Hydrol. Earth Syst. Sci. Discuss., 11, C2784–C2786, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C2784/2014/

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Gravitational and capillary soil moisture dynamics for hillslope-resolving models" *by* A. Castillo et al.

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Received and published: 28 July 2014

Review of A. Castillo, F. Castelli, and D. Entekhabi, Gravitational and capillary soil moisture dynamics for hillslope-resolving models

Review by Andrew J. Guswa, aguswa@smith.edu

Overall Evaluation In this paper, the authors present a proof-of-concept for using a zero-D dual-porosity soil-moisture model in lieu of the 1D Richards equation to represent hydrologic fluxes in the vadose zone. The new representation is implemented in the MOBIDIC model. The authors compare predictions of soil-moisture (integrated over the top 30 or 50-cm) from a calibrated version of the MOBIDIC model to predictions from a calibrated SHAW model for two sites: one semi-arid (Arizona) and one

sub-humid (Mississippi). The paper shows that predictions of the temporal dynamics of average soil-moisture can be adequately captured by either model.

The primary message of the paper is that the simpler MOBIDIC model can describe the soil-moisture dynamics with much less computational and modeling effort than soil-physics codes. The authors present this case well, and the two case studies provide a decent proof-of-concept. What I found lacking in the paper, however, is a clear articulation of the benefits of the dual-porosity approach over a simpler "bucket" model. As the authors themselves point out in the introduction, use of such models is quite common. And, similar to the results in this manuscript, many have shown that these simpler models of average soil-moisture can capture the temporal dynamics as well as more complex representations.

The authors have told only half of the story – they have demonstrated the adequacy of their simpler dual-porosity model relative to codes that solve Richards equation. However, the current version of the manuscript does not effectively make the case as to why the dual-porosity model is needed – what is gained? What are the advantages over a bucket model? And models such as that of Milly (1993), Rodriguez-Iturbe et al. (1999), and Laio et al., (2001) employ a piece-wise loss function, effectively changing the dynamics when the soil moisture exceeds critical points, such as field capacity. Those bucket models in some ways already capture the essence of gravity versus capillary water by turning drainage off when soil moisture drops below field capacity. How is the explicit representation of gravity and capillary water, and the exchange between them, superior? Would a bucket model with a single state variable for soil moisture be unable to represent the average soil-moisture dynamics for the two case studies? Without that comparison, the impact of the paper is much less than it could be.

Specific Comments In the description of the losses from the gravity water store (equations 4-7), it appears that the sum of Qas, Qper, and QL could exceed the water available in the gravity store. If Wg is low, is it possible that Wg/dt + gamma Wg + Beta Wg would exceed Wg?

Also, in that same section, the manuscript refers to subscripts “up” and “down”, which do not show up in equations. Overall, I think the methods section could be improved in terms of clarity.

Lastly, the authors offer an explanation for the underprediction of soil moisture at Site 2 during the validation phase (Figure 6b, days 850-1100) – that irrigation water may be propping up the observed soil moisture. However, during the calibration phase, the models show the opposite behavior – that is, they show a more muted response rather than the rapid dry-down observed in the measurements. This difference may be worth some additional discussion.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 7133, 2014.