Hydrol. Earth Syst. Sci. Discuss., 8, C4759–C4760, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C4759/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "An analytical model for soil-atmosphere feedback" *by* B. Schaefli et al.

Anonymous Referee #2

Received and published: 31 October 2011

General comments

The authors present a new analytical model to study the moisture exchange between the land surface and the atmosphere in a simple and therefore elegant way. Similar to the reviewer that posted the first review, I find that there are assumptions in the model that cannot be accepted, until the authors come up with data that justify the assumptions. Sometimes the paper gives the impression that the authors oversimplified the system in their pursuit of creating an elegant and solvable analytical system.

Whereas the first reviewer casted his doubts on the assumptions behind the soil model, I would like to spend a few lines on the atmospheric model. The authors consider the atmosphere as a box with vertically uniform properties that is being advected along

a stream line. I do not see any justification for this assumption. Moisture is not well-mixed in the atmosphere and wind speeds are not constant with height, which means that the origin of the moisture varies with height as well. An illustrative example is the West-African monsoon system. Here, at lower levels moisture is being advected from the Atlantic ocean towards the continent, whereas the large scale flow patterns at higher levels are in the opposite direction. Such a situation, which is not uncommon in locations with strong coupling between the land surface and the atmosphere, seems to be hard to describe using the model in this paper.

Then, as the first reviewer already suggested, the assumption of having a constant rate of change of soil moisture in time takes away the applicability of your model in regions where soil-atmosphere feedbacks are most exciting, namely those where the soil moisture content is a strong function of the local precipitation rate and vice versa, and not of the season. A fixed rate of change of soil moisture represents a non-coupled system and limits the applicability of the model to regions where the soil moisture is determined by large scale meteorological systems.

Furthermore, balancing soil moisture in a Lagrangian framework seems rather odd to me as the soil column should not advect together with the air column. Streamlines are variable in time, and have a much shorter time scale than soil moisture, so I do not directly see how soil moisture can be balanced along a stream line. Therefore, I would like the authors to clarify this decision. Then, the authors do not justify the assumption that the parameters u_x, τ_p, τ_p, τ_e, e_m, s_m and I are constant in space.

To make their model convincing, I suggest that the authors take reanalysis data to back up their assumptions and to show that their framework at least reproduces the general trends of soil moisture and atmospheric moisture when moving over streamlines, before they apply the model to explain the behavior of the land-atmosphere system under different regimes.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8315, 2011.