**Hydrology and
Earth System
Sciences
Discussions**

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

Anonymous Referee #3

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Let me state in advance that I am not a specialist in hydrological modelling.

I am a firm supporter of simplified ("toy") mathematical models of meteorological processes. In other words, I welcome attempts to understand complicated non-linear problems with the help of simplified mathematical models. In this sense the paper under review can, in my opinion, potentially contribute significantly to the problem of understanding the hydrological feedback between atmosphere and land, which is an important open problem in hydrology and meteorology.

However, my first impression of this paper is that it needs a lot of editing before it can meet with this need. Not only that, in my opinion the argumentation is frequently unclear and imprecise and sometimes, I suspect, in error. I also suspect that the authors are not fully in control of some basic fluid-dynamical principles.

One example of an imprecise sentence occurs in the abstract: "The solutions of the resulting water balance equations correspond to two different moisture regimes along a stream line, either monotonically increasing or monotonically decreasing when travelling inland,..." What exactly is increasing or decreasing? As it stands, "the moisture regimes" are increasing or decreasing. I do not think the authors are intending to say that the "the moisture regimes" are increasing or decreasing.

Another imprecise statement occurs on page 8319, line 5: "This approach presents analytical advantages since Langrangean trajectories, which can be obtained from data (e.g. ....), contain considerably more information than what we would have in a purely Eulerian description using only velocity fields". This sentence does not make sense to me! What kind of data are you referring to? Do you mean observational data or do you mean model data? How are Lagrangian trajectories determined if they are not determined with information about the velocity field?

The term "Eulerian-Langrangean" is used very frequently, but is unfortunately hardly very informative. For instance in the following sentence (p. 8318, line 10): "In this paper, (why the comma?) we present a different type of analytical model: it describes the hydrological cycle at points along an atmospheric stream line (Eulerian-Langrangean approach) using only...etc. What is Eulerian about this approach and what is Lagrangian about this approach (note the correct spelling of Lagrangian!!)? A Lagrangian approach is one in which the observer follows an individual material element, such as an air-parcel, along its trajectory. A stream line is not the same as a trajectory, except in stationary conditions (i.e. local derivative with respect to time equal to zero). The authors do not explicitly assume stationary conditions, although there is an indication that they are in fact doing this in equation (4) (page 8320), by assuming that $\partial f/\partial t=0$. Equation (4), nevertheless, is introduced and explained inappropriately with the following text: "Equation (3) can be written in Lagrangean framework using the substantial or Lagrangean derivative" (the spelling of "Lagrangian" is now different, but still wrong). There is no explicit indication by the authors that they are actually assuming station-

ary conditions by assuming that $\partial f/\partial t=0$ in equation (4) and $\partial b/\partial t=0$ and $\partial W/\partial t=0$ in equation (5).

Another inappropriate introduction of an equation is the introduction to equation (1) (p. 8319): "The conservation of mass for M reads as...". However, equation (1) appears to include a term describing diffusion of mass (the second term on the r.h.s.). With this term we are in fact studying a control volume that exchanges mass with its surroundings by diffusion. So, equation (1) should not be referred to as a mass conservation equation, but rather as a mass-budget equation.

Furthermore, the definition of M=VW, where the dimensions of V are [L^3] and W apparently is dimensionless is not consistent with M being a mass. When I compare the dimensions of the different terms in (1), I conclude that, indeed, M=VW must have dimensions of volume. So, I guess my confusion is with the definition and meaning of "relative moisture filling", W, the most important variable of this paper! Clearly, W is not explained adequately.

The assumption on page 8324 that "for sufficiently small time steps (weeks to months), the change of soil moisture $\partial S/\partial t$ can be approximated with a constant rate of change" is crucial to allow a reduction of the problem to the solution of a first order (not a second order) differential equation (17). Except the comment about the time steps, there is no justification for this approximation, which is difficult to understand.

Another important assumption that is presented without justification is the assumption ux=constant (on page 8324, line 11). This assumption, it seems to me, implies that the moisture content along a streamline changes at a prescribed rate, if b varies along streamlines according to the prescribed value of l (line 7 on page 8321) and conditions are stationary. If this is true, the problem is too strongly constrained to yield answers to interesting questions. The coupled system of two equations (14) and (15) probably has solutions that are much more interesting to reveal and study than the solution of equation (17).

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In view of my questions about the (explanations of the) foundations (or fundamental assumptions) of this study, I cannot at this stage recommend this paper for publication in HESSD.

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