Reviewer's 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

General comment

The manuscript deals with a topic falling within the scope of HESS, to which part of the readership will be interested in. The paper is well structured and clearly written, and the presented experimental data are innovative, as very few examples of similar measurements can be found in the literature. Apart of this merit, however, as the focus of the paper is about the possible effects of canopy interception on the triggering of shallow landslides caused by infiltration into the soil mantle up to a depth of 2 m, the analysis of the results in view of the infiltration processes is poor, lacking important information about soil properties, and more in-depth discussion of the soil moisture dynamics should be provided.

Therefore, my recommendation is that some major revisions are needed before this manuscript could be published in HESS. Some of the following detailed comments will hopefully clarify my point of view.

Detailed comments

Page 5, lines 13-14 (minor issue). Please clarify the meaning of "landslides (…) associated with road runoff". A clear definition of the possible triggering mechanisms of landslides in the area would indeed help to better focus the discussion of the measured soil moisture responses to precipitations.

Page 6, lines 6-10 (major issue). Providing more information about soil properties would allow a better understanding of the observed soil moisture changes. Soil porosity is not given, but in the following section 4.4 the authors state that when volumetric moisture content approaches 0.45 the soil is saturated. The provided bulk density data seem to indicate that, at least in the upper layer, the porosity should be greater (by the way, what is the moisture content corresponding to the provided values bulk density?). As the following discussion points out that the triggering of landslide is expected to occur at depths >1.0m, would it be possible to get some information about soil properties (at least porosity and k_{sat}) at depths larger than 25cm? (indeed, the authors say that the upper 20cm are characterized by a soil horizon different from the deeper one).

Page 6, lines 15-16 (typo). I think it should read "(station~~s~~ 429, figure~~s~~ 1b)".

Page 6, line 17 (major issue). The definition of an event should be motivated in view of the expected triggering mechanism. Why the thresholds of 8mm and 4hours have been chosen?

Page 7, lines 6-8 (minor issue). The "dynamic calibration correction" is not clear. Please provide some description of the applied correction.

Page 7, lines 25-30 (moderate issue: I don't know if this issue is minor or major). It is clear that using a large throughfall collector allows the integration over a relatively large area of an inherently inhomogeneous process (in space). However, in the following discussion, in some cases the authors point out that, owing to differences in canopy structure and to the effects of wind (and possibly also to the effects of rainfall intensity, I would add), the dripping of throughfall from canopy could follow different paths, leading to local concentration of drops. How did the authors conclude that the shape, size and position of their collector are adequate? What do the authors think about using several randomly distributed ordinary rain gages? In such a case it could be possible to get information about the adequacy of the obtained spatial mean by

subtracting one (or more) gages and then check if the obtained (spatially averaged) throughfall is affected or not.

Page 9, line 28 – page 10, line 1 (minor issue). The outliers could be an artifact due to concentration of throughfalling drops in the collector, caused by the shape and position of the adopted collector.

Page 11, lines 16-25, and figure 5 (moderate issue). Looking at the provided hyetographs, it seems simply that, regardless of the timing of a peak within the event, when the intensity is below 1.0-1.1 mm/min, it results RF>TF, while it is the other way around when the intensity is larger.

Section 4.4, as a whole (major issue). The whole discussion is too simplistic, and some deeper interpretation should be made. I just give some possible keys. In a soil with k_{sat}<5mm/h at the depth of 25cm (and maybe further reducing with depth), it is easily expectable that it may take many hours before water reaches 2.0m depth (even if we don't know soil properties at depth larger than 25cm), so I strongly suggest to extend the time interval over which the soil moisture changes are visualized and discussed (this issue has to do also with the previously raised issue about the adopted definition of a rainfall event). The interpretation of the (clearly visible) effect of initial soil moisture on the effectiveness of a rain event on the following soil moisture changes should be linked to the degree of saturation (but we don't know soil porosity) of the soil and to its hydraulic conductivity (once saturated, the upper layer cannot retain more water, and so, if the hydraulic conductivity allows it, it is "obliged" to release the excess water to the underlying soil). In other words, there should be a maximum storable soil moisture increase, depending on initial moisture condition, over which the excess water penetrates deeper or runs off laterally (above or below surface, or both).

Page 13, lines 5-6 (major issue). It seems to me that limiting the observation of soil moisture to the (widely variable) duration of rainfall events in many cases may be the reason why a (later) deep soil moisture change was not detected.

Page 15, line 22 (typo). It should probably be "environmental conditions ~~change~~ during the storm".

Page 16, lines 16-21 (major issue). See my previous comment about section 4.4. As RF and TF are quite similar in the considered forest, this paragraph would mislead the reader to the conclusion that soil moisture at 2.0m would not be affected by any rainfall event.

Page 16, lines 30-31 (major issue). It is clear that for the considered rain events canopy interception has negligible effects. But, as I already commented above, the rain events have been defined arbitrarily >8,0mm, and there is (maybe obvious) evidence that canopy interception could be larger for smaller events. Could these neglected smaller events affect the initial moisture state of the soil at the beginning of the considered larger events? And, if so, can the authors exclude that canopy interception may play a role in the establishment of such initial moisture state? I would like to read some discussion about this point, before concluding that canopy interception has no effect on landslide initiation.

Page 17, section 6 as a whole (major issue). In view of the previously raised issues, some of the conclusion drawn could be different.

Figure 1, caption (minor issue). It does not seem that the topography and the major stream channels are actually shown in Figure 1a.

Figure 6a (typo). The title of x-axis is missing.