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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The canopy interception–landslide initiation conundrum: insight from a tropical secondary forest in northern Thailand" *by* R. C. Sidle and A. D. Ziegler

Anonymous Referee #1

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This manuscript reports on rainfall, throughfall, and soil moisture measurements on a steep slope. The topic is of high relevance to HESS readers and the manuscript is clearly written. It has four important limitations: (1) poor data on throughfall intensity; (2) lack of relevance of soil moisture measurements for slope stability; and (3) overstating conclusions beyond data support:

1. The substantial uncertainty in throughfall measurements using the trough system necessitated large calibration correction of 50-78% (P7L14). The total corrected throughfall estimates are 10-14% less than rainfall, so calibration errors are 3-8 times larger than the difference being quantified. I admire the authors for confessing this limitation, but I don't understand why they find data from this instrument sufficient for

addressing their hypotheses. I also think the calibration procedure cannot be omitted from the paper given its paramount importance. There are some apparent absurdities in the data that may be explainable by the calibration procedure as well: e.g., it appears total TF>RF for the early portions of the most intense storms in the dataset (Fig 5a-d), followed by RF>TF later in the storm during low-intensity RF; I am not aware of this pattern ever being reported. It appears that conditioning the calibration on total TF:RF may have resulted in plausible estimates of total mass balance (where they can be compared to expectations), but measurements of intensity are both the least reliable and most important data. I disagree with the "clarity" claimed P17L22.

2. Hillslope hydrology is poorly constrained, so it is difficult to understand relevance of the soil moisture data to slope stability. There are two problems in the manuscript that arise because of this. (1) The instrumented slope was obviously not near failure during conditions represented in the dataset as evidenced by low soil moisture at depth, and the deep and highly weathered soils suggest this site is not prone to failure in general. It is unclear specifically how soil moisture responses in this slope is useful for understanding slope stability, but the lack of responsiveness at timescales relevant to canopy interception is not enough evidence to conclude a general lack of canopy interception effect. (2) The analysis of paired TF and soil water measurements implies a one-dimensional water balance is relevant for slope failure, but in fact hillslope- and watershed-scale hydrologic conditions are important. Depending on slope configuration, there may be little reason to expect substantial effect of local canopy interception on soil moisture at depth and thus slope stability. These conceptual problems can be addressed by modifying the discussion, but I think the conclusion linking interception to stability through soil moisture at this site (P17L26) is oversimplified.

3a. Intensity-duration quantification of slope stability is useful for general purposes, but limitations of the concept prevent literal application of thresholds. Obviously none of the thresholds were correct for the instrumented slope or it would have failed about 30 times in the 30 months of monitoring. So, each slope must have its own threshold, and

presumably some slopes have thresholds that pass between paired TF and RF intensities in triggering storms (Fig 7). How many slopes is that? The answer to that question is the true effect of canopy interception on slope stability, and the effect of canopy interception on stability of one instrumented slope cannot be reasonably extrapolated to encompass all slopes.

3b. The strong conclusion that there was no intensity smoothing (P15L30) is dubious and contradicted elsewhere in the manuscript (P16L27). In “large events” (Fig 7d) and in most events overall (Fig 7), storm-total TF intensity was lower intensity than RF, so in that sense there was smoothing. Later statements (P16L4-7) rightly focus on peak intensities, but are based on highly uncertain data. Blanket characterization of “no effect” is not credible.

Minor points:

P3L10-18 why present a review of root reinforcement literature when this work has nothing to do with root reinforcement?

P6L23 can you use these field data to convert soil moisture content (m3/m-3) to % saturation? The Results and Discussion refer to degree of saturation (e.g., P13L9, P13L18, P16L16) but no information is presented in the figures or text on porosity or soil moisture release curves and the reader cannot link volumetric soil moisture data to pore pressure.

There are some problems with the figures to clean up. Fig 6a: “axis title”; Fig 6c rainfall bars are not at the same interval as the time interval labels; panels in Fig 5-7 are often different sizes and not aligned.

P14L8 editing error muddies a critical statement about the TF-RF comparison.

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