Hydrol. Earth Syst. Sci. Discuss., 8, C2553–C2555, 2011
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Hydrology and Earth System Sciences Discussions

Interactive comment on "The potential of observed soil moisture dynamics for predicting summer evapotranspiration in a successional chronosequence" *by* J. A. Breña Naranjo et al.

Anonymous Referee #1

Received and published: 30 June 2011

Comments: This paper deals with the estimation of AET during the summer season using 2 different data based water balance concepts: the first one uses changes of soil moisture in the root zone (here 30 cm) in a simple mass balance framework; the second approach additionally considers evaporation from an interception storage. These concepts are applied to a successional chronosequence at Campell River, BC, Canada and estimates are compared to eddy covariance data from 3 FLUXET sites for a period from 2001 to 2008. AET was compared at different levels of aggregation including mean total summer AET, the mean diurnal cycle and 10 day averages. Model time steps were varied between 30min and 1day. In doing so, the author present two aspects: i) they test two simple approaches to derive AET; ii) the provide AET data over 8 years for a successional chronosequence, but only at one location.

i) Modelling:

While I am generally in favour of simple data-driven approaches, I have some major concerns with the approaches and analysis presented in this manuscript: 1.One important parameter in eqn. (1) is the depth of the active root zone Hr, which is set to 30cm for all three locations. How is this justified given the vegetation type (douglas fir). Why is the WC measurement 0-100cm not used instead? But even then it might not be the appropriate depth! 2. How spatially variable/stable are the pattern of WC dynamics within the footprint of the EC – measurement? (The footprint is changing with atmospheric conditions?) 3. As precipitation is an input into eqn. (1) and (2), how variable is that within the footprint? 4.Given the non-closure of the energy balance (p.5304,l.20), is there any need for a correction of measured fluxes to compare with? 5.How can authors justify an exchange of water vapour between the canopy and the atmosphere of 0.5 mm/h for P>0.5?? I would expect this rate to depend on meteorological conditions (Rn, T, v, . . .) So, given these question I believe that a much more thorough analysis of the applied water – balance method is necessary.

ii) Data

Looking at the aspect of comparing AET for a successional chronosequence, only the pure eddy-covariance data would be sufficient! However, then my impression is, that the analysis of data from only one location have some value, but only very limited in terms of generality and are not justifying a publication in HESS. Some minor comments are listed in the following: p.5304, l.4: Is the Baldocci et al. 2001 paper really the correct one to refer to the test site? p.5305, l.20: How is I(t) defined? I thought I is the interception storage, not the evaporation from it? p.5305, l.20: What is the final form of Model 2? p.5311, l.2: What is a first order estimate?

Overall, I would like to see some major revisions on the manuscript, strongly considering the comments made and questions posed above.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5301, 2011.