nhess-2021-140 -Introducing SlideforMap; a probabilistic finite slope approach for modelling shallow landslide probability in forested situations - van Zadelhoff et al., 2021

We thank the reviewer for taking the time to rereview our paper. Below, our responses are in black. Red and green are the comments of the reviewer. Results were recomputed to create the AUC curves in Figure 10. Consequently, due to the probabilistic nature of the model, the results change but the conclusions of the paper do not. We took the comments to our computation of root reinforcement seriously and reformulated how we compute the lateral root reinforcement. We hope this is understandable now.

Reviewer 1; GENERAL COMMENTS

1. On L50, I do not quite agree with the authors' assertion that "statistical" and "probabilistic" models are distinguished by whether physical processes are included. It is entirely possible to have a probabilistic model that does not take physical processes into account.

This is indeed indisputable. We revised the text (L 50).

2. On L119, the authors state that SlideForMAP is intended for application on a "scale of 1-1000 square meters." What exactly does this mean (study area, raster cell size, etc.)? This description is confusing in relation to L188, where the authors indicate that the maximum landslide surface area is 3000 square meters.

This is the scale of application of SlideforMAP in square kilometers. This paragraph aims to explain the reader on what study areas it can be applied and to quantify our definition of 'regional scale'. We revised the text to make this clearer (L 119-120).

3. On L177-178, the authors are referring to the landslide density, which indicates the number of landslides per unit area. However, it is unclear if the authors mean that there should be more than 1 center of mass per raster cell, or whether it would be sufficient for each cell to be overlapped by more than one HL, which could occur with a landslide density significantly less than 1 center of mass per cell if the landslides were sufficiently large.

Thank you for the detailed reflection. Not every raster cell should have a random landslide center of mass, overlap suffices. Due to this the landslide density can be less than one 1 per raster cell.

4. In Figure 6, it appears that the number of subsamples retaining the full parameter subset varies. Shouldn't these all have a count of 100?

The total is 1000 samples from a uniform distribution using Latin Hypercube sampling divided over 10 bins. 100 samples per bin is the mean, but minor inaccuracies occur due to sampling.

TECHNICAL CORRECTIONS

L45. Change "spatial" to "spatially".

Thank you, we corrected line (L45).

There is some inconsistency throughout the paper in the use of "SlideForMAP" versus "SlideForMap". Please ensure that the name of the model is consistent throughout.
Thank you for pointing out this inconsistency. After decision making with the team, we settled on 'SlideforMAP'. We made this consistent throughout the paper.

There is some inconsistency throughout the paper in referring to parameters using mathematical notation and fonts (for example, "a and b" in L 209). Please ensure that mathematical notation is consistent throughout.
Thank you, we made this consistent throughout the paper.

Review of van Zadelhoff et al. for NHESS by David Milledge
Thank you to the authors for taking the time to respond to my comments, almost all of which have been addressed. However, I have one major outstanding concern. I think that the equations for basal and lateral reinforcement are still incorrect. It is essential that the authors address this prior to publication. I also have three, fairly minor concerns that can be very easily addressed:

1. The need for a clearer justification for the choice to neglect passive resistance;
2. Some statements in the paper have not been updated to reflect new results and are now incorrect;
3. Discussion of similarities between your 'no vegetation' parameterisation of your model and SINMAP.

My other concerns from the previous review have been addressed. Each of the remaining concerns has some history in previous reviews I have tried to summarise my concern and the history from previous reviews below.

Major Comment: Representing basal root reinforcement as a function of lateral reinforcement is incorrect
It is now clear that you use a Gamma pdf and the additional equation is useful, you clarified in your response that you used numerical integration but should update the text to reflect this. However, my main original concern has not been addressed. Eqn10 calculates basal reinforcement as a function of lateral reinforcement. That is incorrect conceptually because if lateral reinforcement is a depth integration of reinforcement per unit area then basal reinforcement should simply be reinforcement per unit area at the basal depth. Your response to my previous comment suggests that you agree (you said "The equation in essence is the same in both equation 8 and 9. Equation 8 however, applies the cumulative value (soil depth in range [0,x]), equation 9 applies the point value soil depth at [x]"). However, your implementation is at odds with this. Eqn 4 in Gehring et al. (2019) is much more consistent with both your response and my expectation because they use of Rmax rather than Rlat. Is it possible there is an error in Eqn 10 in the paper and it should be using Rmax?

We updated the text to explicitly state that we use numerical integration (L245).

Our previous response was indeed not clear. We revised the original equations and now present lateral root reinforcement as a function of maximum lateral root reinforcement (N/m) and vertical root distribution (i.e. maximum lateral root reinforcement is reduced by the assumed root distribution over the soil depth), i.e. it is not reinforcement per unit area but per unit soil depth. The new equations now reads at:

$$RR_{\max} = (c \cdot DBH) \cdot \Gamma_{PDF}\left(\frac{D_{\text{trees}}}{DBH \cdot 18.5}\middle|\alpha_1, \beta_1\right),$$

The maximal root reinforcement RRmax (N/m) is a function of tree distance D_{trees} [m] and tree diameter DBH [m]. RRmax refers to the maximum (lateral) force of a root bundle under tension. The factor of a1 and b1 are both dimensionless, rendering the Gamma PDF dimensionless. c has the unit N/m^2.

$$R_{\text{lat}} = RR_{\max} \cdot \int_0^{H_{\text{soil}}} \Gamma_{\text{PDF}}\left(H\middle|\alpha_2, \beta_2\right) dH,$$

The lateral root reinforcement Rlat (N/m) depends on soil depth H (m) and is formulated as an integral of a gamma density function. The factor of a2 is dimensionless and b2 is in [m]. This definition renders the integral dimensionless.

Equation 8 in our paper was identical to Equation 3 in Gehring et al., 2019, except for an addition of a depth correction factor. This was poorly formulated in the paper. We reformulated this by adding RRmax explicitly and redefining the depth correction in a separate subsequent equation (Equation 8 and 10 in the paper). The equation in the previous manuscript version reads as:

$$R_{\text{lat}} = c \cdot \Gamma_{PDF}\left(\frac{D_{\text{trees}}}{DBH \cdot D_{\text{trees,max}}}\middle|\alpha_1, \beta_1\right) \cdot \int_0^{H_{\text{soil}}} \Gamma_{PDF}(\alpha_2, \beta_2)\, dh$$

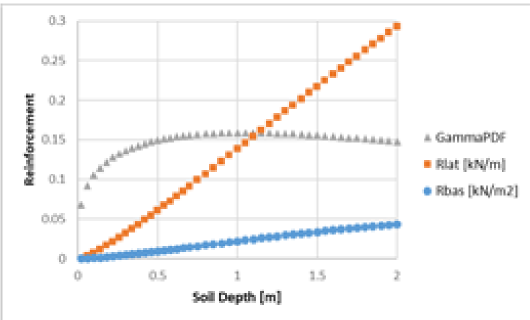
The basal reinforcement is defined as the maximum lateral root reinforcement multiplied with the root distribution value at maximum soil depth (with unit 1/m), which results in unit N/m^2 or Pa.

In addition to the conceptual problem, your approach introduces two errors:

1. The dimensional error that you have to correct with the unit coefficient k.

We believe this not to be an error, but to be coherent with an assumption of isotropic root reinforcement. The same assumption is made in Gehring et al., 2019, though not explicitly stated. We reformulated the calculation of lateral root reinforcement, starting with the calculation of RRmax.

2. A relationship between basal reinforcement and depth that is at odds with observations of root density and reinforcement. Your group's previous observations (e.g. Schwarz et al., 2012) and those of other groups all suggest that reinforcement declines with depth, or at least does not increase. Your equations ensure that basal root reinforcement increases with depth. The graph below was generated using your gammaPDF parameters with an Rmax ($\boldsymbol{Rmax} = \boldsymbol{c}\,\boldsymbol{\Gamma}\,(\,\boldsymbol{Dtrees}\,|\boldsymbol{\alpha 1}, \boldsymbol{\beta 1})$) of 1 kN/m2 (to keep things $\boldsymbol{DBH}\ \boldsymbol{Dtreesmax}$ simple). I'm convinced that this representation is incorrect and that it is potentially important because in shallow soils, where basal reinforcement is particularly important, it will result in a very large underestimation of reinforcement.

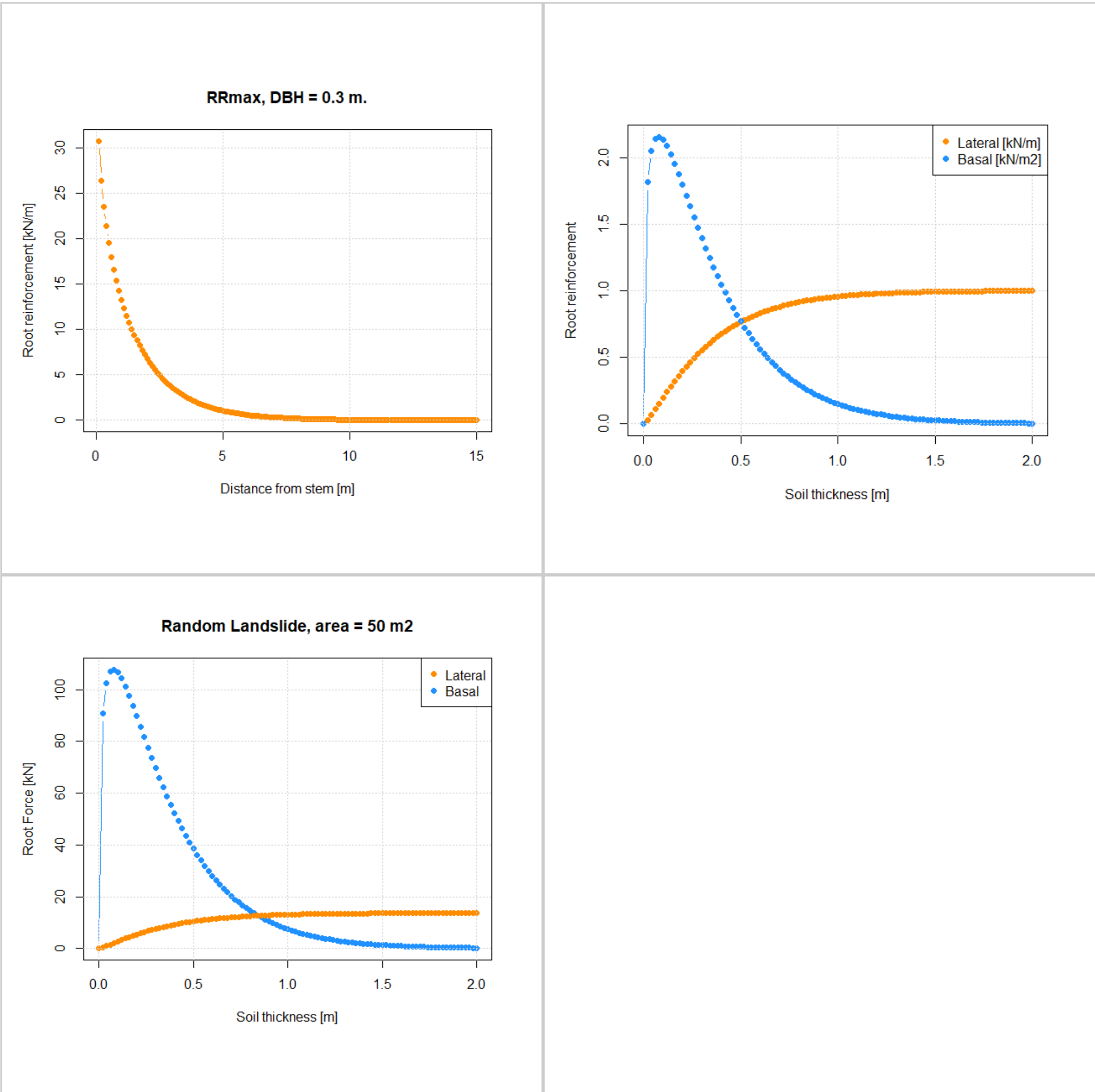


My second concern having plotted the graph above is that the parameters of your gammaPDF implemented with Gehring et al.'s (2019) parameters implies that root density initially increases rapidly with depth while your own observations of the vertical distribution of roots in Switzerland and many observations from other groups all suggest that root biomass (and thus reinforcement) decays with depth, usually exponentially (see Dazio et al., 2018; Schwarz et al., 2012 and references therein: Abe and Iwamoto, 1990, Abe and Ziemer, 1991, Abernethy and Rutherfurd, 2001, Schmidt et al., 2001, Schenk and Jackson, 2002, Bischetti et al., 2005, Laio et al., 2006, Docker and Hubble, 2009). This suggests that your alpha2 and beta2 parameters are not physically reasonable.
Do you agree that gammaPDF values should be lower at 2 m depth than they are at 0.2 m depth?

We agree with the reviewer that basal root reinforcement should decrease with depth. In addition, we expect that the Rlat should be approximating the value of RRmax much sooner than displayed in the figure provided by the reviewer.

Our hypothesis is, 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. Alpha 1 and Beta 1 are the coefficients for the Gamma Probability Density function (PDF) of root density in the horizontal direction. The latter is used to compute RRmax.
Alpha 2 and Beta 2 (Table 1) are the Gamma PDF coefficients for the root reinforcement distribution in the vertical direction. That is used to compute lateral root reinforcement and basal root reinforcement from RRmax.
Below we produced figures on the basal and lateral root reinforcement. The upper right figure is our result of the lateral and basal root reinforcement with soil depth, akin to the figure as given by

the reviewer. In our calculations the results are consistent with the examples in the literature, cited by the reviewer. We produced a concise script (in R), provided in the supplementary material, that produces these figures.



upper left: Maximum root reinforcement (RRmax) as a function of horizontal distance from stem.
upper right: Lateral and basal root reinforcement for an assumed RRmax of 1 kN/m as a function of soil thickness. Under this assumption The Gamma PDF is identical to the basal root reinforcement.
lower left: Resulting Lateral and Basal Root force as a function of soil depth. This assumes RRmax of 1 kN/m and a Shallow landslide of 50 m2 surface area.

A minor point, Table 1 would be improved by replacing 'literature' entries with the relevant references.

We agree. We revised Table 1.

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).
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.

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.

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).

Line specific comments:

L473: Why did “10 repetitions” of the global uniform veg produce such different mean AUC values (Table 8) to the AUC recovered during calibration (Table 7)? Global mean AUCs for each catchment (Table 8) are 4-7% lower than calibrated values (Table 7). The calibrated value is 1.8-3.9 standard deviations from the mean depending on catchment. Perhaps this is because the calibrated AUC takes the maximum from a large number of random realisations. However, in that case the calibrated AUCs (and associated ROC curves, Fig 8) are an inflated values, which are not representative and should not be reported elsewhere in the paper (e.g. L15, L636). The AUCs in Table 8 are much more representative and should be reported instead. You should consider entirely removing the AUC row from Table 7.

You are right that the AUCs in table 7 are inflated values. They are the maximum of the large number of random realisations and in that sense are 'lucky guesses'. We agree and removed any mention of these values outside of Table 7. We updated the ROC curves to correspond to the 10 repetitions as presented in Table 8.

L475: “compared to the reference scenario used for calibration, the AUC remains almost unchanged or slightly increases for the vegetation based on single tree detection.”: I have three points here: 1) this text appears to contradict later claims that single tree detection improves model performance (I think this problem will go away when you address my second point); 2) you shouldn’t compare to the AUC for your reference because this is the maximum of a much larger number of trials (see previous comment) so the comparison is not a fair one; 3) Can you be more specific? This is a key result! it is worth adding a little more detail on the relative improvement for each site and on average. From a quick look, I think that in 2 of 3 cases AUC is increased by 1- 4% in the other it is reduced by 4% and on average there is a 1% increase in AUC.

Thank you for pointing this out as well as for the suggestions for improvement:

1. we removed the sentence and formulated these results in more detail in line with other comments by the reviewer.
2. In line with previous comments we revised this part and used the results of Table 8 as central to our results and discussion.
3. We added the relative change in AUC of the single tree detection compared to the other vegetation scenarios (L536-537). This combined with Table 9 with the reliability of the difference is presented as a key result (L543-551).

L549: “relative gain of 2%”: It isn't clear what this comparison is referring to, which two vegetation parameterisations are being compared? How significant is the change? You can now comment on this using Table 9.

We improved the description and included a reference to Table 9. We argue that though a seemingly small improvement in AUC, the concept of diminishing returns, makes this a valuable improvement. (L537, L547-548)

L551: “single tree detection is the best performing scenario”: This is a key result. Can you give more detail here? What about StA and what about on average? How much better is it than the next best scenario? Is it significantly better than all the other scenarios or only a subset? Again this is a suggestion, not critical for publication.

Thank you for the suggestion. We added more details to the description of the key result, relating to the study area and chosen scenario (L531- 543).

Additional reference (not cited in paper)

Dazio, E., Conedera, M. and Schwarz, M., 2018. Impact of different chestnut coppice managements on root reinforcement and shallow landslide susceptibility. Forest Ecology and Management, 417, pp.63-76.