Anonymous Referee #1 (Received and published: 18 June 2019)

We thank the reviewer for his or her time and useful comments, which we have attempted to incorporate in manuscript revisions. We have also attempted to clarify and further justify the impact and utility of this work in response to specific reviewer concerns. Below are explanations of our responses to the reviewer's comments (R1-C: Reviewer comment; AR: Author response).

Major Comments

R1-C1: Remark: I have been a reviewer of a previous version of this manuscript submitted to Water Resources Research. By chance, the manuscript arrived a second time in my hands, now in HESS. The authors state that the new version is sufficiently different, so this is not a re-submission, but a new manuscript. I find substantial changes reflect a revision, which was expected. I would have appreciated, if the authors would have taken the time to phrase a point by point response, which would have allowed for a much more efficient review round. Please respect the time of the reviewer. Some of my concerns have been addressed, but others not. This review is a mixture of both my previous and new comments.

> **AR1:** We thank the reviewer for the thorough response and appreciate the effort taken in reviewing the manuscript a second time. While we understand that a point-by-point response would have been useful for guiding this second review, we of course had no idea to whom the manuscript would be sent. Thus, submitting a response to reviewers would have been inappropriate in the context of this new submission. For clarity, the WRR decision was to "revise and resubmit", which was our initial intent. However, our assessment of the reviewers' comments led us to believe that the revisions requested, specifically the addition of throughfall and denser soil moisture measurements, were untenable. As such, and because we believed that the findings were defensible without those additional measurements, we sought an alternative venue for the revised manuscript (i.e., this submission to HESS).

R1-C2: This manuscript proposes that the interception storage can be derived from high temporal resolution top soil moisture measurements. The term "interception storage" here defined broadly as the storage of a surface layer contributing to direct evaporation, and encompasses besides the canopy storage also ground cover, litter and the top soil itself. The proposed method analyses the increase of volumetric soil water content in response to rainfall events. This is done by calculating the interception capacity using the Gash Model with an important alteration. Instead of using the event rainfall depth required to cause canopy drip, the authors use the event rainfall depth required to cause a soil moisture response. Separation between aboveground and soil hydraulic processes is achieved by using simulations with an unsaturated zone model (HYDRUS) to empirically estimate the speed of the propagation of the wetting front as a function of initial soil water content and for typical soil properties in Florida. As a proof of concept the authors apply this method on 33 plots (nested design: 5 sites each with 6 subplots, plus 1 site with three subplots) analyzing soil moisture responses to rainfall events during three years. Direct measurements of canopy, litter interception or soil properties are not available for comparison. They find that their derived interception storage is comparatively high, but plausible. Using multivariate statistics they show that their derived interception storage depends considerably on plot leaf area index, ground cover and antecedent soil moisture. They conclude that their proposed method of deriving "whole forest" interception storage has potential and suggest it as an alternative to other empirical assessments. In a last step, the interception storage is applied to calculate plot interception and the variation between the plots is discussed.

I was very intrigued by the presented idea and also by the dataset, which has a great deal of potential. The paper itself is mostly well written and discusses the case well. The presented data and analysis are of interest for the readers of HESS.

> **AR2:** Thank you for the accurate summary and positive words regarding the potential applicability of this work.

R1-C3: Nevertheless I have some major concerns with the methods and conclusions in this manuscript. My main concern is that the authors claim is too strong, given the substantial uncertainty in the analysis as well as limited data availability:

> **AR3:** We acknowledge the reviewer's specific concerns and answer each point below in detail. We have also worked to generally temper the strength of the conclusions drawn in manuscript revisions.

R1-C4: No direct data of canopy or litter interception are available, and those would be necessary to validate the method for good

> **AR4:** We fully acknowledge that this is the case. As noted in the manuscript, these data come from a multi-year study quantifying forest water use under varying silvicultural management, which was measured using diurnal variation in total soil moisture. The analyses we present here were thus performed on a data set that was not directly intended to measure interception. As such, we did not collect any additional empirical interception measurements, nor can we do so retrospectively.
>
> We acknowledge that a lack of "reference" interception measurements is not ideal from a methodological point of view, particularly if our intent was to exactingly quantify the canopy interception of specific sites. However, we believe that these results are useful for illustrating the utility of soil moisture-based interception estimates and is surprisingly well validated against measurements from previous interception studies in southeastern US and other pine stands. Indeed, Reviewer 2 notes that "[p]erhaps a full-fledged throughfall monitoring campaign is not necessary in this case," given the availability of "…throughfall and interception field studies…for similar pine stands." We argue that this is particularly true given the relatively long-term dataset from which our estimates were derived and their broad numerical and theoretical agreement with both total interception storage capacity and total annual interception losses relative to rainfall estimated in previous studies of similar systems. The reviewer's concern that these results were not directly validated using contemporaneous and co-located data is well taken, however, and we have modified the text to better contextualize the limitations of our comparisons with other studies and stress the potential for this novel method, rather than asserting its quantitative robustness.

Examples of such revisions include:

"Notably, the analyses we present here were performed on a data set that was not initially intended to measure interception, but rather to quantify forest water use and yield. As such, we did not collect independent empirical interception measurements (e.g., throughfall and stemflow). Rather than aiming to meticulously quantify the total interception of specific sites, our goal was thus to illustrate the potential utility of soil moisture-based interception estimates. As such, we indirectly validated our results using previous interception studies in southeastern US and other pine stands and by assessing the expected associations between estimated interception and co-located measurements of stand structure (e.g., LAI and groundcover)."

"While general agreement with previous studies supports the feasibility of using a soil moisture-based approach for estimating interception, we reiterate that a more robust validation of the method using co-located and contemporaneous measurement using standard techniques is warranted. Below we summarize the assumptions and methodological considerations that affect the potential utility and limitation of the method."

R1-C5: The method assumes only vertical matrix flux takes place between soil surface and measurement depth (the example is 15 cm soil depth), this reduces the applicability of the method to only suitable sites, without lateral flow and without preferential flow. The error is difficult to assess. Similarly, the method assumes that soil properties are comparable between sites and soil moisture measurement points, since the damping of the infiltration front signal should only depend on the differences in interception, not on small scale variation in hydraulic properties.

AR5: We acknowledge that the method assumes only vertical flux through a homogenous soil matrix, with the limitations noted by the reviewer. Regarding lateral flow, we acknowledge that it could delay the wetting front arrival, leading to an overestimation of interception using this method. However, we contend that the shallow placement of the soil moisture sensor would limit this effect to settings where strong vertical layering that leads to lateral flow (i.e., at capillary barriers or differential conductivity layers; Blume et al. 2009) exists very near the surface. Such effects of vertical soil heterogeneity would be further minimized by placing the soil moisture sensor closer to the soil surface (e.g., @ 5 cm depth); we now make this specific recommendation. On the other hand, Blume et al. (2008) observed lateral flow within the duff layer (i.e., partially decomposed organic material between the A-horizon and fresh plant litter) during high-intensity precipitation events (Blume et al. 2008). This phenomenon could occur across a broader array of settings. These considerations are now mentioned in the methods and discussion sections:

"This approach assumes no runoff or lateral soil-water flow near the top of the soil profile from time t to T. Except for very fine soils under extremely high $\overline{R}$*, this assumption generally holds during early storm phases, before ponding occurs (Mein and Larsen,*

1973). In settings where strong layering very near the surface may lead to lateral flow above the sensor (i.e., at capillary barriers or differential conductivity layers; Blume et al. 2009), the wetting front simulations described above would need to account for layered soil structure to avoid potential overestimation of interception. Further, placing the soil moisture sensor as close to the soil surface as feasible (e.g., within 5 cm) would help to minimize potential influences of soil vertical heterogeneity. Lateral flow within the duff layer during high-intensity precipitation events as observed by Blume et al. (2008) would be more difficult to correct for; however since our goal is to determine β_s, extreme storms can be omitted from the analysis when implementing Eqs. 1-10, without compromising our estimates. Similarly, the presence of preferential flow (e.g., finger flow, funnel flow, or macropore flow), if not accounted for in wetting front calculations, could lead to underestimation of interception."

"*There are several important methodological considerations and assumptions inherent to estimating interception using near-surface soil moisture data. First is the depth at which soil moisture is measured. Ideally, θ would be measured a few centimeters into the soil profile, eliminating the need to account for infiltration when calculating P_G in Eqs. (4-6) and thereby alleviating concerns about lateral and preferential flow. Soil moisture data used here were leveraged from a study of forest water yield, with sensor deployment depths selected to efficiently integrate soil moisture patterns through the vadose zone. The extra step of modeling infiltration thus likely increases uncertainty in β_s given field-scale heterogeneity in soil properties and lateral and preferential flow. Specifically, lateral flow would delay arrival time, leading to overestimation of interception, while preferential flow would do the opposite. In either case, accounting for these processes in wetting front calculations would reduce these errors. Despite these caveats, infiltration in our system was extremely well-described using wetting front simulations of arrival time based on initial soil moisture and rainfall. As such, while we advocate for shallower sensor installation and direct comparison to standard methods in future efforts, the results presented here given the available sensor depth seem tenable for this and other similar data sets."*

Regarding preferential flow (PF), we acknowledge the potential for multiple PF types (e.g., finger flow, funnel flow, and macropore flow) to reduce the time from infiltration to soil moisture response, leading to a potential underestimation of interception. While many authors have highlighted the importance of preferential flow in driving the timing and magnitude of water and pollutant fluxes (e.g., Orozco-López et al. 2018), the characterization, analysis, and simulation of PF remains a fundamental challenge in the hydrological sciences (Jarvis et al. 2012). Orozco-López et al. (2018) synthesize some of the newer laboratory and field-scale attempts (e.g., Jarvis et al. 2016) to address the complex PF challenge, but they note that most current soil-water modeling approaches do not include this process. Given the goal and scope of this work, we have thus modified the methods and discussion as described above to acknowledge this limitation and place the potential errors from neglecting this process in context.

R1-C6: Compared to the last version of the manuscript, the new version addresses the problem of antecedent soil water content and its influence on the propagation of the wetting front by use of a soil hydrological model. I am however still skeptical that the rather idealistic model accounts for confounding soil processes sufficiently. Especially preferential flow occurring specifically in forest sites would strongly affect the wetting front arrival times.

> **AR6:** Please see response to R1-C5. We have added several caveats to the discussion to highlight potential differences between an idealized soil profile simulation and a "real-world" forested site.

R1-C7: Research indicates that the correct assessment of interception in the presence of spatial heterogeneity of net precipitation requires a substantial number of sampling locations (i.e. 10 to 100 depending on the forest structure, see Zimmermann et al. 2010, WRR, W01503). Additional spatial variation is introduced by stemflow, which also varies between individuals. Also, soil hydraulic properties vary substantially at very small scales in forests. All this suggests that three sensors are not sufficient to capture the spatial heterogeneity. A larger number of sensors would at the same time imply much more installation effort, which contradicts the claim that this is a comparatively simple method.

> **AR7:** The sampling effort required to characterize interception variability using existing methods has been characterized as ranging from "extreme" (200 funnel-type collectors per hectare for event-based sampling) to "moderate" (25 funnel- or 5 trough-type collectors per hectare for longer-term studies) to maintain mean relative error to 10% (Zimmerman and Zimmerman 2014). We note that this more recent publication updates the recommendation in Zimmerman et al. (2010), which suggested that 1300 funnels or "…150, 100, 75, and 30 troughs of 1, 2, 4, and 10 m length" would be required to meet the same standard. While troughs and soil moisture sensors are not directly comparable in their spatial configuration or methodological approach, given the 5-trough/ha recommendation by Zimmerman et al. (2014), we argue that it is reasonable to at least evaluate the stability of the interception estimates derived from our study using three sensors and assess their agreement with previously measured values.
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> Specifically, our method yielded interception values that were stable and predictable with only a small number of measurements, indicating that while surface inputs of water may be strongly heterogeneous, the subsurface smooths out some of that variation. In a sense, the soil moisture sensors are in this way acting like troughs, which are intended to sample a larger surface area than funnels, thus capturing more throughfall heterogeneity (i.e., smoothing the surface inputs due to spatial variability in precipitation and canopy structure). Support for the potential of our approach comes both from the fact that our estimates of total interception storage capacity and total annual interception agreed with previous studies and that there were strong and logical associations between forest structure (LAI) and estimated values.
>
> We agree that increasing the number of soil moisture sensors would better characterize spatial heterogeneity, just as adding more trough- or funnel-type collectors would, but we do not think this undercuts the utility of our findings or limits the applicability of the method. Regarding effort, both trough-type collectors and soil moisture sensors can be set up to log automatically, so their installation and data collection efforts are likely

comparable. However, trough-type collectors must be consistently maintained to prevent build-up of litterfall, whereas soil moisture sensors require little to no maintenance besides visiting the site to download data. With newer modem-enabled loggers and soil powered sensors, it would be possible to implement long-term interception measurement campaigns with much reduced effort. We have added a new paragraph to the discussion to contextualize the number of measurements presented here relative to guidance for standard methods:

"Among the many challenges of measuring interception is the spatial heterogeneity of canopy and ground cover, with associated heterogeneity in interception rates. Consequently, researchers have suggested that 25 funnel collectors or more per hectare are necessary to maintain mean relative error below 10% for long-term monitoring, with as many as 200 collectors needed for similar error rates during event sampling (Zimmerman et al. 2010; Zimmerman and Zimmerman 2014). Spatial averaging using larger trough collectors obviates some of this sampling effort, yielding guidance of 5 trough collectors per hectare (Zimmerman and Zimmerman 2014), but still misses stemflow and groundcover variation. While the spatial integration extent of troughs vs. soil moisture sensors remains unknown, the three soil moisture sensors we deployed per plot (with sensor locations selected to span stand spatial heterogeneity) seems likely to capture similar spatial extents. Moreover, the strong correspondence in magnitude and forest structure controls (i.e., LAI, ground cover) between our measurements and literature reported values underscores that soil moisture measurements, at least in this setting, integrate key quantitative aspects of the interception process. If soil measurements are subject to the same fine-grained spatial heterogeneity as funnel-type collectors, it seems highly unlikely that our results would comport with literature expectations as closely as they do. One plausible explanation is that soil moisture dynamics average across extant spatial heterogeneity in canopy processes, allowing soil moisture measurements in the subsurface to provide comparable spatial integration to troughs, without the considerable maintenance of litter accumulation. Additional soil moisture measurements would undoubtedly improve the accuracy of field estimates, and indeed we recommend that more explicit methodological comparisons are needed. However, our results support the general applicability of the soil moisture-based approach for developing forest interception estimates across a wide range of hydroclimatic and forest structural settings."

R1-C8: Thus, based on the provided evidence I am not convinced that the method allows to estimate interception loss based on soil water content measurements. In the absence of direct measurements, the main claim of the paper is not supported by data. I agree that the derived values are plausible, and the paper can make this claim, but this requires a much more careful formulation of the title, abstract, discussion and conclusion.

AR8: We disagree that the paper's claim is not supported by data but acknowledge that the data supporting the findings come from other studies. We have modified the text in the abstract, methods, discussion and conclusion to stress the potential utility and benefits of the proposed method, along with conceptual caveats, methodological considerations, and suggestions for future work.

R1-C9: Furthermore, I think the paper contains a great deal of really interesting information, data collected in a thoughtful design as well as a clever analysis. The paper definitely allows drawing conclusions about how strongly different factors like LAI, %GC and antecedent soil moisture actually affect the top soil moisture response to rainfall. I would therefore highly welcome a change of the key message, and instead focusing on the observed soil water response to precipitation. This can be addressed with very similar analysis, but without the need to refer to very indirect evidence as is the case now.

> **AR9:** We appreciate the reviewer's interest in the data, design, and analysis. As noted in AR8, we have modified the text to temper the conclusions and further clarify that we rely on evidence/validation vis-à-vis other studies. We believe that refocusing the paper on observed soil water response to precipitation would reduce its utility, especially given the great quantity of excellent work on that topic over the past many decades. Moreover, the reviewers concern that we have insufficiently sampled a spatially heterogeneous process underscores the promise of this method since the results appear to be both stable and conform with stand structural predictions of interception losses. As such, we view this work, like all scientific efforts, as a contribution to a longer dialog and not the final word on the subject. Throughout the revised manuscript we now make clear that future work should more explicitly consider direct validation rather than literature-based validation as we've done here.

Detailed Comments

Furthermore, some editorial remarks:

R1-C10: The nomenclature in the manuscript is unnecessarily confusing and can be improved easily by homogenizing. For example, abbreviations of P and R are used for variables both referring to precipitation, while P could be used throughout with different indices. The abbreviation f is rather unfortunate choice for "infiltration flow", etc. Also, "soil moisture content" or "SMC" and Greek letter theta are both used for variables referring to volumetric soil water content. Please note that soil moisture content is rather unspecific and in the entire manuscripts actually "volumetric soil water content" is meant. The latter is a well-defined and established term. The established abbreviation is the Greek letter theta.

> **AR10:** We appreciate the reviewer's comments and apologize for any unnecessary confusion. We have attempted to better harmonize nomenclature in the revised manuscript. Regarding P and R, we have modified symbology such that all abbreviations of rainfall use P. Regarding the use of "f" for infiltration, this is the standard symbol for infiltration rate (dimensions of length per time) (e.g., in the Green-Ampt and Horton equations) with capital "F" referring to cumulative infiltration (dimensions of Length), so we have left it unchanged. We have modified SMC to θ throughout.

R1-C11: I propose separating the discussion and conclusions section.

> **AR11:** Modified as suggested.

R1-C12: Eq 1: Something is wrong with formatting of the equation. There should be no power to exp.

> **AR12:** Apologies, there was some conversion error during document upload, which we have rectified in the revision.

R1-C13: Eq 3: I find "f" a very unfortunate abbreviation for infiltration rate. The lower case f is so very commonly used to mean “function of” that this "f(..)" is strongly misleading.

> **AR13:** See AR10

R1-C14: L 126: change “E and f are infiltration and evaporation rates” to “E and f are evaporation and infiltration rates”

> **AR14:** Modified as suggested.

R1-C15: L 134: Something went wrong with formatting. It is sometimes bar and sometimes prime to demark the average.

> **AR15:** See AR12

R1-C16: Eq. 7: The sides of the equation are not equal. The logarithm in the middle part should be in the denominator (as in the right hand side).

> **AR16:** Modified as suggested.

R1-C17: L 140: R is now newly introduced as the rainfall rate – why not P with a different index? The many abbreviations are confusing.

> **AR17:** Modified as suggested; see AR10.

R1-C18: L 215: What is meant with banks? Vertical profiles? I tried a search engine and it appears this is a very uncommon formulation. Please rephrase.

> **AR18:** We have changed this term to “sets”.

R1-C19: L 216: “soil moisture content” or “SMC” is rather unspecific. The entire analysis assumes that the “volumetric soil water content” is meant. The established abbreviation is the Greek letter theta. I strongly suggest adjusting the nomenclature to the established scientific literature.

> **AR19:** Modified as suggested; see AR10.

R1-C20: L 261: The ANOVA should be introduced in the Methods section.

AR20: Description of the ANOVA has been added to the Methods section

R1-C21: Table 2: From the methods section, it appears as if more model versions were tested: four potential predictors and their interactions. Could you confirm or specify and also state how were the presented models selected? How about a case without LAI and only site and %GC?

AR21: As we stated in the methods, we ran a variety of permutations of model predictors. All models without LAI were markedly worse, and were omitted from comparison. We have updated the methods and results to make this clearer.

R1-C22: Figure 2: I have commented on this before: The equation in all panels are repetitions of Eq. 1, where y=P (Rainfall), and x= Δ SMC. However, the x-axis in the Figure is Rainfall (and not Δ SMC). In other words, the equation in the Figure is wrong, given that x and y are swapped in the figure as compared to the original equation. This should be harmonized.

AR22: Modified as suggested.

References:

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