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 #1

S. P. Burns et al.

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Date: October 16, 2015

The comments by Referee 1 are greatly appreciated. We have listed the comments by Referee 1 below in italics, followed by our responses.

Under the category "General Comments":

Referee Comment: *"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.

Reply to Referee Comment: We thank Referee 1 for noting the positive aspects and objective of our manuscript. We will address any parts of the manuscript that "failed to deliver" our objectives in the replies to more specific comments below.

Comment 1: *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.*

Reply to Comment 1: We feel that our analysis achieved this goal by explicitly showing how the diel cycle of scalars and fluxes were affected by days with precipitation (relative to to days without precipitation). Our answers to comments 1a–c are provided below.

Comment 1a: *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.*

Reply to Comment 1a: This is an excellent idea. We took this idea one step forward and included the full word “Dry” and “Wet” for the current day. So our categories are: dDry, dWet, wWet, and wDry. We have modified the text and figures to use this nomenclature.

Comment 1b: *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.*

Reply to Comment 1b: Though we agree that rain does affect the sensors, we don't fully agree with this statement. First, the sensors will work when it is raining lightly so it is only periods with heavy-rain which are gap-filled. The amount of gap-filled data is shown in Table 2 and even in wWet conditions this only accounts for roughly 30-40% of the time periods. While we agree this is far from perfect (and make a note in the text that our results should be considered with this in mind), we feel that gap-filling is the current “state of the measurement” so it's useful to show these results. We leave it up to the reader to decide if these results are truly valid or not. If gap-filling during heavy precipitation is not used, then every paper that analyzes fluxes at an annual time-scale would also be considered problematic and/or invalid.

With regard to wet days being cloudier than dry days: the important result we have presented is not that H and LE are altered due to cloudiness, it's that the surface energy balance was roughly the same for all precipitation conditions as shown in Fig. 13. This means that even though the radiant energy was reduced on wet days, the turbulent fluxes were responding in an appropriate manner.

Comment 1c: *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.*

Reply to Comment 1c: This is a good point and we completely agree that precipitation and other environmental variables are co-dependent. Any study of the natural world needs to deal with this issue. We made a statement in the conclusions (at the bottom of p. 8969 in the discussion paper) that, we think, addresses this issue. The statement is:

> Our study has provided an example of one way to look at the complex interconnections between variables that make modeling ecosystems so challenging...[text not shown]...We have shown that precipitation is intrinsically linked to changes in air temperature, pressure, and atmospheric humidity.

We have presented our results as one way to look at how precipitation changes the fluxes and surface energy balance. It is surely not the only way to look at precipitation effects. When we analyzed the data based on precipitation state we were not necessarily expecting the other variables (such as air temperature) to follow the pattern of precipitation (as shown in Fig. 6). In hindsight, this makes perfect sense because it tends to rain on cooler days. In order to soften any statements that our study shows a direct effect of precipitation we replaced the word “effect” in the title with “influence”, so the title of the revised manuscript is, “The influence of warm-season precipitation on the diel cycle of the surface energy balance and carbon dioxide at a Colorado subalpine forest site”. The comment about “causal statements” within the text is a good one. Within the text we have tried to use the term “precipitation state” to refer to how variables were changed on a particular type of day (i.e., a dDry versus wDry day).

Comment 1d: *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).*

Reply to Comment 1d: This type of analysis was done for Dry1 (dDry) days. It seems the reviewer wants something similar done for Dry2 (wDry) days? This is a good idea, but then the study becomes focused on the effect of clouds (not on precipitation). Though clouds and precipitation are certainly related to each other, it is our preference to keep a focus on precipitation so we did not follow the advice of the reviewer and pursue this comparison (at least not for this paper). Also, we are trying to shorten the manuscript, so if we were to add this extra analysis it would make the manuscript even longer (opposite of our intention).

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

Reply to Comment 2: Our replies are below. In the revised manuscript, we have created a subsection that specifically addresses the partitioning of ET, Sect. 3.2.5, “The evaporative contribution to LE”.

Comment 2a: *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.*

Reply to Comment 2a: We found that when conditions were dry, there was very little dependence of LE on VPD. For example, compare dDry and wDry days versus VPD in Fig. 11a3; LE from both dDry and wDry days are close to each other and show little VPD dependence (the same is true for dWet and dDry days in Fig. 11a1). Since there is reduced liquid water present in the soil, the soil resistance to evaporation is probably controlling evaporation more than any effect due to VPD differences. In Sect. 3.2.5, we clearly state that we have assumed daytime evaporation is similar to nighttime LE in dry conditions. We have also provided evidence why we think this assumption is true. If the reviewer has a specific reference which shows that soil evaporation in dry conditions has a large VPD-dependence, we would be willing to re-consider this assumption.

Comment 2b: *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.*

Reply to Comment 2b: To address this question we thought it would be extremely useful to add transpiration measurements to our analysis. As a result, we invited Jia Hu to join as a co-author and include her transpiration data collected during the summers of 2004, 2006 and 2007. Though sampled over a much shorter period than the fluxes, we added the transpiration data to Fig. 9 in the revised manuscript. These data give us an idea that mid-day transpiration was similar in both wDry and dDry conditions (what is shown in Fig. 9 is for pine trees, but spruce trees show even closer agreement in T between wDry and dDry conditions). Since T and Rnet were similar in dDry and wDry conditions, this means the increase in LE is due primarily to increased evaporation. We have quantified this difference and explained our assumptions in Sect. 3.2.5. We also revised our nomenclature to make the point that wDry days are not necessarily with a fully wet canopy, but instead these are conditions where the forest is transitioning from wet to dry and has a mostly dry canopy (based on leaf-wetness data) with a relatively high amount of liquid water in the soil (which provides an evaporative source).

Comment 3: *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.*

Reply to Comment 3: We explained our use of the term "frontal passage" in section 3.2.2 of the discussion paper with the following text:

> Classical cold-front systems over flat terrain are associated with pre-frontal wind shifts and pressure troughs (e.g., Schultz, 2005). Mountains, however, have a large impact on the movement of air masses and can considerably alter the classical description of frontal passages (e.g., Egger and Hoinka, 1992; Whiteman, 2000). Our classification of the composite plots as a "frontal passage" is simply because there was colder air present at the site during the Wet1 and Wet2 periods.

While we agree that a significant percentage of the precipitation events at the site are convective in nature, we found that during periods with two days in a row of *above-average* precipitation three things occurred: (1) there was a significant drop in the air temperature (see Figs. 5 and 6), (2) barometric pressure was lower, and (3) the mean CO2 of the atmosphere was distinctly different (see Fig. 7a). These factors taken together led us to the conclusion that a different air mass was present at the site on wWet days. It makes perfect sense that when above-average precipitation occurs on consecutive days this is not a "normal" event and due to a large-scale weather system. The key here is that we are classifying "wet" days as precipitation that is close to the average precipitation for the site. So most small convective storms are excluded from the wet-day classification. We have left this description as-is for now, but are open to more discussion about it if the referee can provide additional details or evidence that the wWet days are not a frontal passage.

Comment 4: *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.*

Reply to Comment 4: The interannual variability of NEE, LE, and H are shown in Fig. 2 (right-hand panels) and discussed in Sect. 3.1 of the discussion paper.

Comment 5: *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.*

Reply to Comment 5: We have made modifications to the text that attempt to focus the results more clearly. As part of this effort, we redefined the subsections in Sect. 3.2. We feel that the suggestion in 1d above leads to a study of clouds and not precipitation. Our goal is to broadly show how precipitation affected many of the measurements at the site (not only the fluxes). A future study that focuses more on the mechanistic effects of precipitation (and includes a modeling aspect) is being considered for a future study.

Under the category "Other suggestions":

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

Reply to Comment 1: The disadvantage of using the CNR1 for our study is that in summers of 1999, 2004, and 2005 there was no CNR1 on the US-NR1 tower. Furthermore, the CNR1 sensor used prior to 2005 appears to have a much larger value of outgoing shortwave radiation than those from the CNR1 sensor installed in late 2005. Therefore, we would need to either reduce the amount of data in our analysis or come up with an ad-hoc correction for the Q-7.1 R_{net} data. For simplicity, we opted to use the Q-7.1 sensor in our analysis.

In Figure C1 below we compare the changes to the energy balance if we use the REBS Q-7.1 sensor (top row) or the CNR1 sensor (middle row). There is almost no change during the daytime and a small change at night (with REB Q-7.1 leading to a SEB that is slightly closer to 1). We felt that the comparison between the Q-7.1 and CNR1 has already been discussed within the literature (e.g., Turnipseed et al. (2002), see their p. 183 and pp. 189-190; and Burns et al. (2012), see their Fig. 6) and re-hashing this comparison would detract from the main message of the paper (i.e., precipitation effects). The main conclusion from these previous studies is that the CRN1/Q-7.1 differences are primarily due to longwave radiation. During the daytime, the longwave radiation component of R_{net} is a small percentage of R_{net} so any effect on the SEB is small. At night, however, longwave radiation dominates R_{net}, and the sensor difference are more important.

For completeness, we have included a comparison between the Q-7.1 and CNR1 sensors in Fig. C2 and a short summary here:

- The mean difference is between 5-20 W m^{-2} over the diel cycle (Q-7.1 $>$ CNR1). This difference is slightly smaller in the afternoon and larger during the morning transition which suggests one sensor might be slightly tilted relative to the other.

- The standard deviation of the difference is fairly constant at night with a value of around 14 W m^{-2}.
- The Q-7.1 sensor was found to be closer to closing the surface energy balance (e.g., Turnipseed et al., 2002). This does not imply that the Q-7.1 is correct. Further study is probably needed to establish the reason for this difference.

Comment 2: *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.*

Reply to Comment 2: By definition, NEE includes the CO_2 storage term below the flux-measurement level. The storage terms for H and LE are rather small so they were not included in the original analysis. However, in the revised manuscript, we have now included all the associated storage terms as suggested in the next comment.

Comment 3: *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.*

Reply to Comment 3: This is an excellent idea. We originally thought that including the storage terms would add too much extra information to the manuscript, but we agree with the referee that this should be done. Though these terms are not large, they have a significant effect on the energy balance and we have now included them. We show the effect of including the storage terms on the SEB in Figure C1 below (compare the top and bottom row). Interestingly, inclusion of the storage terms pushes the SEB closer to 1 during the daytime, but makes it further from 1 at night. In order to keep the length of the manuscript reasonable, we added the description of the storage terms to the appendix. We have listed several possible reasons for lack of SEB closure and possible improvements to the SEB calculations in the conclusions of the revised manuscript.

Under the category "Minor Comments":

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

Reply to Comment 1: The root depth is not something we explicitly measured, but visual inspection of fallen trees suggest that rooting depth is in the range of 40-100 cm. We added the following text to the site description:

> Empirical evidence from windthrown trees suggest rooting depths of 40-100 cm which is consistent with depths from similar subalpine forests (e.g., Alexander, 1987) and as discussed in Hu et al. (2010a).

Comment 2: *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.*

Reply to Comment 2: 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.

References

Alexander, R. R.: Ecology, Silviculture, and Management of the Engelmann Spruce - Subalpine Fir Type in the Central and Southern Rocky Mountains, USDA Forest Service, Agriculture Handbook No. 659, 144 pp., 1987.

Burns, S. P., Horst, T. W., Blanken, P. D., and Monson, R. K.: Using sonic anemometer temperature to measure sensible heat flux in strong winds, Atmospheric Measurement Techniques Discussions, 5, 447–469, doi:10.5194/amtd-5-447-2012, URL http://www.atmos-meas-tech-discuss.net/5/447/2012/, 2012.

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Jasechko, S., Sharp, Z. D., Gibson, J. J., Birks, S. J., Yi, Y., and Fawcett, P. J.: Terrestrial water fluxes dominated by transpiration, Nature, 496, 347–351, 2013.

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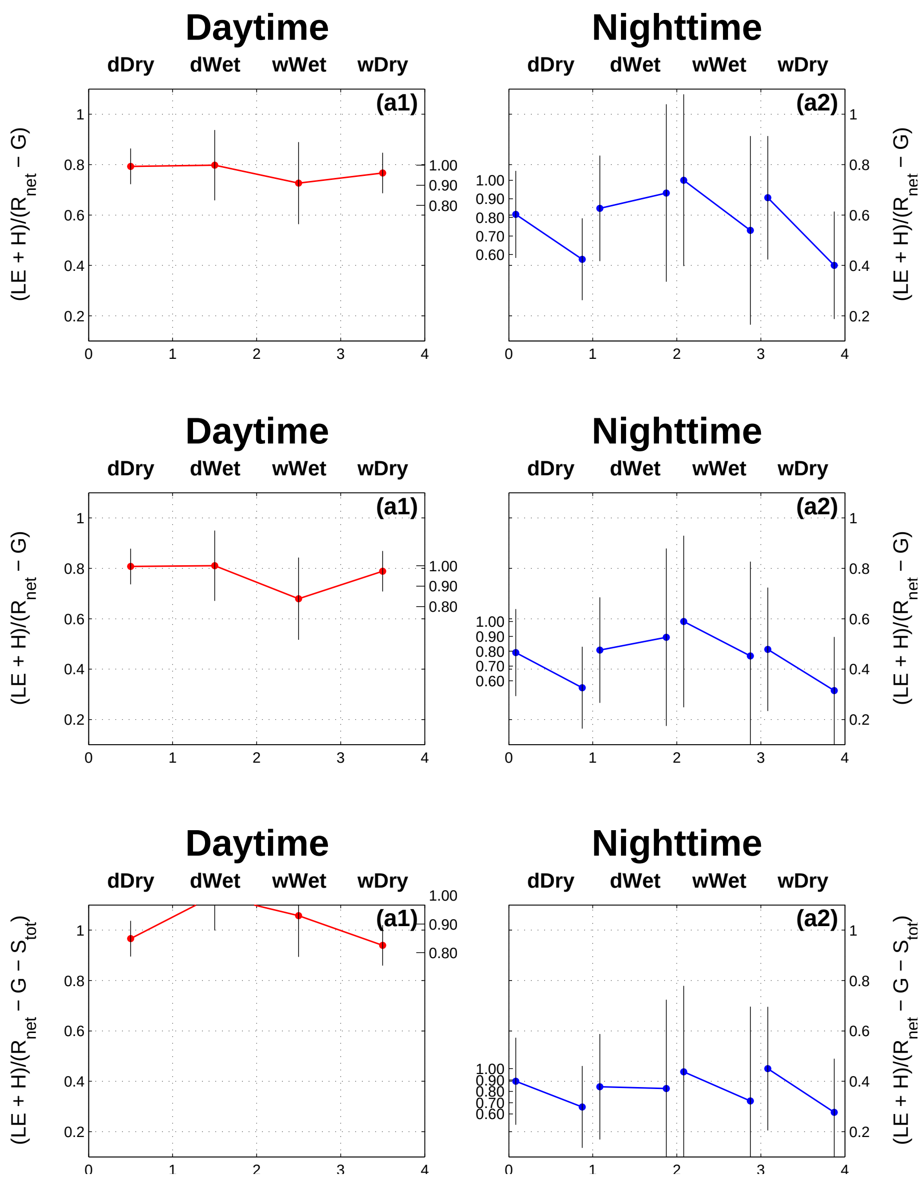

Figure C1: Similar to Fig. 13 a1-a2 in the discussion manuscript. (Top row) using REBS Q-7.1 for Rnet; (middle row) using CNR1 for Rnet; (bottom row) using REBS and including the storage terms. Note: the bottom row assumes dry conditions for the soil properties so it is slightly different than what is shown in Fig. 13 in the revised manuscript.

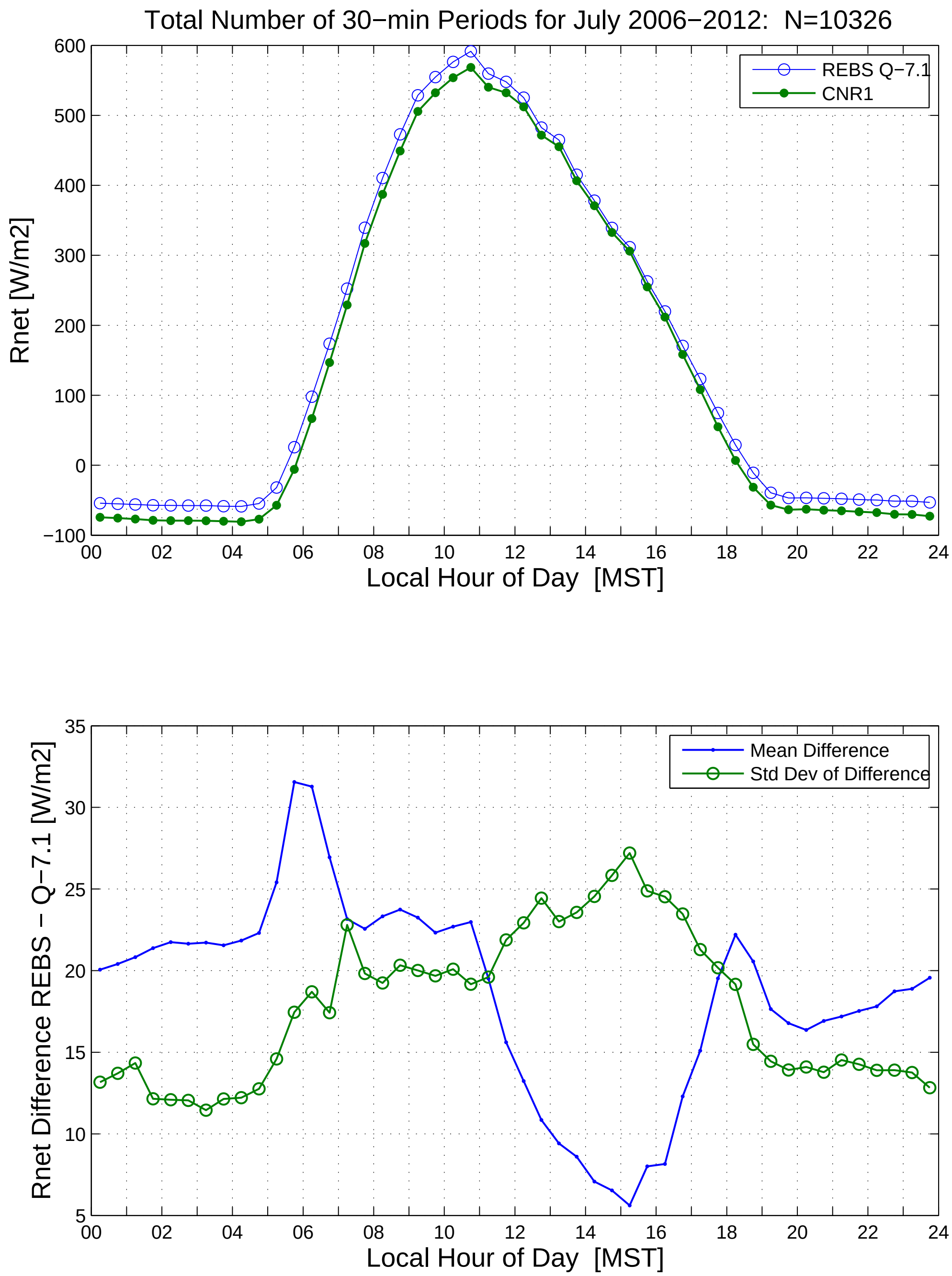

Figure C2: The six-year (top) mean and (bottom) difference statistics for $R_{\rm net}$ in July for the Q-7.1 and CNR1 sensors at the US-NR1 tower.