Hydrol. Earth Syst. Sci. Discuss., 3, S403–S407, 2006
www.hydrol-earth-syst-sci-discuss.net/3/S403/2006/

Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #2

Received and published: 10 July 2006

Overall

The manuscript presents a rigorous comparison of two models of infiltration with the goal of determining an appropriate level of complexity needed to represent the soil-moisture dynamics of the vegetation root zone. Neither of the infiltration models has any spatial resolution, and the focus is on those situations for which the root zone is treated as a single unit. The first model, the instantaneous event model (IEM), is a simplified bucket-filling model with no temporal resolution in which rain events are characterized by a depth and arrival time. The second, the finite-duration event model (FDEM), is a model based on Philip's infiltration equation, and storms are represented as rectangular pulses of rainfall with an arrival time, a duration, and an intensity. The two models are structured so as to be equivalent between storm events; i.e., the state dependency of the loss of water from the root zone is the same for both.

The comparison of the model behavior is rigorous, and the authors present some very nice insights. In particular, the graphical partitioning of losses from the root zone, presented in Figure 6, clearly shows the importance of percolation during a rainfall event. The results that speak to the equivalency of the models are presented in Figure 4, which shows that the two behave quite similarly under the conditions explored.

The results of the work are sound, but the impact of the effort is somewhat limited by two factors. The first is the restriction of the comparison to a very small subset of the climate-soil-vegetation parameter space. The second is the limited articulation of the modeling objectives, simplifications, and the character of the parameter space that is explored. While these limitations do not detract from the work that is presented, I think they represent missed opportunities. I have offered specific suggestions here, and I encourage the authors to take them into consideration. A few minor corrections and recommendations are offered at the end.

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.

Modeling Simplifications

In the model presented, the rate of water loss from the root zone increases with increas-

HESSD

3, S403–S407, 2006

ing 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).

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 soil-moisture 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.

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 100

In general, I encourage the authors to better articulate how the choice of parame-

HESSD

3, S403–S407, 2006

Interactive Comment

ters has influenced the results and conclusions presented and how these results may change for different sets of parameters (see also below).

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.

Minor corrections and recommendations

When the words “soil moisture” are used as a compound adjective, such as in “soil-moisture model” or “soil-moisture dynamics”, “soil” and “moisture” should be joined by a hyphen.

On p. 2, the authors present their expression for drainage as $k_s S^{c+1}$. The Brooks and Corey relationship is $k_s S^{(2+3m)/m}$. For the two expressions to be equivalent, c must be equal to 2*(1+m)/m. Below equation 2, the authors present this as c = 2/m(1+m). While this is equivalent to the expression above following the strict order of operations, at first glance it gives the visual impression that the denominator contains the product m(1+m). I recommend rewriting this equation to make the relationship clearer.

On or near p. 5, please define the normalized precipitation rate (P tilde), which I believe is P/k_s

Figure 6, which depicts the partitioning of precipitation, is predicated on an initial soil-moisture value of 0.6. This value of soil moisture is at the edge or outside the range

HESSD

3, S403–S407, 2006

of the distributions presented in Figure 5, and thus may be misleading for the reader. I suggest recreating the figure with a value of $S_0 closer to 0.3$.

To help characterize the types of environments investigated, I recommend that the authors also use standard dimensionless groups such as the Index of Dryness.

In all of the figures, but particularly for figures 6 and 8, I recommend that the authors improve the axes labeling and add detail to the captions so that the figures can stand alone.

On page 7, reference is made to the "one-to-one" correspondence between event depth and change in saturation. However, this correspondence is also a function of initial soil moisture and precipitation intensity. Without specification of those variables, the relationship is not one-to-one but multivalued. I suggest that this be made clear.

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

HESSD

3, S403–S407, 2006

Interactive Comment