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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 #1

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"The effect of war-season precipitation on the diel cycle of the surface energy balance and carbon dioxide at a Colorado subalpine forest site" by Burns et al., investigates the modification of precipitation on the measured meteorological variables and ecosystem fluxes at diel cycle during the warm-season period at Niwot Ridge Subalpine Forest AmeriFlux Site. The manuscript is very detailed, well written, however also very long. In my opinion, it will be a very good contribution to Biogeosciences, but it definitely requires a substantial revisions before publication, especially addressing the goals and some technical details.

General comments: Burns et al. "The effect of warm-season precipitation on the diel cycle of the surface energy balance and carbon dioxide at a Colorado subalpine forest site" undertakes a worthwhile objective, but in its present form fails to deliver on that objective. There are several serious issues.

1) The goal is to evaluate the effect of precipitation events on the diel cycle of a suite of fluxes and met variables, but the analysis does not accomplish that goal.

a) Current form of nomenclature is confusing. I highly recommend changing the nomenclature. As an example, the nomenclature could be made much clearer by using the convention dD, wD, dW and wW, where lower case refers to the previous day and upper case refers to the analyzed day.

b) But I would argue that the only meaningful comparison is of dD and wD. They are meaningful because: 1) the sensors are dry and so the flux data are not infilled; and 2) they do not face the confounding effects of cloud differences – both dD and wD are mostly sunny with similar Rn. The dW and wW stratifications do little that say that wet days tend to be cloudier than dry days, with lower Rn and thus altered H and LE, which is not worth saying.

c) The paper title and many statements within make causal statements about a precipitation effect. Be careful. All the analysis does is to compare dD, wD, dW and wW days, which is much different. I am not sure what term to use, but perhaps (?) precipitation events? What you call a precipitation effect is confounded by other associated difference, including cloudiness, frontal air-mass passage, and differences in convective BL-top entrainment. The objective is NOT achieved.

d) The interesting points to make are in comparing dD and dW, looking at H versus LE partitioning and associated diel cycles in NEE. These results may be interesting. I would suggest a further stratification, with both dD and wD stratified into sunny and cloudy (but define sunny and cloudy and use more stringent criteria, e.g. sunny (daily total SWdown/SWtop-of-atmosphere > 0.6 or 0.7) and cloudy (SWd/SWtoa < 0.3 or

0.4).

2) The partitioning of ET into E and T is not convincing for either day or night.

a) The arguments that the nighttime ET is pure E and also represents daytime E may be incorrect. Surely, as you yourself say, the day-night VPD difference will cause a day-night difference in E.

b) It is equally dangerous to assume that the daytime wD versus dD difference in ET is a measure of E. Wet canopy conditions will be energy-limited, favour E over T, and suppress T relative to dry canopy conditions.

3) The use of the term frontal passages to denote your four stratifications, which becomes a major part of the Conclusions, is not warranted. A lot of the warm-season precipitation is convective and has nothing to do with airmass change.

4) Contrary to the secondary objective (L18 p.8944) and conclusions, the paper contains nothing about inter-annual variability. It simply makes use of 14-years of data.

5) The paper needs to be rewritten with much greater focus, clearer primary conclusions, and much less reporting of results that are purely descriptive but do not support the primary conclusions. I suggest that you focus on the suggestion from 1d above, and then introduce the met and state variables only as they add physical, mechanistic understanding.

Other suggestions:

1) If the REBS Q7.1 was so different than the CNR1, why was it used? It has known deficiencies.

2) Were H, LE and NEE computed to include the storage changes in the air-layer below the flux measurement? They should be, esp. for an analysis of the diel cycle from such a tall flux tower.

3) Likewise, if you have Ssoil and S canopy, why not use them? With the soil heat flux

plates so deep in a forest-floor horizon, Ssoil is large and Gz is a poor estimate of G.

Minor comments:

1. It may be beneficial to give root depth and/or soil depth in the 2.1. Site description part.

2. For ET separation into E and T, it may be good to check ecosystem specific T values reported by Schlesinger and Jaseckho (2014). Schlesinger W.H. and S. Jaseckho, 2014. Transpiration in the global cycle. Agricultural and forest Meteorology, 189-190, 115-117.

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