Authors' discussion of the comments **bg_2020-312-RC2**

The authors would like to thank Reviewer #2 for his helpful comments. By taking them into consideration, the authors think that they will also clarify some of the misunderstandings of Reviewer #1.

To address the comments in the document *bg_2020-312-RC2*, the authors first copy the exact comment by the reviewer, add numbers to order each comment and make cross-referencing easier, and format in grey and Italic. The answers given by the authors are in black after each comment. In the end of the document, a list of References that support the answers was added.

General comments

The paper by Oliveira et al., provides exceptional and very valuable information about the CO_2 flux behavior immediately after a wildfire, and therefore I encourage its publication. However, the paper, in its present form is difficult to follow and should be re-structured before its publication. Results section 3.1 and 3.2 should be moved to methodology. And the section 3.3 about results should not have references. References (and its arguments) should be moved to the discussion section. See my specific comments below.

In fact, it is not easy to decide whether sections 3.1 and 3.2 should be assigned to the "methodology" section or to the "results" section, since some steps in the data processing required special analyses beyond what is standard practice. In the end, however, the authors agree to follow the arguments of Reviewer #2 to give the manuscript a clearer structure. Consequently, some results in Section 3.3 related to other sources will be included in the discussion.

The objective mentioned at the end of the introduction section is not a real objective. The "in-depth analysis of the obtained EC data" is the way (the method) to analyze the behavior of CO_2 and water vapor fluxes and its dominant factors immediately after a wildfire (your objective). Another objective of your paper could be the optimization of the quality tests for EC data to correctly interpret the obtained fluxes immediately after a wildfire.

The authors agree with revising the formulation of the objectives of the manuscript, also with regard to the notes of Reviewer #1, to emphasize the efforts for an adequate data analysis under the conditions immediately after a wildfire. The authors will take these comments of the reviewers into account in the revised version of the manuscript.

Specific comments

1. *Table 1. I think there is a mistake about the frequency of sample for net radiation. It should be 0.02Hz. This info is correctly written in the above paragraph.*

The net radiation was sampled at 20 Hz. Due to limitations in the data loggers' channels, the net radiometer had to be connected to a "fast" channel so that averaging was carried out in the same way as for "slow" data. The authors will clarify this in the final version of the manuscript.

2. *Ln 139-140 "For the cumulative fluxes over the first post-fire year, all EC data with quality classes 1-8 were combined with gap-filled data"*
   What about the footprint area? Did you also selected footprint areas that consisted for more than 80 % of the Maritime Pine stands?

The authors will clarify the text with a short remark at this point. The complete explanation is given in Section 3.3.3.

3. Section 2.3.1. To include the % of missing half-hourly flux data due to measurement failures or rejection after the data quality check could be a very interesting information. This info can be divided into daytime and nighttime data.

All relevant information is contained in Supplementary material Figure S8 but the authors will insert a percentage value in the manuscript.

4. Ln 109, A parenthesis after (table 2 is missing. I would located sections 2.3.6 and 2.3.7 before section 2.3.4 because they are also related to EC measurements.

The authors thank the reviewer for the correction regarding the parenthesis. Following the re-structuring proposed and acknowledged in the general comments, Section 2 of the final manuscript will be reorganized.

5. Ln 223 "The test was carried out with 12,011 30-minute records that" There is something wrong in the numbers.

The authors do not perceive what could be wrong here. A total of 17 760 records were available. For the mechanical turbulence test, 12 011 records with nearly neutral stratification were used, as specified in the sentence:

L223-L225: The test was carried out with 12,011 30-minute records that were selected for conditions of neutral stratification (-0.2 < z/L < 0.1) and data quality classes 1-8 (i.e. without footprint 225 selection).

This data selection is also shown in Figure 2 in the authors' answer to Reviewer # 1.

6. Section 3.1 "Additional data quality test" should be section 2.4.
7. Section 3.1.2: Since the objective of this study is not to investigate the closure of the energy balance, I would recommend to remove this subsection and to include a sentence in section 2.3.1 with the % of the gap in the energy balance closure (that is in the range reported by most EC sites). If the authors consider that part of this subsection must appear in the manuscript, just move it into discussion section (4.1 data analisys).
8. The first paragraph for section 3.2.1 is "methodology" not results. Please, move this paragraph to section 2.3.2. What is more, the second paragraph is mostly discussion. Figure 7 is a result, but should be better explained in the text in order to show its relevance.
9. Section 3.2.2 is again "methodology" not results. Please, move this paragraph to section 2.3.2. Again, despite table 3 is a result, should be better explained in the text in order to show its relevance.
10. Section 3.2.3 should be also moved to "methodology" section.

Please see answer to general comments. The authors appreciate the specific comments 6-10 and will take these in due consideration when re-structuring the manuscript.

11. Ln 336 Please include information in the methodology section about the storage term.

The authors will include the information about the storage term in Section 2.

12. I would recommend to move the figure S10 into section 3.3.1. The inclusion of Figure S10 (maybe it is not necessary to include the four days) would help to the lector to better understand the Figure 8. I would

also improve the Figure S10 (and next) including the 0 Y line for NEE and the time in the X axis. The period showed in Figure 8 can be shadow in Figure S10.

The authors agree with inserting in Figures S9 and S10 a shadow band for the 4 hours shown in Figure 8. The width is then about 6 mm. Due to the time resolution of Figures S9 and S10, very little will be visible, except for the daily cycle. The authors will show the Figures S9 and S10 in the supplement. Also, the zero line will be added

13. *The measured CO2 uptake in September and October 2017 should be due to the presence of plant cover in the studied area. Do you have some pictures to test it? Otherwise, you should provide another explanation, for the CO2 uptake in September and October 2017 (Eucalipts?).*

The authors discussed this question in detail in the answer to Reviewer #1, and also provided a orto-photomap showing the pine stands with scorched crowns and the 4 individual eucalypts. The authors will convert this discussion into additional text for the manuscript, especially elaborating on which of the possible explanations that were originally postulated (uptake by dying pine crowns or by resprouting plants, especially eucalypt) is most likely.

14. *Section 4.1. Just curiosity..., did you try to compare the cumulative NEE using your procedure for rejecting data and filling gaps and with the "standard procedure" available in https://www.bgc-jena.mpg.de/bgi/index.php/Services/REddyProcWeb?*

The processing of the measured data was carried out according to international standards (Aubinet et al. 2012) with a software package that has been compared internationally several times (Fratini and Mauder 2014). The differences in the definition of the criteria for gap-filling (data quality or u* criterion) were shown in the literature (Ruppert et al. 2006). The application of the u* criterion would result in almost 50 % of the data (instead of 10 %) having to be replaced by modelling. Therefore, there is no need to compare our results with the software package that is "standard" at the Max Planck Institute Jena (Wutzler et al. 2018), as suggested by Reviewer #1. What is certainly recommended is to compare with the software routines used in different international programs, which are standard there, just like the author (TF) did 15 years ago (Mauder et al. 2008). For such a comparison, however, the existing data set is not very suitable, since in addition to biogenic processes, chemical processes linked to the presence of wildfire ashes must also be taken into account while the observed flows were generally very small and near the detection limit.

References:

Aubinet M, Vesala T and Papale D (eds) (2012) Eddy Covariance: A Practical Guide to Measurement and Data Analysis. Springer, Dordrecht, Heidelberg, London, New York, 438 pp.

Fratini G and Mauder M (2014) Towards a consistent eddy-covariance processing: an intercomparison of EddyPro and TK3. Atmos Meas Techn. 7:2273-2281. https://doi.org/10.5194/amt-7-2273-2014

Mauder M, Foken T, Clement R, Elbers J, Eugster W, Grünwald T, Heusinkveld B and Kolle O (2008) Quality control of CarboEurope flux data - Part 2: Inter-comparison of eddy-covariance software. Biogeoscience. 5:451-462. https://doi.org/10.5194/bg-5-451-2008

Ruppert J, Mauder M, Thomas C and Lüers J (2006) Innovative gap-filling strategy for annual sums of CO_2 net ecosystem exchange. Agric For Meteorol. 138:5-18. https://doi.org/10.1016/j.agrformet.2006.03.003

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