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.

Reply to Referee #2

S. P. Burns et al.

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The thoughtful comments by Referee 2 are greatly appreciated. The comments by Referee 2 are shown in italics followed by our reply. We have enumerated the comments so they are easier to reference.

Reply to General Comments:

Comment 1: *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..*

Reply to Comment 1: We agree with the summary of the manuscript by Referee 2 and appreciate that they see the value of the analysis we have presented.

Comment 2: *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.

Reply to Comment 2: We agree with the suggestion to remove the plots of the standard deviation and have removed these panels from the revised manuscript. We carefully considered re-arranging the manuscript as suggested by the reviewer. In the end, we decided it was better to reduce the text from the results/discussion section (which we reduced by by $\approx$8%) and create new subsections that better separate the topics within the results/discussion section. Therefore, we divided Sect 3.2 into these subsections:

Sect. 3.2.1 Wind, turbulence, vertical temperature profiles, and near-ground stability
Sect. 3.2.2 Atmospheric scalars (T_a, q), soil temperature, soil moisture, and soil heat flux
Sect. 3.2.3 Atmospheric CO_2 dry mole fraction
Sect. 3.2.4 Net radiation and turbulent energy fluxes
Sect. 3.2.5 The evaporative contribution to LE
Sect. 3.2.6 Net ecosystem exchange of CO_2 (NEE)

We decided this was preferable to creating a stand-alone discussion section which would require referencing back to figures already introduced within the results section.

Comment 3: *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.*

Reply to Comment 3: This is an excellent idea and we have added an example time series of the fluxes as a supplement to the revised manuscript in Fig. S3.

Comment 4: *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 budget.*

Reply to Comment 4: This is also a very good idea. We have now included the leaf-wetness sensor data in Fig. 3 of the revised manuscript. The revised Fig. 3 is shown as Fig. R1 at the end of this document. The leaf-wetness data reveal a few interesting features that are not discernable from the precipitation data. For example: (1) for all precipitation states, the minimum in leaf-wetness occurs just after sunrise in the early-morning, (2) on a wDry day, there is a trend from a leaf wetness value of around 0.6 just past midnight to 0.2 at around sunrise (this is consistent with the canopy drying out following a wet day), and (3) in the afternoons and evening hours, the leaf wetness values were similar for dDry/wDry days (with values around 0.2–0.3) and for dWet/wWet days are similar (with values between 0.6-0.8).

Specific Comments:

Comment 5: *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.*

Reply to Comment 5: line 4-5: We agree that the first sentence about fires, clear-cutting, etc is a bit off-topic and have removed it. line 10: we changed "process" to "processes". line 13: We modified this sentence to include the possibility of rain displacing CO2-laden air from the soil pore space into the atmosphere. This issue has been discussed by several articles already cited in our manuscript (e.g., Hirano et al., 2003; Huxman et al., 2004; Ryan and Law, 2005).

Comment 6: *Pg. 8947 16 "daytime,"*

Reply to Comment 6: This text has been removed.

Comment 7: *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.*

Reply to Comment 7: line 23: The reason that there is a slight drop in LE during April and May (ie, when snow is usually present) is explained by the sentence on lines 26-27 of the discussion paper, which is "Also, winds are much stronger in winter which would promote higher evaporation." Here, we made a mistake in claiming that the winds are much stronger in "winter". The mean wind speed (similar for both daytime and nighttime) for Nov to Feb is between 6–7 m s^{-1}, however, in April and May the mean wind speed drops to around 4 m s^{-1}. To make this point more clearly we modified the text from lines 26-27, to be,

"Also, winds are much stronger between November and February which promotes higher evaporation."

line 26: We agree that VPD is also a factor and modified the sentence in question to be,

"In the spring and summer LE increased during the day from around 50 W m^{-2} to 150 W m^{-2} primarily due to increased forest transpiration, as well as increased VPD."

section 3.2.1: We considered the vertical temperature gradient as part of stability. However, we agree that we should explicitly list the air temperature in this subsection heading so we modified the heading to be,

Sect. 3.2.1 Wind, turbulence, vertical temperature profiles, and near-ground stability

Comment 8: *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.*

Reply to Comment 8: line 27: We added a comma after “mid-day”. Figures 7 and 8: We discussed the issue of how the trend of increasing CO2 might affect our results. In fact, because Fig 8d-f is relative to the top level this effectively removes the effect of any long-term trend on the results. Periods are only used when data from all levels were available, so the only way a bias could affect the composites is if the CO2 of the air near the ground was somehow changing differently with time compared to the CO2 of the above-canopy air. We do not think this is likely, so have not changed anything in the plots.

Comment 9: *Pg. 8959 11-14 This sentence is redundant. The method is described elsewhere.*

Reply to Comment 9: lines 11-14: this is where we first introduce the figures with net radiation and the fluxes to the reader. We also describe how Fig. 10 is related to Fig. 9. This does not seem to be redundant information. In the revised manuscript this text has been modified considerably, which likely makes this a moot point.

Comment 10: *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%*

Reply to Comment 10: Thanks for pointing out the paper by Schlesinger and Jasechko (2014). We included Schlesinger and Jasechko (2014) as an update to Jasechko et al. (2013) and added the following text to Sect. 3.2.5:

> In a survey of 81 different studies from around the world, Schlesinger and Jasechko (2014) found that the ratio of transpiration to evapotranspiration in temperate coniferous forests have a typical range between 50-65%. This is a large-scale estimate from the perspective of an overall water budget that does not include details such as a dependence of evapotranspiration on LAI or surface wetness (they also note that uncertainties in their estimates are large).

The discussion in Sect. 3.2.5 of the revised manuscript has been changed to reflect this new information.

Comment 11: *Pg. 8963 8 NEE wasn't "reduced" but made less negative (i.e. increased). 18-21 Sentence typo in here.*

Reply to Comment 11: line 8: Good point. We modified CO2 with, “magnitude’, so the revised sentence is:

> As one would expect, the magnitude of daytime NEE was reduced during wet conditions due to decreased photosynthetically active radiation (PAR) which is shown as a decrease in R_{net} in Fig. 9a.

lines 18-21: Thanks for pointing this out. We fixed this error.

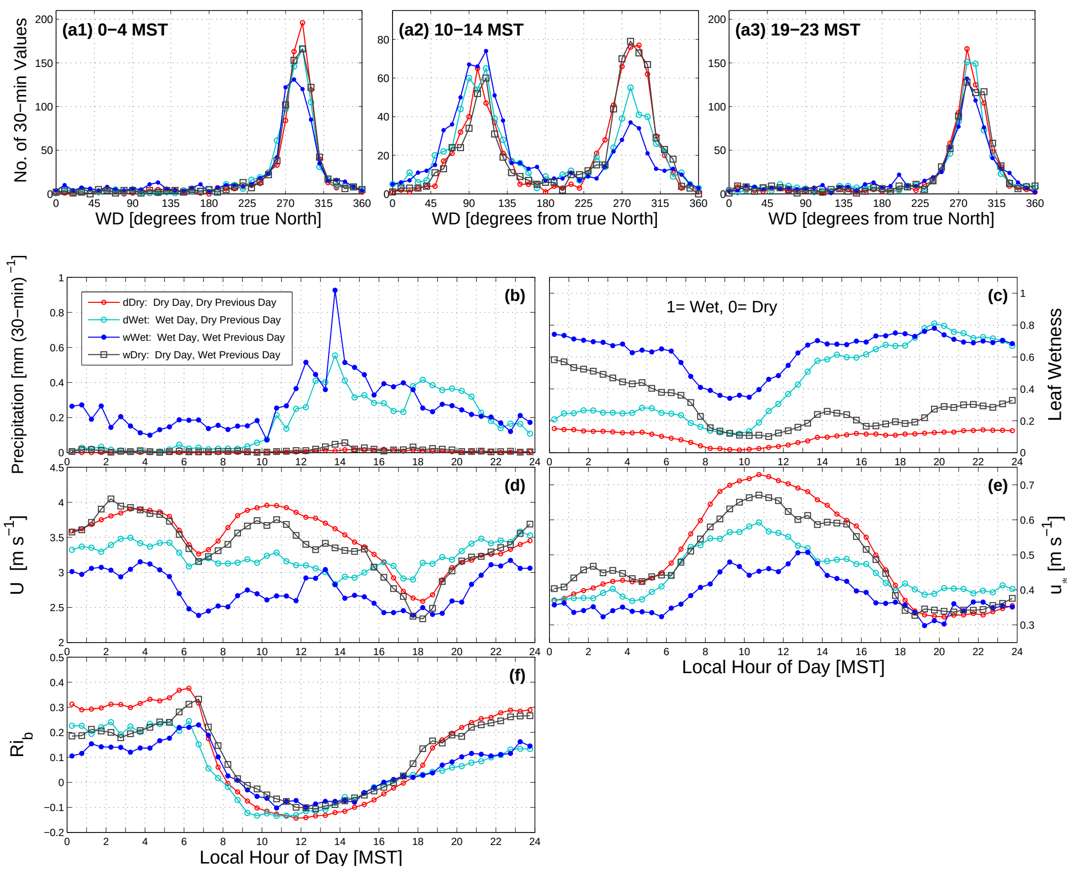

Figure R1: Frequency distributions of wind direction WD for different precipitation states for **(a1)** nighttime (00:00–04:00 MST) **(a2)** mid-day (10:00–14:00 MST), and **(a3)** late evening (19:00–23:00 MST) periods. Because there are a different number of 30 min periods within each precipitation state, the frequency distributions were created by randomly selecting 800 values for each precipitation state. Below **(a1–a3)**, the mean warm-season diel cycle of **(b)** precipitation, **(c)** leaf wetness, **(d)** horizontal wind speed U at 21.5 m, **(e)** friction velocity u_*, and **(f)** bulk Richardson number $\mathrm{Ri_b}$ are shown. These composites are from 30 min data during the warm-season between years 1999–2012. For all panels, each line represents a different precipitation state as shown in the legend of panel **(b)**. **[NOTE: This is Fig. 3 in the revised manuscript.]**

References

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