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

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

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.

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).

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.

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.

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 R2 $>$ 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). 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.

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 before or after the thinning?

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

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

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

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

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. The comparison in line 17 is possible.

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

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.

Fig. 1b: This is not intermittent turbulence.

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