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 model itself is similar to a number of existing models but also makes some important changes. It would be really useful to make these similarities and differences more explicit. The striking similarities to me were: 1) the hydrological model (Eqns 11-12) is exactly that of SHALSTAB (Montgomery and Dietrich, 1994) and SINMAP (Pack et al., 1998); 2) modelling discrete landslides of defined dimensions with lateral resistance due to roots only (Eqns 1-6) follows Montgomery et al. (2000), Schmidt et al. (2001) and Roering et al. (2003); 3) the probabilistic treatment of stability using distributions for parameters follows Pack et al. (1998) who represented c, phi and the R/T ratio as uniform distributions; 4) introducing a slope dependence to failure depth follows Prancevic et al. (2020), though with a different functional form. The similarities are strongest between SfM and Montgomery et al. (1998), they use very similar stability models (both infinite slope with root cohesion only on the margins), the same hydrological model, and both impose discrete landslide dimensions; so differentiating your work from theirs will be important.
SlideforMap is similar in many aspects of the approaches. We added the specific similarities and distinctions to our method section in the revised paper L155, 190-194, 239-245 . A subsequent discussion was added in the discussion section 5.6.

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.

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.

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."

Having read the paper I have one primary outstanding question: What do you gain as a result of the additional data collection and modelling efforts involved in a detailed inclusion of the influence of vegetation?

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.
We emphasized better as a goal in the Introduction L124-128 and a discussion on the outcome in the discussion section 5.4.
This additional text is useful.

Your paper focuses on predictive skill (using ROC AUC) and predicted instability (using an unstable area ratio). That focus enables a straightforward assessment of improvement in predictive skill from this more complex model relative to a simpler models such as SHALSTAB or SINMAP. In fact, I think you already have an answer to this in Table 7. The 'no vegetation' case in SfM is very close to the SINMAP model: in this case, there is no lateral resistance (i.e. an infinite slope), probability of failure is calculated from pdfs of friction, cohesion and depth with pore pressure predicted using the SINMAP/SHALSTAB model. The uniform vegetation cases (Global and Forest area) are very close to the SHALSTAB implementation of Montgomery et al. (2000): in these cases landslides have predefined dimensions and lateral cohesion is spatially uniform. The difference is that landslide dimensions (area and depth), and material properties (c and phi) are sampled from distributions to generate a probability of failure rather than using the critical P/T as a metric for propensity to failure (as in SHALSTAB). In all these cases I would expect a direct comparison to SINMAP and the SHALSTAB of Montgomery et al. (2000) to yield almost exactly the same AUCs as those from SfM. The clear structural difference between SfM and previous models comes in the case of 'Single tree detection'.
We added the similarities and distinctions between our model scenarios/metrics and the existing models to our methodology.
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.

Reading Table 7 in the context of these connections to simpler early models leads to three conclusions:
- *Landslide predictions are surprisingly (and encouragingly) skilful even when models as simple as the 'No vegetation' SfM (equivalent to SINMAP) are used. Models like SINMAP are very attractive if they perform so well given their simple structure and parsimonious parameterisation.*
- *Representing landslides as discrete features (as in SfM or Montgomery et al. (2000)) rarely improves predictive skill unless detailed vegetation information is available. Best AUC for SfM with 'Global' or 'Forest area vegetation' are equal to the 'No vegetation' case for 2 of the 3 study sites and only 1% better for Sta.*
- *Detailed vegetation information from single tree detection does subtly improve predictive skill but only in 2 of the 3 sites (slightly worse for Eriz) and only by 3.8 and 3.2% in AUC for Trub and Sta respectively.*

One interpretation of this would be that while SfM is much more satisfying from a process representation point of view it offers only very marginal gains in predictive skill and has considerable cost in that it is more highly parameterised and more complex. An alternative interpretation would be that small skill improvements on an already excellent model are worth the additional complexity (and cost). Reframing the percentage changes in AUC as percentage of the unrealised AUC that has been eroded by the new model (thus changing in denominator from AUC_{pre} to $1\text{-}AUC_{pre}$) the same values are: 6% and 43% for Trub and Sta respectively. I think this interpretation, which recognises the diminishing returns in model improvement is reasonable and if so it suggests the improvement is non-trivial.
It is interesting that the unstable ratio metric is more sensitive to model structure than AUC, and perhaps encouraging that this ratio is reduced by improved process representation. However as you point out (L355), this ratio is a measure of instability rather than accuracy.
The authors agree to a large degree with this interpretation and would like to thank CC for this interesting and concise discussion. We added this to our discussion section 5.4 on the vegetation scenarios and the model in general.
I don't see where the first two bullets above have been incorporated into Section 5.4, though I do see where the third bullet has been included and where some of the interpretation might have influenced your revised text. I still think those first two points are worth making (even if the 'no veg' case is not directly equivalent to SINMAP) because they capture the move from infinite to finite slope stability modelling, which has been a subject of debate

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.

SfM also makes predictions about the size of landslides. We decided, as suggested by CC, to not include this (though interesting) analysis in this manuscript and save it for a later date.
This is fine by me.

Beyond these three major points I have several other questions that are more specific but less important. I don't expect any of them to alter the primary messages of either the paper or the points I raise above but I hope they might be useful for the authors during revision. I do not understand the rationale behind some of the assumptions in SfM's boundary resistance representation

• *Neglecting lateral earth pressure. It is true that active and passive earth pressure are maximised at some strain but neglecting them on this basis leaves two problems: a) you still need a treatment for the forces acting at the head and toe of the landslide; b) you need to apply the same criteria to root reinforcement since this is also maximised at some strain.*

In the revised version we specified the phase in the landslide we assume and better explained the resulting force balance in the methodology L152.
This new text is much clearer. 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.

• *Neglecting soil cohesion on the sides. It seems inconsistent to apply root reinforcement but not soil cohesion on the lateral boundaries if you apply both on the base*

Like the answer above, we explained this better in the methodology.
This is much clearer in the new text. 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.

• *Lateral root reinforcement acts only over the upslope half of the landslide's perimeter (Eqn 3). I don't see a justification for this and Schwarz et al., 2010 point out that it underestimates lateral reinforcement.*

Like the answer above, we explained this better in the methodology.
This is a little clearer in that it explicitly explains what you do but you still don't explain why you do it. As above, 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.

• *Lateral root reinforcement in Eqn. 9 is depth independent. This seems inconsistent with observed depth dependent rooting (density and size); and the depth dependence of basal reinforcement in SfM (Eqn 10).*

You are right. 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.

• *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?*
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).

Variability
The form amplitude and spatial pattern of variability in material properties are all likely important in defining landslide location and size (e.g. Bellugi et al., 2021). Representing this variability seems important. I would have liked to see more detail on your rationale for your choice of distribution form and spatial (de)correlation. I recognise that observations to inform this are sparse and these properties are not well known. The normal distribution has some specific problems that you grapple with but that others chose to avoid by using a log-normal (e.g. Griffiths et al., 2007).
We agreed that the log-normal distribution is a more appropriate choice. We applied this in the paper, section 2.4, in a recomputation of the results. In addition, we added a comparative figure in the appendix.
Addressed. 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?

Soil depth variability is treated slightly differently (spatially de-correlated but slope dependent). I was unsure whether soil depths distribution was parameterised from observed landslide scar depths (L178) or using mean and standard deviation as parameters to optimise (Figure 7). The former seems problematic: landslides likely occur in deeper soils biasing the sample. Perhaps Eqn 7 was designed to account for this? However, I don't understand why the coefficients on mu (1.35) and sigma1 (0.75) in Eqn 7 have these particular values. The second approach, tuning mean depth rather than setting it from observations seems more appealing to me and would also enable a comparison between model results and observed landslide depths, which would be a nice addition.
We adjusted, pointing out that this tuning is optional L203-204.

This is now much clearer. 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?

Hydrology
Your approach is exactly the same as that of SHALSTAB and SINMAP but is considerably different from Topmodel (Beven and Kirkby, 1979). All three use a topographic index to define hydrologically similar units. Topmodel uses these (with simple treatments for evaporation and infiltration) to simulate a time-varying catchment averaged response to a rainfall timeseries that can be mapped back onto the HSUs; the others simply solve for a single steady recharge rate (neglecting these processes). Even the topographic index (i.e. A/sin(B)) differs from that of Topmodel (which uses ln(A/tan(B))). This reflects differences in reference frame (the sin vs tan) and assumed conductivity profile (uniform vs exponential). I don't disagree with the approach but I think it follows Montgomery and Dietrich (1994) and Pack et al. (1998) so it would be simpler to say that. If you wanted to give credit to earlier work then the TOPOG model of O'loughlin (1986) was behind the original derivation of SHALSTAB and the first introduction of a topographic index was by Kirkby (1975).
We let go of the formulation of using TOPmodel or TOPmodel assumptions and gave explicit credit to O'loughlin (1986) and Kirkby (1975) in the methodology L239-240.
Addressed, my only minor comments would be:

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.**

Previous papers that apply this hydrological model do not claim that it is particularly well suited to slopes with macropore flow. Montgomery et al. (2002) highlight the importance of macropores and fractures (and a steep soil water characteristic curve) for hillslope hydrologic response but also recognise that "that rapid pore pressure response that controls slope instability […] is driven by vertical flow, not lateral flow" (Montgomery et al., 2004). ***There is general agreement that lateral flow (modelled here) strongly influences the pore pressure field antecedent to a burst of rain that could initiate a landslide*** *(Iverson, 2000; Montgomery et al., 2002; 2004). This has important implications for the approach though because it implies that Q/T is an index for the 'propensity for landsliding' rather than a parameter to be calibrated within a complete hydrological treatment. This explains the apparent problem of predicted pore pressures independent of rainfall duration but observations that landslide triggering depends on both intensity and duration. Broad spatial patterns of pore pressure and instability should be well captured but triggering rainfall properties may not be. In fact, discussion of the influence of macropores on pore pressure tends to focus on the unpredictable localised pressure peaks associated with constrictions or terminations to macropores (e.g. Pierson, 1983; Montgomery et al. 2002). Even given these limitations I don't think this is a bad model relative to the alternatives because it captures broad phreatic surface patterns and I'm convinced that the finer detail of these patterns is set by (unknown and perhaps unknowable) heterogeneity in material properties (e.g. macropores). If so, a more refined and expensive hydrological model may improve predictions of spatial pore pressure patterns very little.*
We updated the introduction L84-86 and methodology section 2.6 stating our assumptions, limitations and similarities to our model more explicitly.
This is partly addressed in that the assumptions and limitations are now clearer and the connection to macro-pore flow has been clarified. I have two outstanding comments though:
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.

Sensitivity Analysis
As you point out parameter interaction makes it very difficult to infer parameter sensitivity from Figure 7 I think that may make it difficult to support some of your assertions in L388-395 because you cannot guarantee that interactions are not masking other stronger sensitivities. For me the clearest example is the interaction between P and T (Table 6). Both are listed as uncertain parameters within the sensitivity analysis but only feature in pore pressure definition and only in that equation as the P/T ratio. As a result their inclusion as two separate variables in this analysis is likely to lead to severe equifinality (with high or low values will result in the same outcome as long as P/T is constant). Why not include the ratio of the two in your sensitivity analysis?
We added P/T ratio to our sensitivity analysis results and discussed the equifinality L546-547 that appears to be at play.
Addressed

Queries on equations:
1) I think there is a dimensional problem in either the first term of Eqn 3 or the second term of Eqn 4. Eqn 10 expresses R_{bas} as a function of R_{lat} so I think both should be either a force per unit length or a stress. If R_{lat} (in Eqn 9) is a stress then Eqn3 is dimensionally incorrect because the first term is a force per unit length and the second a force. The first term needs integrating over landslide depth. This could take the form cos(s) H if you assume reinforcement is depth invariant. However, this would then be inconsistent with Eqn10, which assumes that root reinforcement declines with depth. On the other hand, if R_{lat} is a force per unit length (which might be more consistent with Moos et al (2016), Fig 3) then the problem may be more difficult to solve because the lateral depth integrated stress (N/m) is being applied across a basal area (m_2).
Indeed, it uses a dimension correction factor, we added this to Equation 9.
Addressed, though 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).
2) Are h and H measured in a vertical reference frame as indicated in Figure 2? If so then I think there is a cos(s) missing from Eqn 12. The first cos(s) converts vertical depth to slope normal thickness, the second converts phreatic surface thickness to pressure head (under assumptions of: uniform steady slope parallel seepage).
You are right, we corrected this and recalculated. Page 15 of 15
Addressed
3) Eqn 15 is incorrect because the original equation calculates DBH in cm from tree height in metres (Dorren, 2017) but you use DBH in metres (L292). I think Eqn 15 should be adjusted to $0.01H_{1.25}$.
You are right, we corrected this.

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.

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