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Interactive Comment

Interactive comment on “

Quantifying the uncertainty in estimates of surface- atmosphere fluxes through joint evaluation of the SEBS and SCOPE models” *by* J. Timmermans et al.

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Review of paper by Timmermans et al., ‘Quantifying the uncertainty in estimates of. . ..fluxes. . .’

This is a good, well-written paper which discusses a couple of decent land surface models and their ability to retrieve surface turbulent energy fluxes. It follows the methodology expressed in a large number of papers on this subject. As such I have no objections to this paper, aside from a few minor points to be made. My main caveat is a general one, which I will now try to make. These two models cited In the paper are probably as good as most SVAT models in existence and can be used with good results in studying plant-atmosphere behavior for specific surface plots with accompanying ancillary surface data. I don't believe that this approach is best for estimating surface fluxes over regional scale (1 - 100 of hectares) using satellite for the reason that too much ancillary information is required to make such models useful for operational purposes. Moreover, for such purposes it is probably a waste of time trying to improve this or that parameterization, whether it be zo or kB-1 or G/Rn, etc., because the effort in improving the model by a few Wm-2 is not going to be worth the effort. The authors themselves refer to the uncertainties in such parameterizations on page 2874. I suppose that the authors are aware of the various types of 'triangle' methods such as those published by Jiang and Islam, Petropoulos, and Carlson; see Sensors, 2007. This type of method uses the configuration of pixels in fractional vegetation-scaled surface temperature space to constrain the solution to the myriad of equations such as those listed in this paper. As such the need for obtaining the correct formulations of every type of parameterization is much reduced and the need for an elaborate SVAT model is almost irrelevant. For example, Jiang and Islam use only a Priestly Taylor type of model and assume linear variation of the phi parameter across the domain of the triangle. The authors seem to be using this approach as a sideline at the start of section 4.5, though I had trouble understanding what they were doing here with this equation. Carlson uses a SVAT model but only to map the isopleths of LE/Rn and surface moisture availability across the triangular domain, just once for any series of simulations given that both fractional vegetation cover and radiometric surface temperature are scaled between 0 and 1.0; better still to use fractional vegetation cover rather than NDVI. The latter approach employed by Carlson and others, does make the isopleths in the triangle domain quite non-linear and thus may improve the accuracy of estimating EF over the Islam method, though this is not certain. Carlson utilizes a two parameter SVAT model, similar to the Norman formulation cited in this text, in which the surface is imagined to consist of patches of dense vegetation with LAI something like 3, and bare soil. Fluxes from both surfaces are melded according to the fractional vegetation cover. No doubt comparisons with these models or methods against each other or against surface data will show one or the other at times better than the rest for a particular data set, but the overall accuracy of this type of model is bound to be very similar. And why not? All use similar physics in the SVAT equations. Regardless of the formulation for the SVAT model, or whether a Priestly Taylor model is employed, the triangle approach, is, I feel, superior to the approach in which H or LE is computed as a residual of the energy balance for a single patch of mixed bare soil and vegetation because the solutions for LE and H are constrained by the observations. As such, H or LE cannot stray beyond those limits, thereby avoiding nonsensical results, such as EF greater than 1.0. In regard to the mixture idea, surface in which LAI are less than 1.0 probably no longer respond well to the LAI formulation, as much of the surface would then consist of bare soil with patchy plants; the authors make this point themselves on page 2874 and again on page 2876. (My advice to them is to apply a two stream model, treating vegetation separately from bare soil, and let the boundaries of the pixel distribution constrain the solutions. Then treat surface temperature and vegetation or NDVI as a parameter that varies from 0 to 1.0.) In summary, my major criticism of this paper is that this paper would have been ground breaking 25 years ago when this type of SVAT model was first being published. Specific comments: P 2870 what do the authors mean by saying that the model can reproduce past, current and future data? P2874. I wonder if my pages did not print properly. I have no equation 11. P2878. Did the authors mean to say ‘higher’ in line 11 of section 4.5? Looks like that to me when I look at their figure. BTW, even if the EF swings up or down at the end or beginning of the day, the resultant error in assuming constancy of EF with time during the day would be minimal because the fluxes themselves would be much smaller at the beginning and end of the day. P2880. Here I became confused on line 25, where the

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authors refer to an ‘original parameterization’ that uses a weighted average approach for H and LE. This would seem to be the two stream idea I just mentioned, though the previous discussion did not seem to indicate that such a two stream methodology was being applied. Please enlighten this reviewer on this point! Finally, if the authors have not already checked out the literature on the triangle method, they should look at the various papers by Jiang and Islam, Carlson, Petropoulos, and others (though my memory seems to be blocked at this point).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2861, 2011.

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