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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Contribution of root and rhizosphere respiration to the annual variation of carbon balance of a boreal Scots pine forest" *by* J. F. J. Korhonen et al.

Anonymous Referee #3

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The manuscript presents the results of a girdling study in Finland, showing seasonal dynamics of decomposition derived and plant derived CO2 sources within the soil. What this study offers beyond those of other girdling experiments is that this was carried out within the footprint of an eddy covariance tower, so that root- and decomposition fluxes can be related to total ecosystem fluxes (TER and GPP). However, what surprises me, and ultimately makes these results unpublishable as a study in its own right, is the lack of replication. I find myself agreeing with most of the interpretation of the results, and find it plausible that the reported relations are real, but in the absence of replication, it is not possible to trust these results, and they should not be allowed to make it into the scientific literature. Spatial heterogeneity within the footprint of the eddy covariance tower seems to be considerable, and the authors need to apply several corrections first to scale the treatment fluxes to control fluxes, and then to scale the girdling results to the wider footprint area. This would not be necessary given adequate replication and a better choice of study areas based on preliminary surveys and a blocked approach. I do realise that a lot of work went into this study, and that it may seem harsh to preclude publication on grounds of principle, but I think that this is necessary here. I hope that the results can be used in some other way, but in this presentation, they can not strand alone. Some of the observations, for example within plot variability of Rr vs. Rd, and the bias resulting from the use of growing season data only compared to annual data of partitioned fluxes are very interesting, and I hope that the authors can find a way of working this information into a good publication.

Forest floor vegetation: The ground cover contributes to total ecosystem fluxes seen by the eddy, but you ignore these in your interpretation of Rr and Rd. I would expect to see the likely influence of continued ground cover contributions to Rr and Rd. There a a couple of studies dealing with ground vegetation contributions to stand flux estimates that could be useful to this end.

Sap flow: Here also, replication is inadequate (one and two trees for treatment and control). Results are referred to but not presented - either include them completely or leave this aspect out. No conclusions are drawn from these anyway.

The text is written fluently, and the authors express themselves very clearly. However, it would clearly benefit from being proof read by a native speaker.

I include some more detailed comment in the hope that they may be useful in case the results can be used in combination with a different publication.

6180, 10: You have so far not explained what Rd stands for

6183, 26: From your description, this is not a flow through, but what is commonly referred to as a "dynamic" chamber (same as a Li8100 or Li6400 chamber principle).

BGD

6, C1563–C1566, 2009

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"Flow through" implies a constant draw of ambient air through the chamber and a differential CO_2 measurement between ambient and chamber air.

6184, 15-17: I note that there is a fundamental difference in collar installation between treatment and control plots which might confound results, and may indeed partly explain the observed difference between the plots prior to girdling - or not?

6185, 11: You should make it clear that in your calculation, Rs refers to the total soil CO_2 efflux in the control plots.

6185, 1: State that T_0 is 10 deg. Celsius in your case, i.e. your reference temperature for the basal respiration.

6185, 20-26: The Q_{10} values reported here are extremely high, which is indicative of a too deep measurement depth of soil temperature. If you choose to present temperature response of your results, you should treat the issue of where temperature was measured carefully, as you could generate almost any Q_{10} value by using a deeper or more shallow measurement depth.

6187, 7-8: This information is repeated later on.

6187, 11: I think you should have divided the results by 1.22, rather than multiplied them?

6188, 5-7: The peak time for respiration components is a repetition from earlier.

6188, 11-14: Why report the sap-flow under respiration results heading? As I said earlier, these either need more space, or should be removed.

6191, 10-15: I fully agree that a modelling of Rr on the basis of soil temperature is not adequate, and the poor fit presented in Fig. 5 illustrates this. Apart from capturing that there are higher fluxes in summer than in winter, it shows little resemblance to the measured values from which it was regressed. The seasonal bias is considerable for winter fluxes and summer fluxes alike, and I don't agree that you can call them "close to correct" on the assumption that the two unquantified biases simply cancel out.

6192, 3-5: Are allocation and substrate availability not the same thing when it comes to Rr?

6194, 6: I do not see the stronger seasonal cycle in Rd. If anything, Rr has more extreme values between summer and winter, with more drastic transitions between them.

Interactive comment on Biogeosciences Discuss., 6, 6179, 2009.

BGD

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