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Biogeosciences Discussions

Interactive comment on "Multiple quality tests for analysing CO_2 fluxes in a beech temperate forest" *by* B. Longdoz et al.

Anonymous Referee #3

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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

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and published aspects, but then the paper should be focussed more on the effect of the quality test rather than on the tests themselves.

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.

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.

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.

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.

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).

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.

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.

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 inter-annual 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.

BGD

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

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?

MINOR COMMENTS:

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

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

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

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

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

Interactive comment on Biogeosciences Discuss., 4, 4197, 2007.