The manuscript entitled “Estimating immediate post-fire carbon fluxes using the eddy-covariance technique” presents a unique data set of carbon-, water-, and energy fluxes measurements 43 days after a wildfire in Portugal. The ecosystem is/was a Maritime Pine with some Eucalyptus stands inside which were mainly burned and only the trunks of the trees remained. The authors explain broadly their data quality control scheme how data were filtered, selected and gap filled. Based on the data they represent cumulative fluxes of NEE GPP and Reco. Additionally, they focus based on one event on the interactions of dew and ashes with respect to carbon dioxide fluxes.

Overall the manuscript is well written, but it could need some polishing on the figures as well on the text where sometimes method parts are in the results and discussion parts are in the results. I think this should be cleaned up. Overall, the manuscript is worth to publish especially because we don’t have many studies presenting data from ecosystems shortly after a fire disturbance and its recovery.

My main concern with the manuscript is that the authors interpret a lot into the eddy covariance data without having the right measurements to back it up. See my comments for details. Further, I think the u*-threshold estimation and removal of data with low u* values is nothing to debate about as the eddy covariance technique is not working under those conditions and this must be accounted for in the data processing.

Major comments:

I would highly recommend a standardized data processing for the gap filling and flux partitioning. The REddyProc (Wutzler et al., 2018) package is easy to use and has all needed functions to do the u*-threshold estimation, subsequent gap-filling, and finally the flux partitioning of NEE into Reco and GPP. I think the argument that in this specific case the standardized methods are not working does not hold. In this context I would like to question the QC scheme used here. All fluxes between QC 1 and QC 8 (based on the Foken 1-9 system) are used but here we are already quite far from the assumptions which allow us to apply the eddy covariance technique. I would suggest to go for QC 1-6 to maintain reliable data.

Within the manuscript for different analysis different QC schemes are used which is very confusing. Sometimes wind sectors are removed then again, they are not. In one instance QC 1-6 is used in another QC 1-9 this is confusing for the reader because it jumps back and forth.

The uptake in burned area after the fire is either a measurement artefact or it comes from vegetation that is still taking up carbon. Maybe arising from imperfect footprint estimates? Or a too tall canopy height which will reduce the footprint size and thus suggest that footprints are smaller than they are in real (minor comments below)? Which in turn would reduce the number of footprints having 80% of Maritime Pine contribution. In the east there is the eucalyptus patch close to the tower. Not much is mentioned about it but it seems to be the second most important wind direction and thus potentially influencing the measurements?

The data set associated to this discussion paper is only containing daily data for NEE, GPP and Reco. But most analysis are based on half hourly data. I guess it would be nice to also have those data available in the dataset. But this depends on the journal / editors decision.

Minor comments

L42-43 not fully clear if the ecosystem is still a source on an annual basis or considering the totally emitted carbon vs. the total carbon sequestered by the ecosystem during the following 10 years.

L57 maybe adding a city or village would make it easier to find.

L78-79 How can a median value have a range (4.5 -6.7m)?

Figure 1 Include the slope angle map from the supplementary figure 1 and also add a map with the footprint climatology. Also add the points for the fire severity sampling into the map.

L100 Based on what was the zero-plane displacement set to 3.8m and the canopy height to 8m if the median height only goes up to 6.7 m? This seems wrong.

Table 1 I assume the wind vector components at 20Hz were likewise sampled with the CSAT 3 correct? If so please insert that for "Sensor" instead of sonic anemometer.

Table 1 The EC5 Sensors are not mentioned in the table but should be included.

L129 – 131 What percentage of the half hourly footprint contribution were used to determine the contribution of the pine trees (e.g. 80%, 90%, 95%)?

L38 the supplementary material is not ordered as they appear in the text. This should be Figure S2. Please change accordingly throughout the manuscript and supplementary material accordingly.

L150-152 The night time estimation is very coarse and might create huge biases especially during the winter period when sunrise and sunset are clearly later and earlier, respectively. I would recommend to use the potential radiation to estimate day- and nighttime. This is for example included in the REddyProc package that can also take care of the gap-filling as well as flux partitioning including u*-threshold estimation.

L157 I think "initial slope" is better than "linear slope" given that the function is not linear.

L158-159 I would argue that α and Qc,sat are no constant but variable based on environmental conditions which are not just a function of air temperature but also of vapor pressure deficit (VPD), soil water content (SWC) and status of the vegetation. Qc,sat will increase based on the leaf area index (LAI). Was this procedure done for individual periods or windows e.g. 4 weeks? This would be important to clarify.

L168-169 what were the upper and lower threshold for the mean vertical wind speed?

L177 Why was not 10 and 20cm used and instead the average? Further your first sensor is at -2.5 cm so the integral should be from -0.025 to -0.15 (equation 3). From Table one it seems that only the sensors 1.5. to the tower were used but not the whole transect. Is this correct? If so I would suggest to also use the other profiles from the transect to get a better spatially representative average soil heat flux.

L186 not clear what inter-patch means. I assume the sensors from the transect. Please indicate this also in table 1 and in lines 107-108. Which sensors were used for what? It is not consistent because the deeper VWC sensors were also used for estimating the soil heat flux.

L209 what is the value of q for your data set?

L230-231 If all data from that wind direction (30 – 90 degree) are flagged and removed this should be included in the schemes for data treatment. Also, it would be helpful if Figure 2 would be adjusted in a way that the 30° sectors fit to the bins of figure 6.

Figure S8 more details on the individual symbols and numbers could be needed. I think total number of removed data is quite low given all quality criteria mentioned. If I see that correctly there are about 10-15 % of all data missing to their wind direction (30 – 90°), further there are 40 % of data

with not enough Maritime Pine coverage in the footprint area. But from Figure S8 it looks like only 1727 data points were removed and gap filled.

Figure S4 the different grey colors are hard to distinguish. Would be nice to add different symbols to the individual lines. Additionally, this figure shows nicely how wrong a fixed period between 10pm and 4 am is for night time estimates. Even around end of June the sunrise is just around 6am UTC when considering the increase in H and net radiation. The same is true for the nighttime.

L245-248 What about heat storage in the burned trunks of the trees? This is something that is not considered or discussed here. The half hourly fluxes are missing the storage term of heat but this is not discussed. Additionally, the trunks certainly contribute to the sensible heat fluxes and increase the surface area compared to the bare soil. Further it is not clear if these trees are also fully in the field of view of the net radiometer. Especially in the morning at high solar zenith angle the trees will absorb a lot of the short-wave incoming radiation, heat up and contribute to the sensible heat flux. This would happen in the morning and the afternoon. Something for the discussion I guess.

Figure S5 The linear equation is very small and hard to read including the r^2 values. As you did not provide an offset value in the formula I assume you forced the intercept to be 0? Please state that explicitly if it is the case. If the detection limit of 10 W m-2 was used then this should be an absolute value. During nights with wind you might get negative sensible heat fluxes which can be larger than -10 and the same is true for latent heat during those nights just that it will in most cases not be negative. Also mention that there is the confidence interval of the linear fit included as the grey shaded area. The number of points going into the linear relation estimate should also be mentioned.

L257-259 how do we now that net radiation was underestimated? Due to the tilting? Is there a hysteresis in the net radiation and we do know that there is overestimation in the afternoon and underestimation in the morning caused by the tilting toward SW (L253)? This is not clear at all. This tilting would overestimate the incoming components in the afternoon and underestimate them in the morning which could then lead to the observed pattern. Is that the reasoning? How do we know there is an overestimation in the long wave outgoing? What evidence do we have for that? Many of these thoughts belong in the discussion and not the results I would say.

L264-270 this is all discussion and no results. And even for the discussion I think this is not really needed. Your manuscript is not about the problems of energy balance closure under post-fire conditions but to characterize ecosystem fluxes. I find this whole hypothetical discussion on where the non-closure is quite distracting from the main objective. And we all know that the energy balance is not closed at any site. With you lack of 10% you are actually closer than most sites around the globe.

L273-276 I don't agree at all with statement. I don't see why the development of vegetation is a reason not to use standard gap-filling procedure? There are many grasslands in the Mediterranean or other dry areas which senesce during the summer and regreen during the autumn and winter. It is the same as happening here. And all of these sites in FLUXNET use the standard gap filling and flux partitioning schemes similar to the one used in REddyProc. So this is not a valid reason. The second point is somewhat true because moving windows smear the signal when rapid changes are happening e.g. the re-wetting of the ecosystem after the summer. Still if you are interested in the annual sums which also seems to be a point of the manuscript I would highly suggest to use standard gap-filling and flux partitioning.

L289 E0 "0" must be subscript

L300-305 Formula 6 is not fully explained. What is T in this formula? Is it the surface temperature as assumed by the Stefan-Boltzman law? How was this then estimated or is it the air temperature. The reasoning and how this was derived should be explained in the methods and not in the results. I would recommend to move this part to methods section. And at the end of line 305 a point is missing.

3.2.3."Generating the final data set" this part is not consistent with what is written in the methods and what is shown in the schematics. Here night time is defined different as compared to the methods part (L150). Here nothing is mentioned about the filter of wind direction or the footprint filter. This is confusing.

L336 first time that the storage term is mentioned. This should be made clear in the methods part. And it should be mentioned if it was the 1-point storage correction or based from a multiple point vertical profile system.

Figures S9 and S10 it is very hard to associate the events across variable of the plots. First the horizontal zero lines should be included. Secondly the areas with dew should be shaded vertically so the points can be clearly associated and compared across the different variables.

To me it looks like we see a flushing of CO2 in the morning that accumulated during the night at the ground. If the 1-point storage correction was used this one will for sure not capture what is happening at the ground. How was the friction velocity during the night? This could also tell a bit about the storage of CO2 at the ground. If the moment of the high CO2 flux goes together with an increase in u* then we can be quite sure that this was rather a flush of CO2 than CO2 originating from instantaneous microbial activity or water flowing into pores and flushing out CO2. Also the argument of the negative sensible heat flux is not very convincing. Dew evaporation needs energy but we must separate the individual processes. As long as we have a negative net radiation and we reached already 100% relative humidity we will observe more dewfall which means a negative latent heat flux (this was actually not shown in figure 8). Only when energy is provided in form of incoming short-wave radiation we can have conditions of evaporation. But then the energy for evaporation is coming from the incoming short-wave radiation which means sensible heat is not increasing fast but slow. The negative sensible heat flux looks to me like an inversion of cold air at the surface was breaking up in the morning and transporting cold air upwards together with the CO2. I think here we are missing many data streams to bring the story together.

Figure S12 and S13 here it seems very clear that the burned are S13 is more heterogeneous as expected. Else the fluxes would be more homogeneous and not scatter so much. This indicates that there are patches which take up carbon. Maybe a better representation would be a plot of midday NEE or CO2 flux vs wind direction. I think this would make it clearer and highlight the differences between the burned and the other areas and maybe also give you more trust in the differences you see. Maybe I would even do it for every month/ season but this is just a suggestion.

Wutzler, T., Lucas-Moffat, A., Migliavacca, M., Knauer, J., Sickel, K., Šigut, L., Menzer, O., Reichstein, M., 2018. Basic and extensible post-processing of eddy covariance flux data with REddyProc. Biogeosciences 15, 5015–5030. https://doi.org/10.5194/bg-15-5015-2018