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

Response to the comments of the reviewers

In these responses, we provide the original comments (in italics) and our related responses. The corresponding changes in the manuscript are indicated in the track-change version submitted along with the revised paper at the end of this response. for readability, we removed certain old section in the review-rebuttal threads and marked these as: (...)

Dear Editor, This is my second review of the manuscript proposed by Van zadelhoff et al. I went through the point by point replies and the revised paper. I can confirm that the authors have responded adequately to most of the comments/suggestions and made significant improvements to the manuscript. The two criticisms that I had raised actually remained, i.e. the ones related to the rainfall intensity as calibration parameter and the selection of only rainfall events of 1-hour duration. Please, read below a few additional comments.

1. Literature review (introduction/discussion).
The author added this sentence in the introduction, under my request of discussing the hydrological effects of vegetation. L95-98: "The hydrological effect influences effective soil moisture by interception, increased evapotranspiration and increased infiltration (Greenway, 1987; Masi et al., 2021). Over the short timescale with intense rainfall these hydrological effects are negligible (Feng et al., 2020)."
Actually, I found it as an extrapolation of an opposite statement suggested in my first review!

MY PREVIOUS COMMENT: "With regard to the effect of vegetation, the aspects related to the hydrological effects should be at least discussed, which can sometime be even more significant than the mechanical ones (Feng et al., 2020)".

MY NEW COMMENT: I absolutely agree that during the single event the hydrological effect of vegetation are negligible, but in a long-term analysis, which is required to assess the initial condition prior the event, this is not true. Please smooth it.

The authors agree and we smoothed the text (L97) emphasizing the importance of knowledge on initial conditions. In the methodology we stated the shortcoming of SlideforMap that is does not include prior knowledge on the initial conditions (L255).

2. Hydrology and precipitation.
MY PREVIOUS COMMENT: "In the description of the model application (section 3.4.2) it is not clear how rainfall initiating events are selected. If I understood well, only events of 1hour duration are selected, whose intensity is identified from the Depth-Duration-Frequency (DDF) curve at different return periods (i.e. from 10 to 100 years). Therefore, I guess 10 events of 1 hours are simulated. Is that correct? If so, it should be explained and justified the reason of analyzing events of only 1 hours, which cannot be 'critical' for landslide initiation. Authors should deeply clarify this part in the manuscript, explain the methodology used to define the events, and report the parameters of the DDF curves". –

AUTHOR REPLY: We emphasized the choice for 1-hour events (assumed macro-pore activation time period) in the methodology L256 and the parametrization of the DDF curves in the supplementary material.

MY NEW COMMENT: I think that this is still a strong limitation. I would suggest to report the values of the rainfall intensities to have an idea. Just report a summarizing table in the paper. I think that rather than the R.code (supplementary material, that I appreciated though) it would be more useful to have the resulting rainfall intensities.

Thank you for pointing this out. we added a table with the rainfall intensity to corresponding return periods per study area in the methodology (Table 5). A 1 hour and 24-hour period were chosen to account for both a short and long duration event. The maximum and minimum rainfall depth of the Table is subsequently used in the sensitivity analysis.

3. Calibration/sensitivity analysis
MY PREVIOUS COMMENT: "With regard to the best set of parameters, my question is: are the found parameters consistent and realistic? For example, I argue the choice of including the precipitation intensity as calibration parameter. As discussed in the previous comment, rainfall represents the triggering forcing and it is a dynamic variable. Ideally, we should know the precipitation intensity associated to each observed landslide. Otherwise, if used as parameter, it seems that the model is tuned ad hoc just to reproduce the past events. If so, which could be its utility?"

AUTHOR REPLY: We agree that in an ideal case this should be known, however no detailed information is available. Therefore, we have to rely on the more simplified steady state approach. We added the lack of detailed information in the data section to justify our approach L335-336.

MY NEW COMMENT: The criticism still remains. Then, it should be clear that the calibrated model (with these found parameter values) cannot be used in a forecast usage mode, i.e. with rainfall series as a forecasted input.

We updated the introduction to strongly emphasized that SlideforMap should not be used as a forecast tool (127 to 129).

Review of van Zadelhoff et al. 2021 for NHESS by David Milledge I made a community comment on the first version of this manuscript and was then asked by the Associate Editor to review the revised paper. The revisions have improved an already very interesting paper, which demonstrates the applicability and predictive capability of a new model for shallow landslides to provide a detailed inclusion of the influence of vegetation. I have responded in bold to the authors' responses below. I think my additional comments or queries are only fairly minor and should require only fairly minor alterations to the paper. The only exception to this the updated Equations 8 and 9, where I think there is an inconsistency in the way basal and lateral root cohesion are being calculated.

(...) The comparison to other models in Section 5.6 is very useful. However, I think there are a few other models that capable of catchment scale application and that resolve lateral root reinforcement which are worth discussing. Montgomery et al., (2000) is particularly important to discuss in Section 5.6 because they use your hydrological model and a very similar geotechnical model (infinite slope with roots the only lateral reinforcement). Other important models to consider are: Hess et al. (2017), who also assume fixed dimensions for the landslides but include a treatment for boundary friction and passive resistance (which you neglect); Cislaghi et al (2017, 2018) sample landslide sizes from a distribution (similar to your approach); von Ruette et al. (2013) and Bellugi et al. (2015) predict size in the model rather than imposing a single size or distribution of sizes. I suspect that both of the latter are fairly slow to run relative to your approach and you certainly improve on the approaches that define a single landslide size. You should perhaps comment on the ways in which your approach differs from that of Cislaghi et al. (2017, 2018). It would be useful to say a little more about the respects in which SfM "uses a more realistic implementation of root reinforcement" and how this differs from these other models. If the main difference is that you neglect compressive resistance this may need stronger justification either here or in the methods section.

Do any other studies predict landslide locations accounting for the spatial distribution of root reinforcement as a function of forest structure? We can argue about the specifics of the stability model but this for me is probably the most novel aspect of the work.

We added a comparison to the forest clearing model by Montgomery et al., (2000) in the discussion (L583-587). Differences to SlideforMap in this model are quite profound in their fixed values for soil depth and landslide dimensions, which are variables from distributions or in SlideforMap. Their fixed value for root reinforcement also is a different approach from our spatial distribution.

As suggested by the author we added a comparison to PRIMULA by Cislaghi et al. (2018) in the discussion (L588-594). We agree this is important as they use a similar application as SlideforMap, the main difference is our computation of root reinforcement on a shallow landslide scale

In comparing to Hess et al., (2017), the difference is that to our knowledge, adding all forces (both passive and active) at the same time overestimates the slope stability. We make our argument in the methodology (L153-155) and added a comparison in the discussion (L594-596)

We see a similarity in computation of slope stability by von Ruette et al. (2013) and Bellugi et al. (2015), but the approach is different from ours by focusing on a single cell, where we focus on the whole landslide surface area. We think this is not appropriate to compare here, as they are more similar to SOSlope (Cohen & Schwarz, 2017).

I also had a couple of other minor comments on the new text:

L113-4: "displacement independent": I think your approach also models displacement-independent reinforcement. If you make the point that this is a limitation of previous approaches up here, you should probably add in your comparison to other stability models that SfM uses displacement independent reinforcement whereas Cohen and Schwarz (2017) and to some degree von Ruette et al. (2013) account for displacement in their treatments.

We worded this poorly. Our model applies the maximum root reinforcement under tension, which is in itself a displacement independent value, but based on displacement-tension based pullout tests. We worded this more concise (L114) and do indeed think this experimental root reinforcement is an improvement upon a single assumed value. We asserted this in our objectives (L 123).

L155: "second stage of the activation phase" this isn't explicitly referred to in the introduction so it is not clear what you are pointing back to here. The sentence that follows ("This coincides with…") is very helpful and it might be easier to understand this paragraph if you simply said something like: "…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 no longer act."

Thank you, we took the reviewer suggestion and defined the second activation phase (maximum tension related forces, neglected compression related forces) right after introducing the term (L 153-157).

(...) We expected an improvement in the performance of the model with the detailed inclusion of vegetation, which we analyzed in the paper. This sentence is great! It captures your hypothesis for the paper really nicely and would be worth including around L124 because I don't see such a clear statement of your expectation in the current paper. (...) This additional text is useful.

Thank you for the suggestion. We included the hypothesis of performance improvement by including vegetation this in our objectives (L 123).

(...) I think it would be useful in the discussion of similarities to other models (Section 5.4) to 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. I would have really liked to see a comparison to SHALSTAB in terms of AUC because it is so simple and so widely used. But it certainly isn't essential that the authors choose to do that.

We agree that comparison of AUC values in our study areas generated by both SHALSTAB and SINMAP to SlideforMap would be interesting but the presented material is already extensive".

(...) In the literature. I have one final suggestion in relation to Table 7, since you know mean, standard deviation and sample size you should be able to estimate the significance of differences in AUCs between different approaches. A quick attempt at this for the overall performances using a t-test (which may not be the best approach) suggests that single tree detection is significantly better than no veg and Forest uniform veg at 99% and 90% confidence respectively; but is not significantly different from Global uniform veg. The Global uniform veg is significantly better than the no veg case but the Forest uniform veg is not, nor is there any significant difference between Global and Forest uniform veg cases.

We agree and added a the generalized (Welch) t-test (Table 9) on the values as computed in Table 8 identifying the reliability of the difference in the mean, finding a significant difference in the improvement of the AUC by single tree detection in 2 of the 3 study areas. We added the significance of this result to the discussion as well.

(...) It would be helpful to put a range of values to the 'minor movement' that you have in mind (I guess this is on the order of 10-100 mm?). If I understand L153-54 correctly you argue that beyond this displacement there is no passive earth pressure, lateral root compression or lateral soil cohesion. I think each of these claims needs a citation. How do you reconcile that with your earlier findings that passive resistance remains important even for displacements > 300 mm (Schwarz et al., 2015; Cohen and Schwarz, 2017)? Alternatively, if your reason for neglecting some of these components is that we don't yet understand them well enough to represent them satisfactorily then I think it is fine to simply say that here.

The maximum passive earth pressure is activated in a different moment than the tensile forces in the tension crack, though we agree there still will be a certain compressive force under maximum tensile force. This depends strongly on the stiffness of the soil, which we do not yet fully understand (as formulated well by the reviewer). Our understanding in the initiation phase so far of shallow landslides Askarinejad (2012) and partly from Cohen and Schwarz (2017); Figure 7 with a numerical approach. Our negligence of the passive forces is a short

As suggested by the reviewer we added a specific range for minor movement from Schwarz et al., 2015, giving a 10-100 mm displacement (L 157).

(...) I still don't understand why lateral soil cohesion would go to zero at some displacement but basal cohesion would be unaffected, unless you are assuming that a tension crack opens up along the entire upper half of the landslide. In that case, it might be worth adding a comment that explains that you neglect all resistance in the compression zone and assume that a tension crack opens along the entire length of the tension zone such that lateral resistance in this zone is only due to root reinforcement.

We assume as the reviewer describes that a tension crack opens up along the entire upper half of the landslide circumference indeed leaving root reinforcement as the sole lateral resistance. The behavior of initial movement and cracking in the upper part of the landslide is well described in Askarinejad (2012), Figure 14. As with the previous comment, we described this more explicitly (L 153-157).

(...) I think adding a sentence that explains that: you neglect all resistance in the compression zone and assume that a tension crack opens along the entire length of the tension zone such that lateral resistance in this zone is only due to root reinforcement. I don't necessarily agree that this is the best approach, I would do it differently but this isn't my model.

As with previous comments. We read this in displacement measurements of shallow landslides Askarinejad (2012) which indicates that the upper part start moving first and independently from the bottom part. A similar behavior is numerically modelled in Cohen and Schwarz (2017); Figure 7, which shows that the maximum compression force is after the tension crash has opened up. But of course, we can agree to disagree.

(...) OUR PREVIOUS REPLY: We integrated the RBM root probability density distribution and added this as a correction factor to the lateral root reinforcement. This is described in the methodology section 5, equation 8 and the results are recalculated.
This sounds great. However, I'm still a little confused. Perhaps out of ignorance around Gamma distributions. I have two questions: 1) is this an analytical integration? If so can you write it out in Equation 8? 2) if the parameters should be selected to ensure that the function decreases with increasing Hsoil does that require alpha=1, in which case the function becomes an exponential and everything gets simpler? 3) is the second gamma density function in equation 8 the same as the gamma density function in equation 9? The notation in the equations and the definition in the text suggests not but I haven't understood the distinction between the two. Looking at equation 8 again, I think there may still be an error in the way that you relate basal and lateral reinforcement. The best way I can illustrate that is to set the alpha parameter to unity in the gamma distribution so that it reduces to an exponential distribution. In that case I can rewrite equation 8 as:

$$R_l = c\,\Gamma\left(\frac{D_{trees}}{DBH\,D_{treesmax}}|\alpha_1,\beta_1\right)\int_0^H \Gamma(h|\alpha_2,\beta_2)\,dh$$

Applying alpha=1 and substituting a constant C0 for the first term (since this is depth invariant) gives:

$$R_l = C_0 \int_0^H \Gamma(h|1,\beta_2)\,dh$$

In this case the gamma density function simplifies to an exponential and the equation can be re-written as:

$$R_l = C_0\,\beta_2 \int_0^H e^{-h\beta_2}\,dh = C_0\,\beta_2\left(\frac{1-e^{-H\beta_2}}{\beta_2}\right) = C_0\left(1-e^{-H\beta_2}\right)$$

If root density follows this (exponential distribution) then it is clear that basal cohesion (assuming isotropic rooting and strength) is:

$$R_b = C_0\,\beta_2\,e^{-H\beta_2}$$

In this case the dimensions also work out, without the need for the dimensional correction coefficient k (though k takes a value of unity and can be ignored in calculations). However, applying equation 10 in this case results in:

$$R_b = k\,R_l\,\Gamma(H|1,\beta_2) = R_l\,\beta_2\,e^{-H\beta_2} = C_0\,\beta_2\left(1-e^{-H\beta_2}\right)e^{-H\beta_2}$$

This extra term has a very large influence at small depths, pulling Rb down to zero at the surface, which is not compatible with a gamma root density distribution with alpha =1 (i.e an exponential root density distribution). The differences between the two formulations differ by <20% when depth exceeds 0.5 m and by <5% when depth exceeds 1 m (using beta=3.2 as you do in the paper). Therefore this error (if you agree that it is an error) is unlikely to result in large changes to the stability in the areas of deeper soil where landslides typically occur but should make other parts of the catchment where soils are shallow considerably more stable.

We thank the reviewer for the detailed mathematical analysis. We believe we did not accurately state that we use the Gamma probability density function rather than the gamma function as in the rewritten equation by the reviewer. We adjusted the paper by writing the Gamma probability density distribution explicitly. Additionaly we have the following responses to the numbered questions posed by the reviewer:

1. In our code we apply numerical (approximation) integration instead of analytical integration. We assume a step size of 0.01 meters makes the approximation suitable for practical application.
2. Upon considering, this is wrong, our apologies. The function can take non-monotone decreasing shapes as well (for example: Moos et al., 2016; Figure 3)
3. 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].

Calculating root reinforcement using spatially averaged distance to trees within the Gamma function. Previous applications of the Gamma function (Eqn 9) appear to use it to predict root reinforcement at a known distance from the nearest tree (Moos et al., 2016). Given its nonlinearities, is it reasonable to use an average distance in Eqn 9 rather than evaluating Eqn 9 for the distribution of distances then averaging?
OUR PREVIOUS REPLY: For the current paper, we decided to keep this methodology as it is.
I think that is reasonable, but I think you should say that explicitly in the paper and explain why you make that choice (e.g. because it is prohibitively expensive to evaluate 9 for the distribution of distances).

Thank you for the suggestion. We explicitly stated this choice, attributing it to the tendency of root systems to not overlap (L225-226).

Variability
(...) The only remaining point here is that I don't see where you define the symbols you use to represent these parameters in Table 6. It would also be worth explaining that the parameters of the log-normal are mean and standard deviation of log transformed data. I guess you then transform them back for table 6?

Thank you. Indeed, the values from 6 are log-transformed when applied in the code of SlideforMap. The properties (mean and standard deviation) of the log-normal are the same as that of the originally used normal distribution. For clarity and openness on the method, we added the code transforming a mean and standard deviation into parameters of the log-normal distribution in the supplementary material.

(...) I have just a few remaining minor suggestions / queries:L197: "definitive values for soil thickness": I'm not sure what you mean by definitive in this context.L198: It would help to clarify what sets the "initial thickness" if you added "which is sampled from a log-normal distribution".L202: Why do the coefficients used to find mu1 and sigma1 from mu_h and sigma_h take these particular values (1.35 and 0.75 respectively)? Did you establish them by trial and error based on your perceptual model for how soil depth varies with slope in your study areas?

L.198. We agree by adding "sampled from a log-normal distribution and dedicated a more specific section to the soil thickness (L202-205)
L.202. The coefficients of 1.35 and 0.75 were from manual fitting of our available Slope - soil depth data. Upon reconsideration we perform a numerical fit to compute the coefficients. This results in values of 1.47 and 0.5 respectively. We recomputed our results, adjusted the text accordingly (L348-351) and added a plot in the appendix.

Hydrology
(...)
1) L239: TOPOG should not be cited as Montgomery and Dietrich (1994), because they didn't develop it, but should instead cite O'loughlin (1986).

2) L241: Pack et al. (1998) is the incorrect citation for SHALSTAB, this should be Montgomery and Dietrich (1994).
3) I agree though that Pack et al. (1998) should be cited here because the identical model is used in SHALSTAB and SINMAP and the latter is Pack et al.'s model.
4) L478: 'application of the TOPOG/TOPMODEL approach'. I suggest you cut reference to TOPMODEL here because you aren't using a TOPMODEL approach.

1. Thank you, changed it (L248)
2. changed it (L250)
3. We added SINMAP reference (L250-251)
4. Changed it (L491), thank you

(...) L255: "Based on the literature data discussed in the introduction": I think the literature you refer to needs citing again here, ideally with a sentence that explains the basis for your claim that the time to equilibrium is 1 hour. Li et al. (2013) aren't much help here because they are talking about the time to equilibrium for vertical infiltration only, there is no lateral flow in their model as I understand it. Montgomery et al. (2002, 2004) and Iverson (2000) are counter examples because while they disagree on many things, they agree that the equilibrium time even in the conductive Coos Bay soil should be much longer than 1 hour. As I said above, I don't think that makes the model inappropriate because (lateral flow sets the antecedent conditions for the triggering burst). But it does mean that R/T should be treated as an index for the propensity of landsliding rather than a quantity that can be directly compared to observed rainfall.If you disagree, and instead think hillslope lateral response times in your study areas are fast enough to allow direct connection to observed rainfall then a sentence or two recognising the debate around these response times would probably be sufficient to make readers aware that you differ with others on this point.

Upon consideration, we agree with the reviewer. We let go of the 1-hour assumption and indeed assume the P/T ratio as a measure for the propensity of shallow landslide activity (L498-499). In line with other reviewer comments, we added a table (Table 5) of rainfall depth corresponding to different return periods, to analyze both the conditions of a short/high intensity and long/lower intensity event. The full range of corresponding rainfall intensities is used in the qualitative sensitivity analysis.

Queries on equations:
(...) see my earlier comment about a potential problem with Equation 9. If you took my alternative approach then the dimensional mismatch would disappear because Eqn 8 would be integrated over depth while Eqn 9 would be calculated directly from the depth decay function (whether gamma or exponential).

This is in line with previous comments on the equation. We thank the reviewer for the critical look but propose the keep the equation as is.

3) (...) Adjusting the units in Eqn 14 fixes the problem there but leads to ambiguity earlier in the paper where you define is one outstanding problem here in that you define DBH earlier in the paper (L214) then use it as a term in equation 8 is DBH also being measured in cm here or is a conversion required? I think it would be better to adjust Eqn 14 so that DBH is expressed in metres there in order to avoid confusion elsewhere.

Thank you for the suggestion. We updated equation 14 and units throughout the paper making DBH consistently in meters.

Literature

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