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Interactive comment on "The effect of warm-season precipitation on the diel cycle of the surface energy balance and carbon dioxide at a Colorado subalpine forest site" *by* S. P. Burns et al.

Anonymous Referee #2

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In this manuscript Burns et al., describe changes to the energy balance, latent and sensible heat fluxes associated with warm season precipitation events in a forest in Colorado. The work utilizes a 14 year EC timeseries, which provides the authors enough data to develop precipitation composites. This is generally an issue because precipitation is sporadic and thus difficult to get a "generic" picture of its effect on the forest fluxes. The motivation for the work is well founded as the effects of precipitation are generally ambiguous, for the reasons mentioned in the previous sentence. The methods and development of diurnal composites emerges as a very clear way to visualize

and isolate the effects of precipitation. The analysis is unique and the conclusions well supported by the analysis. Overall, I have very few comments on the approach. The data treatment was conservative and not over-interpreted.

The main issue with the paper is its organization. It is very long, containing (if my count is accurate) 101 figure panels. All of the figures and analysis are certainly useful but not necessary. The shear scope of the paper, I think, makes it rather unapproachable. I would recommend, for example, removing the panels showing the diurnal cycles of standard deviations. It can simply be stated how the SD changes through the day without needing to spend so much space and discussion on this. The organization of the text also requires some consideration. The choice to merge Results and Discussions into a single (16 page) section I would recommend against. By embedding the discussion within the results it reduces the coherence and flow of the paper. I would simply report each results but strip out discussion of its significance. Then write a purely "Discussion" section which develops how the ecosystem response to precipitation events emerges from all of these analyses. The significance of the work gets lost by interweaving so much interesting discussion within the more banal description of results. Further, because the Discussion is not presented in isolation it requires Summary and Conclusions section which is too long. Thus, if the Discussion was isolated the Summary and Conclusions could be shortened to simply a paragraph.

Although my previous comments were critical of the length of the paper, it would be useful to also include a few timeseries' of fluxes during precipitation events. In other words show how the system evolves, not in a composite sense, as the forest transitions from dry to wet to dry. These figures could be included as supplemental.

If the site includes a Leaf Wetness Sensor, this also struck me as a potentially critical piece of information. There is a general lack of discussion on how the formation of dew and or occult precipitation just following a rain storm when so much excess vapor is available. The leaf wetness sensor would help shed some light on whether there is surface condensate that is lingering post storm and how this influences the latent heat

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budget.

Pg. 8941 4-5 the first sentence seems to suggest that precipitation is a disturbance akin to fires, clear cutting etc. . . I would just lead with the second sentence. 10 “processes” 13 My understanding, though I cannot think of a reference, is that rain can also displace soil air with high CO2 into the atmosphere.

Pg. 8944 15 The sentence beginning “To estimate. . .” doesn’t fit into the flow of that paragraph. This is simply a statement on the method used to characterize turbulence as opposed to discussion of precipitation processes, which is that paragraph was about.

Pg. 8947 16 “daytime,”

Pg. 8951 23 The drop in LE seems to occur when snowpack is still present this seems inconsistent with the explanation that latent heat flux drop because snow is no longer present. 26 Increased transpiration but also increased VPD, which reaches higher maximum values in the summer. 3.2.1 This section also considers temperature but the header doesn’t indicate this.

Pg. 8956 27 “mid-day, the soil”: Figures 7 and 8. I was curious about the presentation of composite CO2 mixing ratios over a 14 year period when background CO2 levels have risen substantially. This would lead to biases if, for some reason, the days were not distributed evenly across this 14 year period. I would perhaps consider normalizing the CO2 mixing ratios to the average of that given day

Pg. 8959 11-14 This sentence is redundant. The method is described elsewhere.

Pg. 8962 13 My sense is the original data from Jasechko et al., have largely been negated by a follow up paper: Schelsinger and Jasechko 2014 :”Transpiration in the global water cycle”, which brought the average T fraction closer to 60-70%

Pg. 8963 8 NEE wasn’t “reduced” but made less negative (i.e. increased). 18-21 Sentence typo in here.

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