you to simply say no thankyou we don't want to do that. We thank the reviewer for pointing this out.

Our idea was to indicate the benefits of the inclusion of lateral root reinforcement over infinite slope calculation, but we agree that we can state this more explicitly, both in the vegetation scenarios and the discussion. We revised the lines accordingly (L531 - 542).

Thank you, this is now clearer.

Line specific comments: these have all been addressed so there is no need to reproduce them here.