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Hydrology and Earth System Sciences Discussions

Interactive comment on "Simplified stochastic soil moisture models: a look at infiltration" *by* J. Rigby and A. Porporato

J. Rigby and A. Porporato

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[Reviewer comments in italics, authors replies in bold]

The authors would like to thank all of the reviewers for thoughtful comments and constructive criticism which has led to a better manuscript. Below we respond to each reviewer's comments individually.

Reviewers please note that figure numbers refer to figures in the revised manuscript. Two figures have been added. The figures are supplements to the original manuscript Figures 5 and 6. Thus the original Figures 5 and 6 are now Figures 5 and 7 in the revised manuscript.

Response to F. Laio:

Rainfall modelling The first problem is an analytical one: at page 1346 and 1347 the Authors assume that the distribution of the wet periods durations, $f_W(w)$, is exponential with mean δ, and that the distribution of the rainfall depths, $f_D(D)$, is also exponential with mean α. By assuming independence between w and D they obtain the marginal distribution of the rainfall average intensities, $P = D/w$, see Eq. (8). However, assuming independence of w and D, as done in the manuscript to obtain Equation (8), is not the same as assuming independence between the intensities P and the wet-period durations, w, as Eagleson did in his 1978's paper. In fact, independence between D and w does not imply independence between P and w, as erroneously mentioned at page 1347, line 17-18: on the contrary, under the hypothesis that D is independent of w, the average rain intensity and duration become mutually dependent variables, with a (strong) negative correlation.

The reviewer's observation is certainly true. The statement on page 1347 was not intended to imply independence, but to state that neither model made an explicit aim at accurately modeling the dependence between intensity and duration. The statement was poorly phrased and has been removed. In addition, the dependence between intensity and duration has been made explicit in the text.

This is not only a matter of notation, since one should account for the dependence between P and w when carrying out the simulations: in fact, one should first sample a wet-period duration w, and then sample a rainfall intensity P from the conditional distribution of P given w, $f(P|w)$ rather than from the marginal distribution of P, as done in the manuscript. The use in the simulations of the marginal distribution likely produces an overestimation of the number of rainfall events with long duration and large intensity, which in turn increases the probability of having Hortonian runoff. The two infiltration models considered in the paper will then probably produce even closer results when the correct P distribution is adopted in the simulations. An additional problem could be the necessity to find out a physical justification for the resulting negative correlation between P and w.

In the manuscript, intensity was not drawn from its marginal distribution. Instead, a total rainfall depth and a (statistically independent) duration were drawn according to respective exponential distributions. The intensity is then determined by the ratio. The resulting marginal distribution of intensity is that given in Eq. (8). The negative correlation is therefore present in the same way as if we had selected a random duration and a statistically dependent intensity to characterize each event. The negative correlation between intensity and duration seems physically reasonable (e.g., de Michele and Salvadori 2006) though the structure of this correlation is admittedly simplistic as it is derived from a simple exponential model for rainfall. Furthermore, we wish to note that the choice of rainfall process will certainly affect the runoff production. In this paper the question is that, insofar as we are willing to accept this model of rainfall, when does time-varying infiltration play a significant role in partitioning rainfall events? A more detailed analysis of effects of durations and intensity of different hyetograph models is of interest but outside the scope of the paper.

Lack of generality Another area of possible improvement regards the possibility to make the comparison a more general one: the Authors selected a set of parameter values, which are listed in Table 1, with only a couple of these parameter values which are allowed to vary. Since the aim of the paper is to define the conditions when the two infiltration models can be considered to be equivalent, the parameter space should be explored in greater detail: for example, the saturated hydraulic conductivity ks is taken to be 200 mm/d in the manuscript, but one can guess from equation (14) that this value has a strong influence in limiting the relevance of the Hortonian runoff component (i.e., decreasing ks the differences between the two models could become much more relevant). My suggestion is then that the Authors include some more Figures and comments to better describe the influence of the climate and soil parameters (e.g., ks, or the rooting depth Zr) on the two infiltration models.

We have attempted to address the issue of generality by focusing on the vari-

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ability of hydraulic conductivity as it is by far the largest source of variability in the Philip solution, Eq. (15). Two figures have been added to extend the analysis to a larger range of saturated hydraulic conductivity (two orders of magnitude). The first is a supplement to Fig. 5 for $K_s = 5$ cm/day and $K_s = 200$ cm/day corresponding to a clay and a sandy soil, respectively. With respect to the partitioning of rainfall we include a new figure showing the proportion of losses attributed to surface controlled runoff as a function of storm depth for different intensities. Note that this figure may be read either as showing different intensities falling on the same soil, or different soils acted on by equally intense rainfall. Table 1 has been updated with the range of parameters.

Response to Anonymous Reviewer 2:

Modeling Objective The goal of the paper is to determine when two models of infiltration can be deemed equivalent. The answer to this question depends on the particular modeling objective, and I think this aspect of the study warrants greater attention. The primary comparison presented is in the representation of soil moisture over time, as characterized by Figures 4 and 5 in the paper. While the specific values of soil moisture may be important for some applications and questions, such as determining rates for nutrient cycling, of at least equal importance is the partitioning of rainfall between evapotranspiration and runoff and recharge. I recommend that the test of equivalency between the models be expanded beyond the representation of soil-moisture behavior.

The comparison in the paper has been expanded to focus on two central points. First, the effect of variable infiltrability on the probabilistic character of the soil moisture process is examined (Fig. 5 and 6). Second, the effect of variable infiltrability on partitioning of rain events is examined (Fig. 7 and 8). Figure 8 in particular shows the proportion of losses attributed to surface controlled runoff for an initial soil moisture value near the mode of the distribution (see Fig. 5).

Modeling Simplifications In the model presented, the rate of water loss from the root *zone increases with increasing soil moisture - the dependence is linear in the case of ET and nonlinear (Brooks and Corey) in the case of percolation. This state dependence acts to correct for differences between the models that may arise in the infiltration process. For example, if the IEM overestimates infiltration, the resulting increase in soil moisture will lead to a higher rate of losses from the root zone. Thus, the conclusion that the traces of soil moisture over time look similar for the two models is not terribly surprising as differences in infiltration are quickly damped out. The partitioning among fluxes may be different, however, and the manuscript would benefit from an expanded evaluation as discussed above. Additionally, with respect to ET, the authors refer the reader to the paper by Kim for a discussion of the linear dependence - as opposed to a function that reaches a plateau at higher values of soil moisture. As the functional form is central to the determination of equivalency, I believe it warrants discussion and not simply a reference (see also below).*

To address the partitioning of fluxes, we have included a new figure showing the proportion of losses attributed to surface controlled runoff as a function of storm depth for different intensities (Fig. 8). This figure shows deterministic curves derived from numerical solution of the differential equations governing the models presented. With respect to the rainfall process, Figure 8 assumes only that rainfall intensity is constant over the duration of the event and is therefore a more general result than the simulated probability densities.

Additionally, with respect to ET, the authors refer the reader to the paper by Kim for a discussion of the linear dependence - as opposed to a function that reaches a plateau at higher values of soil moisture. As the functional form is central to the determination of equivalency, I believe it warrants discussion and not simply a reference (see also below).

A short discussion has been added to the text. However, since evapotranspiration (ET) occurs only between rainfall events, in the two models considered, it is our belief that any detailed discussion on this point is outside the aims of the

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paper. The results of the paper are practically independent of the treatment of evapotranspiration since both models use the same ET model and since evaporation acts only during interstorm periods.

Characterization of the parameter space The authors explore a limited set of climate, soil, and vegetation parameters in their work. To ensure that the results that are included are interpreted properly, I recommend that the authors provide a more complete description of the parameter space and the impacts of their choice of parameters on the results. For example, with the parameters given in Table 1, the rate of percolation is equal to the evapotranspiration rate at a soilmoisture value of approximately 0.5. Above this saturation, percolation is faster and below this point losses due to ET are greater. It may be worth noting the value of this crossover point.

As said before in the reply to ref. 1, we have expanded the range of values considered in the analysis. Moreover, as evapotranspiration is assumed not to occur during rainfall events, while interesting, this last point seems tangential to the primary question in the paper and will be explored in subsequent analyses.

Additionally, the index of dryness, the ratio of mean precipitation to potential evapotranspiration, for the set of conditions investigated is 1.2. This indicates that precipitation is greater than potential transpiration, and, since potential transpiration is only met when saturation is 100Ě

The end of this comment was lost somewhere. With respect to the index of dryness, we investigate D=1.2 and D=0.6 corresponding to the two Poisson frequencies in the rainfall process (Fig. 5 and 6). The second value of lambda was missing from the original table of parameters (Table 1). This error has been corrected. Explicit mention of the index of dryness is made in the caption of Fig.5.

In general, I encourage the authors to better articulate how the choice of parameters has influenced the results and conclusions presented and how these results may change for different sets of parameters (see also below).

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Coverage of the parameter space The authors explore a limited set of climate, soil, and vegetation parameters in their work. As mentioned above, the general applicability of the results will be increased if that set of parameters is discussed in greater depth. Additionally, the results can also be extended by exploring a greater range of the parameter space. Answering the question of under what conditions are the models equivalent vs. not equivalent would have greater power than answering whether they are or are not equivalent for one set of parameters. I am not advocating that the authors span the entire parameter space, but I encourage them to select a few different environments for evaluation of model equivalency.

We have attempted to address the issue of generality by focusing on variations of hydraulic conductivity as it is by far the largest source of variability in the Philip solution, Eq. (15). Two figures have been added to extend the analysis to a larger range of saturated hydraulic conductivity (two orders of magnitude). The first is a supplement to Fig. 5 for $K_s = 5$ cm/day and $K_s = 200$ cm/day corresponding to a clay and a sandy soil, respectively. With respect to the partitioning of rainfall we include a new figure showing the proportion of losses attributed to surface controlled runoff as a function of storm depth for different intensities. Note that this figure may be read either as showing different intensities falling on the same soil, or different soils acted on by equally intense rainfall. Table 1 has been updated with the range of parameters.

Response to Anonymous Reviewer 3:

The main merit of this study is to assess the effects of a major simplification underlying the analytical model, i.e., the assumption that rainfall is instantaneous (this assumption was necessary to obtain an analytical expression for the probability distribution of soil moisture). The manuscript is technically sound, well written, and provides an important contribution to the field of soil moisture modeling. I definitely recommend it

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for publication in HESS.*This paper compares two models of soil moisture dynamics: (1) an analytical stochastic model of the soil water balance with instantaneous rainfall occurrences and (2) a numerical model of soil water dynamics with rainstorms of finite duration. By accounting for the duration of rainfall events the second model is able to resolve the temporal dynamics of infiltration and to calculate possible losses of soil water associated with Hortonian runoff. To this end the authors use Philip's (1957) infiltration theory. On the other hand, the analytical model accounts only for saturation-excess runoff, which can be calculated without resolving soil moisture dynamics in the course of rainfall events. The general goal of this study is to assess whether the probability distributions of soil moisture calculated with the more detailed (numerical) model is significantly different from those provided by the analytical simplistic model of stochastic soil moisture dynamics. It is found that only minor differences exist between these two probabilistic structures of soil moisture. Thus, in most cases the simplistic analytical framework provides a sensible representation of the stochastic dynamics of soil moisture at the daily time scale.*

The main merit of this study is to assess the effects of a major simplification underlying the analytical model, i.e., the assumption that rainfall is instantaneous (this assumption was necessary to obtain an analytical expression for the probability distribution of soil moisture). The manuscript is technically sound, well written, and provides an important contribution to the field of soil moisture modeling. I definitely recommend it for publication in HESS.

The reviewer's kind comments are appreciated.

Response to S. Manfreda:

Authors obtained very similar results in the temporal behaviour of the soil moisture. This is surprising to me and probably related to the limited set of soil textures investigated and to the range of parameters used to characterize the rainfall process. In a similar work we have found, using numerical simulations, that the Hortonian runoff may

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significantly influence, in some cases, the right tail of the soil moisture probability distribution reducing the asymmetry of the soil moisture PDFs. Furthermore, the differences in the PDFs generally increase with the rate, λ, of the Poisson process of rainfall, in climates characterized by short duration and high intensity storms and when the soils have low permeability (Manfreda et al., 2004).

This is an interesting point. No obvious systematic change in the symmetry of the distribution arises in our analysis, even under very low conductivities (Fig. 5 and 6). The plot in Figure 6 for $K_s = 5$ cm/day may reproduce this phenomenon as the IEM curve appears to have a slower decay for $s > 0.5$, but as the shift in the mean is substantial it is hard to compare the tails.

About the Dunne mechanism In the present case, the use of the term "Dunne runoff" is probably incorrect since the authors are dealing with a point process. It would be more appropriate to consider this process as runoff generation for saturation excess.

This point was noted and helped clarify our thinking on the proper terminology for use in the paper. Significant changes and clarifications have been made in the revised text removing the explicit association between subsurface-control and Dunne. In the text we point out that the distinction between surface-controlled and subsurface-controlled runoff is artificial when only considering soil moisture at a point.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1339, 2006.