

Interactive comment on “Multiple quality tests for analysing CO₂ fluxes in a beech temperate forest” by B. Longdoz et al.

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Referee Comment: The paper presents a combination of existing assessment tests applied to eddy covariance measurements, giving a contribute to the discussion about the quality control and pre-processing of datasets acquired with this technique. Although the topic is important and the paper interesting, a number of issues should be better explained, analyzed and discussed before publication in Biogeosciences.

SPECIFIC COMMENTS 1)The paper presents the quality tests but there are not evidences in the results that the tests improved the quality of the dataset since the equation parameterization is not really affected and the energy balance closure is not presented. I agree that it is difficult to prove the quality improvement and that the tests are based on theoretical and published aspects, but then the paper should be focussed more on the effect of the quality test rather than on the tests themselves.

Authors Answer: The MS has been modified and in the results presented in the new version 64 lines (Section 3.1 and 3.2) are devoted to the tests and 120 lines (Section 3.3, 3.4 and 3.5) on the impact of the test on GPP, Reco and NEE.

R.C.: 2)the comparison is between two dataset: one with all the quality test applied and the other without any test. It would be more interesting to see the effect of each single test.

A.A.: It would give a too long analysis for one paper but it's true that it would be interesting to present separately the study of the impact of each important test (not the ones that flag only few half-hours)

R.C.: 3)P4202 L10-22: it is not clear if the threshold value has been set by visual inspection or automatically. I think it is important to propose objective methods where threshold values are selected automatically particularly if, as proposed by the authors, the tests should be applied to large datasets.

A.A.: The threshold values for the different test have been set first by interpretation of frequency diagrams (anomalies appear more clearly in these diagrams) but a visual verification of the data close to the threshold is, at the end, necessary to be sure that any problem remains. After the threshold set up the flag procedure is automatic. The part of the section 2.3 about this procedure has been adapted and includes the element given in this answer.

R.C.: 4)P4205 L3: it is not clear what the authors mean with "significantly". This should be better described and explained so that the reader can reproduce the method.

A.A.: The threshold value for the probability to define a difference as significant is now given in the text and a link to the statistical analysis section has been added.

R.C.: 5)PAR 2.5: The paragraph if generally not clear. In addition I think it is not really necessary to have a separate paragraph if the methods are explained in the text.

A.A.: The paragraph has been rewritten to be clearer. We have keep a paragraph on

statistical analysis because the MS reading is more difficult when all the information about statistic procedures is spread and sometimes repeat in different places.

R.C.: 6)TABLE 3: the authors should explain why there is a large differences in the R2 between 5 and 10 cm only in the DSIFR dataset (0.0554 vs 0.297).

A.A.: This is explain in the beginning of Sect. 3.2.

R.C.: 7)FIGURE 2: the interpolation with the line has no sense. Also the variability in Reco based on one night is not significant. It would be better to have a plot based on more nights, e.g. using the differences between successive measurements.

A.A.: The interpolation line has been suppressed. The authors prefer to present the variability in Reco based on one night because it's these high frequency fluctuations that are not well explained until now and one of the objectives of the tests presented in this MS is to remove these fluctuations.

R.C.: 8)P4211 L10-14: it is not clear if the difference in the parameters are significant and how large is the uncertainty. The difference could be also due to the different number of data points available in the two datasets. A sampling method like the bootstrapping would help to answer this question.

A.A.: The confidence intervals are now given in the Table 5 presenting the parameter values. As GPP2000 are significantly different ($p < 0.05$) the GPP when PPF is relatively high ($> 1200 \mu\text{mol m}^{-2} \text{s}^{-1}$, data included in the new version) are also significantly different.

R.C.: 9)P4212 L8-9: It is not demonstrated that differences in the magnitude in one year, although with the same magnitude of the inter annual variability, would affect the interannual analysis because it could be also a constant bias trough the different years. To check this it is necessary to apply the same scheme to different years and see if the impact is constant or not.

A.A.: The fact that the tests application has an impact on the inter annual variability is

not demonstrated and this affirmation is not present in the MS. We just say that the test application has the potential to modify the inter annual variability. I agree, this has to be check with tests application on the data from several years.

R.C.: 10)P4212 L17-19: The fact that the u^* threshold is constant from year to year is not shown in the paper.

A.A.: I agree, it's only obtained by comparison with the work of Papale et al. (2006). This has then been suppressed from the conclusion

R.C.: 11)PARAGRAPH 3.5 and CONCLUSION: Are the differences in the annual sums significant? Or are the differences in NEE and Reco of less than 100 gC m⁻² inside the uncertainty due to errors, filtering, gapfilling and partitioning?

A.A.: It's difficult to evaluate the uncertainty for total NEE, GPP or Reco because it has different origins (Ec measurements, gap filling and partitioning process,...) The estimation for each origin requires an adapted method and the discussion to evaluate their accuracy and reliability are still going on (Richardson et al., 2008). So the estimation of the uncertainty for total NEE, GPP or Reco seems to be a inadequate objective for a paper focused on EC measurement quality tests

R.C.: MINOR COMMENTS: 1)P4203 L17: "(25m long, 5° large)". Not clear what it means

A.A.: A new text explaining the spatial resolution used for the footprint test is presented in the new version of the MS

R.C.: 2)P4203 L25-28: A table would help to better understand the differences between DSIFR and DSEFR

A.A.: The new Fig. 1 illustrates the new version of the text (section 2.3) explaining the tests and the difference between the two datasets

R.C.: 3)P4206 L18: for consistency with the rest of the text change "CO2 flag" with

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"CO2 IRGA flag"

A.A.: It's done

R.C.: 4)P4211 L3: the spatial variability and other control factors are not removed but hidden. To remove these effects a normalization should be used, like it has been done in the u^* threshold selection.

A.A.: The word "removed" has been replaced by "hidden"

R.C.: 5)P4199 L22-23: the most updated and appropriate paper to cite here is the Moffat et al. (cited later in the paper)

A.A.: Moffat et al. is now cited for the data gap filling method

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