

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 eddy-covariance (EC) method is based on a developed turbulent regime. In night time or calm conditions this regime is not present. Therefore, under such conditions the EC method cannot be used. It is wrong to speak about a low data quality when the method does not work. For such conditions you have to find out other measuring methods like the determination of the storage or advective fluxes. You can figure out the border between turbulent flow and non turbulent conditions with the data quality tests, mainly the integral turbulence characteristics (Foken and Wichura, 1996). A more restrictive method is the u^* -criterion (Goulden et al., 1996). You should make this clear in the formulation (p. 4199, line 8-15; also not well formulated in Papale et al. (2006)).

Authors Answer: The reference to the 'quality tests' has been removed from the MS

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(specially in the introduction) and replaced by the 'tests on instrumental anomalies' for the tests on the high frequency records and by 'stationarity', 'footprint' or 'turbulence characteristics (u^*)' for the other tests. The same replacement has been realised for the words 'quality of the data'

R.C.: Intermittent turbulence is also a case where the EC method fails (p. 4202, line 22).

A.A.: The words 'Intermittent turbulent events' illustrating the Fig 2b are not adequate. They have been replaced by 'turbulence with varying intensity'

R.C.: But obviously you have used a u^* -criterion (p. 4204, line 19ff). Therefore most of the already flagged data are now excluded with this criterion (p. 4007, line 24ff). Probably the selection into DSIFR and DSEFR (complicated name) data is partly senseless. A graph with the selection procedure or even a table could help to understand what you have done.

A.A.: The Fig. 1 has been added with the scheme of the tests procedure and the text of the section 2.3 about this procedure has been modified to be easily understood

R.C.: It is unusual to determine the climate for an eight year period. Is it not possible to use 30 year measurements of a neighbourhood climate station to determine for your site "synthetic" 30 year data? (p. 4200, line 19-21).

A.A.: The climate of 2005 is now compared to the mean climate of the 30 previous year

R.C.: The order of all tests is not clear from the text (e.g. 4206, line 30ff and Tables 1 and 2). The spike test must be the first test. If you use the steady state test as the first one (obviously done) you select also the spikes and the test works as a spike test and not as a steady state test. Some of the tests given in Table 2 are unclear. A better structure of the paper or graphs can help to avoid misunderstandings.

A.A.: The spike test has been applied first and the records have been despiked before to be submitted to the other test. This is now explain in the text of the section 2.3 and

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in the new Fig. 1 like for the other tests.

R.C.: The different tests for the EC method have no influence on the energy balance closure (p. 4213, line 8-13). Their influence is relatively small (Mauder and Foken, 2006) in relation to other factors (e.g. Kanda et al., 2004).

A.A.: The sentence about the questioning of the tests impact on the energy balance closure has been removed.

R.C.: For the reader remains the question: Is an incomplete flagging system for EC data the reason for large differences in NEE or - and this should be discussed also in the paper - are the not well defined Lloyd-Taylor, and probably also the Michelson-Menten, equations the reason. If both equations would parameterize a clear relationship with a value $R^2 > 0.95$ then NEE cannot depend significantly on the number of gaps which must be filled. The test to improve the Lloyd-Taylor equation failed. A conclusion could therefore be to reduce the number of gaps by an improved test system for EC. This would mean canceling the u^* criteria and finding other tests (Ruppert et al., 2006).

A.A.: In the last paragraph of the Section 3.3 it's now mentioned that the relationships describing the temporal variability of Reco and GPP have a moderate validity and, in these conditions, the tests like the u^* one should not overestimate the number of gaps).

R.C.: This may be more objective but does not decrease the number of gaps. Probably the conclusion should be to replace the Lloyd-Taylor and Michelson-Menten equations by better parameterizations or even a model

A.A.: I agree that the conclusion of the MS should be validated in a close future by using different parameterizations or models. This remark has been added in the Conclusion section).

R.C.: Small remarks: p. 4200, line 15: How have you determined the roughness length? From the experimental setup it seems not to be possible. Are the data from

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before or after the thinning?

A.A.: The value given is wrong (MS mistake). The roughness length determined with the logarithmic profile law using the near neutral period (stability parameter between -0.1 and 0.1) is 1.4m This is relatively low but not unrealistic in view of the very homogeneous canopy with 17m height trees. Anyway, as this information is not essential for the MS purpose, it has been suppressed.

R.C.: p. 4200, line 17: The distance tower-edge probably varies with the wind direction?!

A.A.: Yes, this information has been added in the MS

R.C.: p. 2003, line 17: Explain what you mean: "5° large"

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

R.C.: p. 4203, line 19: You can probably now add Göckede et al. (2007)

A.A.: It's done now

R.C.: p. 4204, line 23: Is Black et al. (1996) or Lloyd and Taylor (1994) the right reference?

A.A.: Black et al. present and use the Q10 function and the paper of Lloyd and Taylor is more centred on a modified Arrhenius law

R.C.: p. 4205, line 8ff: The flagging system used in the two references and your system are probably not identical. Therefore an exact comparison is impossible.

A.A.: I agree, the word 'comparison&' used in the MS is not adequate. It has been removed

R.C.: p. 4208, line 16: Use digits of R2 uniform in the paper and tables.

A.A.: It's done now

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R.C.: Table 4 and others: Reduce the number of digits to 2 for all R2 and not only in the first line of this table, and give an interval of significance.

A.A.: All the R2 have now two digits and the confidence intervals have been added in Table 3, 4 and 5

R.C.: Fig. 1b: This is not intermittent turbulence.

A.A.: The legend of the figure is now turbulence regime with varying intensity

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