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Hydrology and Earth System Sciences Discussions

Interactive comment on "Roles of spatially varying vegetation on surface fluxes within a small mountainous catchment" *by* G. N. Flerchinger et al.

Anonymous Referee #2

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Flerchinger et al present interesting data that will be useful to the hydrologic community at large. This data set includes simultaneous eddy covariance measurements of latent and sensible heat fluxes over three vegetation types in a small headwater catchment. This study provides a useful data point for future reference, but some of their description of the problem need to contain more description. In particular, the authors need to provide a more thorough review of the literature, and provide a better assessment of the errors in their measurements including the effect of removing particular periods of data.

One of the three primary measurements in this study is of the turbulent fluxes beneath

an aspen canopy, and presumably representative of the understory. However, they do not provide convincing evidence that this measurement is representative. They use a flux footprint analysis to show that turbulent fluxes originate from within a reasonable range, but these analyses do not include the effect of the aspen canopy overhead, and thus it is not clear that they are applicable. Second, one might expect the aspen trunks to exert an influence on the turbulent structure in this environment. While it is beyond the scope of this paper to perform a detailed analysis on this problem, some consideration of this problem would be reasonable. e.g. does distance from an aspen tree affect the observed fluxes?

The primary conclusion of this paper seems to be that spatial variation in vegetation cover affects ET. It is not clear that this in itself is a novel conclusion. Certainly studies have been addressing this issue for some time (e.g. Balducci et al 2004) and land surface models have been including vegetation type almost since their inception (e.g. Dickinson et al 1986). While it is still a useful publication, the authors need to include a much more thorough review of previous work (beyond their own) to show how the present study fits into the existing science.

In the introduction and methods section, more discussion of the effects of complex terrain on flux measurements is warranted. Is the topography around their sagebrush site in particular variable? If it is sitting near a ridgeline (as it seems to be) then when the wind comes from one side of the ridge, the effective net radiation (as modified by the cosine of the solar zenith angle relative to the slope) will be substantially different then when the wind comes from the other side of the ridge, yet the authors state that most if not all wind directions at this site are reasonable.

The authors claim to close the water budget for the basin, yet they have no estimate of ET over the fir which they state covers some portion of 34

The authors should briefly discuss the effect of extrapolating their aspen measurements to cover a year. e.g. based on the data they use to extrapolate the aspen

data, what are the expected errors and how do those errors relate to the annual water budget.

The authors state they have applied a wind correction to the precipitation record. This is a complicated topic (especially where snow is concerned). It appears that most of the precipitation at this site is likely to come in the form of snow so this is likely to be a particularly large problem for them. What sort of wind shield did they use? What was their correction? What sort of rain gauge? Where was this located relative to the aspen stand (above canopy, below canopy, in a clearing, outside the stand?) This is particularly concerning given how different their annual precipitation is at the sage site (704mm) relative to the aspen site (759mm). This seems like a large annual difference for two sites that are less than 500m away from each other.

They state that at the aspen site they only use data from periods in which winds were from 170-290 degrees azimuth, but wind direction is often correlated with precipitation events, as such this may substantially bias their results, some analysis of this problem is required.

They claim to have screened their data for quality control. While this is acceptable, the objective criteria used to screen the data must be published to make this study repeatable. Authors must also specify which months are missing from the understory data and how this might effect their results.

More details on the footprint analysis would be helpful. Certainly a lot of work has been done on the topic since Schuepp et al 1990 (Blanken et al 1997 is just using the criteria from Schuepp et al, not developing anything new). The authors would do well to at least mention that this paper is still relevant based one more recent research. Also, did they remove data when the fetch was too large? If so, what proportion of their data was that and was it correlated with precipitation events?

How are the authors interpolating precipitation to get their areal average? krigging? a PRISM type interpolation? other?

References: Balducci et al, 2004. How plant functional-type, weather, seasonal drought, and soil physical properties alter water and energy fluxes of an oak-grass savanna and an annual grassland. AGRICULTURAL AND FOREST METEOROLOGY,123(1-2),p13-39

Dickinson,R.E.,A.Henderson-Sellers,P.J.Kennedy,andM.F.Wilson, 1986: Biosphere-Atmosphere Transfer Scheme (BATS) for the NCAR Community Climate Model. NCAR Tech. Note TN-27 STR, 72 pp.

Other references are from the original manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 593, 2010.