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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 1 for the critical reviewing, which allows us to improve the paper, as well as for the numerous specific comments.

The following weaknesses were reported by anonymous referee #1:

1. Differences in the background respiration between the control and treatment

In the experimental design we relied on the study Liski (1995), showing that most of the spatial variability in soil carbon stocks takes place within 1 m distance. If the chambers are more than 1 m away from each other, they are practically statistically

independent of each other. We reassessed the statistical analysis of the initial effluxes measured at the girdled and the control plot. The effluxes before the girdling were 0.143 +- 0.040 and 0.1175 +- 0.0305 (mean of all measurements +- mean of daily standard deviations), from the girdled and control plots, respectively. The standard deviations are 28 % and 26 % of the respective mean effluxes and the difference between the plots was 22%. The test results presented in the manuscript are wrong (page 6186, line 19). We re-tested the difference between the plots by 2-way t-test and we did not see statistical difference between the plots (p=0.0855). In other words, the difference can be explained by random variation in the effluxes, supporting our view that small scale spatial variation of CO2 efflux is more important than large scale variation in forests that are homogenous in forest type, age and tree species composition like in our study site. In our case the difference in the background respiration between the control and treatment was unexpectably high, and the results should be interpreted keeping this in mind.

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

2. Long sampling intervals

We expect that after few weeks from the girdling, the changes in respiration are gradual. Therefore, bi-weekly to monthly sampling interval is enough to clearly show the seasonal pattern of soil respiration although short-term variability will be lost. On the other hand, if the main scope of the study would have been short term environmental response of root and rhizosphere respiration, an automatic chamber measurement system would have been suitable. We see that the temporal coverage of our dataset is strength of our study, as compared to other studies considering root respiration of boreal forests. See also the response to the comment about diurnal dynamics of respiration (page 6183, line 23).

3. Relatively small study plot without any replicate plots

The study plot is relatively small, since we wanted to stay in the vicinity of the control plot (SMEAR II station), but on the other hand did not want to disturb the long-term monitoring site at Hyytiälä SMEAR II -station. We did not have replicate plots, as based on study of Liski (1995) we expected that most of the variation in soil CO2 efflux is dependent on small scale spatial variation. In that perspective, the number of replicates of CO2 efflux measurements is more important than the number of replicates of treatment/control plots. See also the answer to "differences in the background respiration between the control and treatment".

As we discuss in the paper, there may be edge effects in our study, but we estimate that the effect to the defined Rr is probably less than 10%.

4. Lots of scaling and assumptions made in the calculations

To get the initial levels of CO2 effluxes of control and treatment plots so close that the difference is insignificant either a very high number of replicate measurements or luck, sometimes both is needed. Whenever quantifying as accurately as possible the differences between control and treatment plots, this initial difference should always be taken into account. Also see the answer to "differences in the background respiration between the control and treatment".

We made another scaling to compare root and rhizosphere respiration to GPP. We considered this necessary because using the absolute soil fluxes directly would have given unrealistic estimates of respiration (total soil CO2 efflux was approximately equal to GPP). The flux measurements in the girdling study and the long-term reference measurements were done with different chamber setups. Thus, the ratio of root respiration to total soil efflux can be determined more accurately than the absolute fluxes and we determined the absolute Rr in the stand by applying the Rr/Rs ratio.

We listed two assumptions (Page 6185, lines 5-8), which are the fundamental bases

for any studies quantifying root and rhizosphere respiration using girdling, trenching or similar methods. We did not make any additional assumptions related to the difference in the "base CO2 efflux".

The other main assumptions made in the calculations are: 1) ground vegetation does not severely affect the measured effluxes, and 2) there is no edge-effect caused by the relatively small girdled area.

We estimated that the overestimation caused by the ground vegetation to the defined Rr, is 12 %. We estimate that the error caused by the relatively small girdled area is less than 10 % of the defined Rr. The calculations are explained in the manuscript.

SPECIFIC COMMENTS

Abstract: Referee 1: -Line 5: please specify "breast height" -Now specified

Referee 1: Line 10: please specify Rd -Lines 7-9: Done

Referee 1: Is this scaling needed in the abstract You are correct, it is not needed. It is now removed.

Referee 1: Line 15: was GPP significantly risen in winter? We clarified the text, GPP was of course not risen at winter.

Referee 1: -Figures. 2 and 3. Could you combine these two figures and use the same scale for x-axis?

The suggestion will improve the value of the figures, and we are glad to combine the figures.

Referee 1: -Girdling was done on 18 June, how fast does it affect? And how did you take in account the time lag?

Högberg at al. (2001) reported that soil CO2 efflux decreased 54% in 1 – 2 months after the girdling, and that 70% of that reduction had occurred just 5 days after the

girdling. We assumed that after 22 days the girdling practically has the full effect. This assumption is now reported in the materials and methods.

Referee 1: -Page 6183, line 15, which data was recorded?

We removed the whole sentence, as it was irrelevant.

Referee 1: -Page 6183, lines 16-17. Could you shortly describe the Granier method (and give the results in results chapter). The reference (Granier et al. 1985) is in French. –

We decided to remove the whole sapflow from the manuscript, as it would take too much space and it is not important.

Referee 1: The measurements were made only in the mornings (Page 6183, line 23) Respiration rates have diurnal dynamics.

We measured in the mornings, since we wanted to keep the temperature as constant as possible for all the measurements on that day in order to reduce the temperature effect on the comparison between the treatment and control. The soil temperature increases during the course of day and reaches maximum at around 16-18 in the afternoon. When measured in the morning the large differences in afternoon temperatures between sunny and overcast days can be avoided. If the control would be measured at noon and treatment in the afternoon, the treatment effect could be biased since the CO2 efflux is highly temperature dependent. When calculating the cumulative annual soil CO2 efflux, we used exponent function to calculate the diurnal dynamics of respiration. Fig. (1) in Pumpanen et al. (2003) shows that the effluxes at approximately 10 a.m. are close to daily means. Pumpanen, J., Ilvesniemi, H., Perämäki, M., Hari, P., Seasonal patterns of soil CO2 efflux and soil air CO2 concentrations in a Scots pine forest: comparison of two chamber techniques, Global Change Biology, 9, 371-382, 2003.

Referee 1: -Page 6183, lines 10 & 17 Please specify ”breast height”

Now Specified for line 10

Referee 1: Page 6183, line 14. How far from the girdled plot was this SMEAR II station, was it the control plot?

We added more information about this.

Referee 1: Page 6183, line 26, give the dimensions of the chambers, were they similar as in control plots (page 6184, line 12)?

Chamber dimensions are now specified, as well as the mean volumes enclosed by chamber and collars at control and girdled plots.

Referee 1: -Page 6184, lines 4-6. If the CO2 flux was not linear, why did you exclude the first measurements and made it to be linear? See: CO2 flux determination by closed-chamber methods can be seriously biased by inappropriate application of linear regression. Kutzbach L, Schneider J, Sachs T, et al. BIOGEOSCIENCES 4: 1005-1025, 2007

We excluded the first measurements of the closure, because the installation of the chamber on the collar disturbs the CO2 flux from the soil. We used linear fit purely for practical reasons. We excluded the last points of the measurements, because we already had enough data points for fitting compared to the noise in the measurement signal. The saturation of CO2 in the beginning of the measurement is probably masked by other disturbance in the chamber, such as mixing of the air by the fan, pressure pumping effect resulted when the chamber is placed on the soil. The saturation of CO2 in the chamber becomes a true problem with chambers of long deployment time e.g. 10-30 min. In our study the deployment time was max 240 seconds and the CO2 concentration in the chamber headspace did not usually increase more than 100 ppm above the ambient. However, assuming that the saturation is similar in both treatment plots, the usage of a linear fit in this case is not a problem, because we do not directly use the absolute values of the effluxes, but instead calculate ratios between the fluxes

between the two treatments.

Referee 1: -Page 6187, lines 13-14. What does this mean, monthly values from measurements? Why?

We clarified the text by replacing “monthly values from measurements” with “monthly values as an average of the scaled measurements for each month (Eqs. 1 and 2)”. The modelled values do not represent the dynamics very well, and therefore we decided to use the non-modelled values for describing the seasonal variation.

Referee 1: -Page 6187, lines 26-27. What is this Hyytiälä forest, is this SMEAR II station, is it the control plot in Hyytiälä forest? (at the moment reference Kolari et al. 2009 is not found online from Boreal Environment Research)

Clarified, Paper is now available online.

Referee 1: -Page 6188, line 4: Annual Rr and Rd in control plots? which periods?

The periods are now mentioned in the beginning of 3.2. We clarified what was done both in materials and methods and here. Rd is calculated from the scaled effluxes of the girdled plot, and Rr is calculated from the difference between the annual scaled fluxes between the girdled and the control plot.

Referee 1: -Page 6188, line 11-13: Did you test it also later in the following year? Could you show the results?

We only measured sapflow in 2007 and we have decided to exclude the sapflow from the manuscript.

Referee 1: -Page 6190. lines 3-4. Could this be a result of a small girdling plot in your study?

The increase in the CO2 effluxes at the girdled plot after one year of the girdling may be caused by a small girdling plot or by increased substrate availability for decomposers (dead roots and increased litterfall). This is being discussed at chapter 4.4.

Referee 1: -Page 6193, lines 28-29. If there were no annual trend in the total ecosystem respiration, was it then similar throughout the year?

We have replaced word ‘annual’ with ‘interannual’ to avoid further misunderstandings.

Referee 1: Acknowledgements: -Line 17 It is “Maj and Tor Nessling foundation: : :”

Now corrected

Referee 1: -Kolari P et al. (2009) CO2 exchange... and Ilvesniemi et al. (2009) were not found online in Boreal Environment Research

The papers are now available online.

Referee 1: Lloyd and Taylor (1994) is missing from the reference list

We have added the reference to the list

Referee 1: -Figure.1 Where are the control plot and collars?

We inserted the location of control plot to the caption.

Referee 1: -Figure 4. The bars start from negative values, this can be confusing. The bars should start from 0 and the negative values should be downwards from 0. Why do you use here only measured, not modeled values?

The axis in figure is now corrected. This purpose of the figure is to present the seasonal dynamics of soil respiration. Modeled values do not explain well this, as explained in discussion. However, we believe that modeled values are better for budgets, as monthly values may have bias due to low temporal (monthly and diurnal) coverage.

Referee 1: -Figure 5. The measured Rr values during winter are clearly lower than the modeled values. Could you just use the measured, not modeled values for the winter period? Show also GPP here, the GPP results are used for calculating ratios but they are not shown.

When calculating the annual Rr, the same methods should be use systematically used

for all periods, otherwise the result will be biased. See answer to anonymous referee #3 of question about page 6191, lines 10-15. The purpose of Fig. 5 is to demonstrate that temperature response alone cannot explain the seasonal dynamics of the Rr very well. GPP is presented in figure 4. The ratios are based on monthly values, which are also presented in figure 4.

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