Natural Hazards
and Earth System
Sciences

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Discussions

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Interactive comment on "**Physically based approaches incorporating evaporation for early warning predictions of rainfall-induced landslides**" *by* **Alfred Reder et al.**

Anonymous Referee #1

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General comments

I read with interest this research paper investigating the effect of evapotranspiration on physically based models for rainfall-induced landslides. The topic is scientifically significant for the landslide hazard mitigation. I think this paper can be an interesting contribution and is worth to be published but need some major reworking before publication.

First, the introduction is not detailed enough: it lacks of significant contributions in the context of: (1) hillslope hydrology and slope stability and (2) parameters transfer from physical models to real world.

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[Discussion paper]

Second, methodology and results should be discussed in more detail specifying some possible limits of the assumptions made. This will lead to more convincing conclusions.

Third, some figures need to be modified, some merged, and some are redundant.

Overall, the paper merges very important aspects of the hillslope hydrology and stability coupling measurements, physical model, and modeling approaches. For this reason I believe it will be suitable for publication and I hope the comments will help the authors to improve the quality and the impact of their manuscript.

Details

To my opinion specific improvements need to cover the following topic:

a) Literature review is limited. In page 2 (line 15 to 20) the authors list a group of physically based hydrological models that neglect evapotranspiration effect. Montgomery and Dietrich, 1994 present a model that uses steady-state hydrology (not suitable for early warning). Moreover, they specify that they use Peff i.e. net rainfall (precipitation less evaporation). Baum et al., 1998 is not the last version of the model and was modified by the Baum et al., 2008 version. It is an event based hydromechanical model, it is not suitable for long term simulation (the report available to: https://pubs.usgs.gov/of/2008/1159/downloads/pdf/OF08-1159.pdf states "TRIGRS is not suitable for modeling long-term effects of alternating periods of rainfall and evapotranspiration, and choosing the correct initial conditions for a given storm is critical to obtaining accurate results"). Formetta et al., 2014 was not correctly cited. It takes into account of evapotranspiration by using the GEOtop model which solves the coupled heat and water balance equations (see Endrizzi et al., 2014). Finally, in the review there is a lack of hydrological models accounting for evapotranspiration (some of them in a simplified way and some of them in a more rigorous way), e.g. Casadei et al. (2003), Šimůnek et al., (2006), Rosso et al., (2006), Ebel et al., (2010), Arnone et al., (2011). I think this is more fair stating both the aspect in the introduction, i. e.: 1) some applications (and models) neglect evapotranspiration because it is considered not the

most relevant process in the analyzed conditions (e.g. Baum et al., 2008; Pagano et al., 2010; Formetta et al., 2016); 2) some applications consider the effect of evapotranspiration with different degree of simplification (Casadei et al. (2003), Rosso et al., (2006), Šimůnek et al., (2006), Ebel et al., (2010), Formetta et al., 2014; Capparelli and Versace (2011); Arnone et al., (2011)) Moreover literature needs to give: i) examples of paper that adopted the same technique of estimating hydrological model parameters in a physical model and use them in real world applications; ii) examples of papers that performed a similar analysis (i.e. evaluation of the effect of evaporation on hillslope hydrology and stability) in other locations or in the same area, stating what make peculiar the current paper (and findings) compared to them.

b) The methodology section should give more emphasis to the novelty presented in this paper. Subsection 2.1 and 2.2 are long description of Rianna et al., 2014a,b; Pagano et al., 2010. It is not clear if the authors are adding something new to that papers: if yes they should point it out more explicitly to facilitate the reader; if not, although it is clear that the background provided by subsections 2.1 and 2.2 is important, authors should consider to summarize them in the main text and detail them in appendix. Same considerations apply to figures 2 to 7: are they showing new data-results compared to Rianna et al., 2014a,b; Pagano et al., 2010?

c) Authors should include in their Discussion and Conclusion considerations concerning the hypothesis used in the paper: i) considering an homogeneous soil whereas many other studies in the area deals with stratified soils; ii) effect of the hysteresis which is evident in the physical model data (Fig. 9-a); iii) transfer in a real world application the same parameters estimated in the physical model (e.g. is there any limit in using the same hydraulic conductivity, how about preferential flow?); iv) the assessment of hillslope stability by a threshold approach neglecting the soil mechanic parameters such as cohesion and friction angle; v) the assumption of one dimensional flow: is the early warning threshold (estimating neglecting the lateral flow influence) valid for the entire hillslope? Is there any changes in flow behavior at the toe of the hillslope or

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in the less steep locations, where lateral flow could be important?

d) The authors should acknowledge explicitly that the analysis presented for the real case application does not use any measured time series of soil suction or soil water content to validate the model.

Specific comments

1) Page 1 line 20: Could you please define "cover" when you use it the first time and use it consistently in the text.

2) Page 2 lines 15 to 20: please consider to update and extend the literature review here.

3) Page 2 line 27 could the Authors please explain which type of model they use.

4) Page 2 line 28: can the Authors please specify in which location those data are collected? Where the landslide happened or in the physical model?

5) Page 3 line 5: could the Authors specify which parameters or at least which type of model parameters they use?;

6) Page 3 line 5: are these procedures new in some theoretical aspect? if yes please specify the novelty, otherwise is better to say "applied" and to reference the procedure applied;

7) Page 3 and 4: please consider to summarize the sections 2.1 and 2.2.

8) Page 3 line 20: can you spell the hydrological variables? Are the data the same used in Pagano et al., 2010?

9) Page 5 section 2.3: Is the model been applied in other similar experiment? If yes, can you cite them?

10) Page 5 line 23: Could the authors please add the units to each variable they use?

11) Page 5 line 24: Could the authors please spell the name and type of the function

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12) Page 6 line 2: Could the authors please spell the name and type of the function

13) Page 8 line 6-15: could the authors specify if the procedure has been used for the first time in this paper or could you please reference it?

14) Page 8 line 16: could you please spell the remaining calibrated parameters and the calibration algorithm used? And could you please provide a table of the main parameter values?

15) Page 8 line 19: could the authors please provide a quantification of the agreement in calibration and verification period: for example providing a goodness of fit indices (such as Nash–Sutcliffe, King Gupta Efficiency, Root mean square error, etc); this applies also to soil temperature simulations.

16) Page 9 line 13: Could the authors please motivate the choice of the experimental set up: why 4.5 mm for 60 days? Are those typical value in the study area?

17) Figure 15-c shows that the models tend to behave differently starting from around 10000 KPa. How often the soils experiment those value? Looking at the Figure 9-a the soils had suction values between 1 and 100 KPa and correctly the authors extend the soil water retention curve up to 1000 KPa. However the latter is lower than the 10000 KPa where the models tends to differ (Figure 15-a). Can the Authors comment on this point?

18) Pag 10 line 13: Quantifying the model parameters. Does it mean: using the model parameters estimated thanks to the physical model measurements? Moreover, how the values of the optimal parameter set used in the simulation compares with at-site parameter values used in other studies? Is the order or magnitude the same?

19) The authors should specify the time step of each simulation (physical model and real case both for the input/output variables, and for the inner model time step). In the text (page 5) is it hourly whereas in the figures it seems daily (see captions). If this is true, how this contrasts with the early warning applications? Is there a need of a

sub-daily time step?

20) Please include the NEM model results in Figures 16 and 17 in order to have all the model results in the same figures.

21) Page 12 line 10: Please include in the discussion on the threshold values how it will be influenced by the fact that only one event is considered? How the threshold changes in case of multi-events?

22) Please include some of the limitations of the approaches in the conclusion section and discuss them (see General comments c and d)

23) Figure 10 could be a sub-figure of Figure 9.

24) The paper need to be proof-read possible by a native English speaker. Among them:

- Pag1 line 8: Promptness consider to replace with timeliness;

- Pag1 line 10: Evaporation fluxes consider to replace with evaporative fluxes

- Pag1 line 21: Founding part of their instability: consider to rephrase it

- Pag2 line 1: ranfalls consider to replace with rainfall

- Pag2 line 2-3: Analysys results to triggering cause: rephrase it.

- Pag2 line 17: neglect: remove it

- Pag2 line 19: consider to rephrase as: such an assumption can only be considered reasonable

- Pag2 line 25: arises whether consider to replace with arises as to whether

- Pag2 line 26: The study consider to replace with this study

- Pag3 line 3 and 7: The paper consider to replace with this paper

- Pag3 line 30: obtained consider to replace with used

- Pag4 line 9: between soil consider to replace with between the soil.

- Pag 5 lines 11-13: consider to rephrase it.

- Pag 5 line 25: remove the new paragraph

- Pag 5 line 27: taking into account the possibility of changes consider to replace with taking into account possible changes

- Pag 6 line 6: Remove the

- Pag 6 line 19: cut of: please consider to rephrase it.

- Page 7 line 5: It proves consistent with literature consider to replace with This is consistent with the literature

- Page 7 line 8: dry hot consider to replace with dry and hot

- Page 7 line 10: particularized into please consider to rephrase it

- Page 7 line 12: in the atmosphere temperature consider to replace with in the atmospheric temperature

- Page 9 line 13: remove maintained

- Page 9 line 17: with that water amount consider to replace with with the water amount

- Page 9 line 17: remove that

- Page 9 line 24-25: please rephrase it

- Page 10 line 19: by IEM consider to replace with by the IEM

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