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Biogeosciences
Discussions

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.

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We thank referee 3 for willingness to give constructive comments although he/she suggests that the paper should not be published as such.

Referee 3: 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.

Response: Replication obviously affects the uncertainty, but the definition of replication in this case is ambiguous. We think that the level of uncertainty should be criteria for deciding whether a study is allowed to be published or not and the uncertainty should be compared to the knowledge already existing. It should therefore be defined what exactly should be replicated and approximately how big uncertainties are caused by the lack of replication. We specify here what is replicated and what is not, and give further estimations of uncertainties in the manuscript.

Liski (1995) showed that most of the variability in C stocks in soil occurs in scale less than one meter, suggesting that also most of the variation on soil CO2 efflux occurs also in small scale. Our measurements of soil CO2 effluxes in this study shows the same, which is that small scale variation of soil CO2 efflux is so large, that the 22% difference between the plots is not statistically significant (see response to the first comment of referee1). For small scale variation in CO2 efflux, we have 12 replicates at the girdled plot and 14 replicates at the control plot. If we had had more replicates, we would not have been able to measure the CO2 effluxes at a plot during the same morning, and the temperature of the soil would have changed radically during the measurement, causing bias to the results.

For larger scale variation in soil CO2 efflux we do not have replication, but we were able to compare the plots by making two assumptions (page 6185, lines 5-8). We think that in general in similar studies the initial difference between the treatments and controls should be taken into account, whether the initial difference is large or small. In forest, there is always visible heterogeneity, and a blocked approach would have leaded in selecting study areas that are visibly different from each other. This would have leaded to even higher variability in the girdled and control plots, making it more difficult to show the effect of the treatment. Instead, we deliberately selected two plots, which had the same forest site type, forest age, tree species composition and which visibly were as close to each other as possible. We also wanted to measure in the vicinity of SMEAR II –station, which is a matter that unarguably increased the value of our study, but it also caused practical limitation.

We girdled 21 trees, and at the control plot there were even more trees affecting the CO2 soil efflux. Therefore, there was significant replication of trees involved in the study. We also had spatial replication, as we measured the effluxes 24 times in a year.

We have done our best in quantifying the uncertainties of the study, and they are presented in the revised manuscript.

Liski, J.: Variation in soil organic carbon and thickness of soil horizons within a boreal forest stand-Effect of trees and implications for sampling. Silva Fennica, 29(4), 255-266, 1995

Referee 3: 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.

We agree that forest floor should be taken into account. We have done that in the revised version of the manuscript.

Referee 3: 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.

We decided to leave sapflow totally out from the paper.

Referee 3: 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.

We will consider additional proof reading.

Referee 3: 6180, 10: You have so far not explained what Rd stands for

Rd now explained

Referee 3: 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). "Flow through" implies a constant draw of ambient air through the chamber and a differential CO2 measurement between ambient and chamber air.

As you note, the chamber is not flow-through, and it is now corrected to the manuscript.

Referee 3: 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?

As you mention the placement of the collars is different at the plots and may indeed affect the difference in effluxes prior to girdling. The reason is that at the girdled plot the spatial variability is smaller than at the control plot. Unfortunately we could not girdle larger area or several areas, because of the vicinity of the eddy covariance measurements. On the other hand, the whole idea was to girdle the area near the eddy covariance tower to be able to link the measurements to GPP.

Referee 3: 6185, 11: You should make it clear that in your calculation, Rs refers to the total soil CO2 efflux in the control plots.

Clarified in the text.

Referee 3: 6185, 1: State that T0 is 10 deg. Celsius in your case, i.e. your reference temperature for the basal respiration.

Clarified in the text.

Referee 3: 6185, 20-26: The Q10 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 Q10 value by using a deeper or more shallow measurement depth.

In the fittings we initially used different depths of soil temperature measurements and found that average of H and A explains respiration the best. The purpose of calculating Q10 is to demonstrate that temperature does not alone explain soil respiration very well. This is now more clearly specified in the manuscript.

Referee 3: 6187, 7-8: This information is repeated later on.

The reason is to make the text more reader friendly.

Referee 3: 6187, 11: I think you should have divided the results by 1.22, rather than multiplied them?

Correct. We did divide the results by 1.22. Corrected to the text: " …multiplied with 0.82".

Referee 3: 6188, 5-7: The peak time for respiration components is a repetition from earlier.

Removed the sentence considering Rr:Rd-ratio.

Referee 3: 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.

We decided to remove the sapflow from the manuscript.

Referee 3: 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.

It only matters little which function is used for gapfilling, because the integral of the time period can be calculated as sum of averages during the period, and many functions (such as a line) on average gives correct value when the fit is done properly. Functions actually describing the processes well are needed in gapfilling, when the explaining variable during the measurements is systematically different than in reality, or if there are hotspot events that the data does not represent well enough.

However, we removed the sentence you were referring as it is irrelevant in the context, because the focus of the paper is not in annual respirations and we did not use the directly anyway.

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

Rr consists of root respiration and rhizosphere respiration. In this case, allocation controls substrate availability between roots and rhizosphere, which may be different.

Referee 3: 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.

Corrected to text: Annually Rr is lower than Rd, but has stronger seasonal cycle.

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