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Discussions

Interactive comment on "Estimating Interception from Near-Surface Soil Moisture Response" *by* Subodh Acharya et al.

Anonymous Referee #1

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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.

This manuscript proposes that the interception storage can be derived from high temporal resolution top soil moisture measurements. The term "interception storage" is

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.

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:

(1) No direct data of canopy or litter interception are available, and those would be necessary to validate the method for good

(2) 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.

(3) 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.

(4) 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 als 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.

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.

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.

Furthermore, some editorial remarks:

(1) 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.

(2) I propose separating the discussion and conclusions section.

Detailed comments:

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

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.

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

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

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).

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

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.

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.

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

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

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.

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