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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 would like to thank referee 2 for pointing out several factors that affect our results that should be calculated or discussed. We believe that with the help of these comments, the quality of the manuscript is improved significantly.

Referee 2 reports three main problems in the study:

First of all, the many assumptions done in the study concerning the difference in the "base CO2 efflux" between the control and treatment plots and the unexpectedly large difference in the annual CO2 efflux on the control plot between this and the earlier study (Kolari et al. 2009) complicate the interpretation of the data and the calculation

of the separate components for the site. The effect of the assumptions should be more thoroughly discussed and estimated (see detailed comments, e.g. Page 6185, lines 5-8).

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" (see also the reply to detailed comment page 6185, lines 5-8). See also the answer to referee # 1 comment "Differences in the background respiration between the control and treatment". The difference between the control plot and our previous measurements (e.g. Kolari et al. 2009) is larger than we expected. The possible explanations may be interannual variation, changes in measurement systems and analysis, but the reason is not very relevant, as we are mainly dealing with ratios in the manuscript. However, the possible explanations are now discussed more thoroughly in the manuscript. We have also included error estimates to the revised manuscript.

Second, as the strength of the paper is the combination of several flux components like GPP and Rr, I would like to see an effort to explain the annual course of Rr with GPP, or even that of Rs with the combination of GPP and soil temperature. On the other hand, I don't see the Q10 approach used in the paper useful (see detailed comments).

As we assume that the response from photosynthesis to Rr is some days, the temporal resolution of bi-weekly measurements of soil CO2 effluxes is too low. On the other hand, we want to avoid confusing the reader by keeping the manuscript as simple as possible. We believe that analysis of the relationship between GPP and Rr should be done in another manuscript. We found indication that also allocation dynamics, not only GPP drive Rr. Therefore we believe that by using this dataset the results would be inconclusive.

The Q10 values are only used in calculating the annual Rd:Rr. They do not reflect true Q10 of the processes, but they contain information about how well the other fac-

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tors than temperature explain respiration, as now discussed more thoroughly in the manuscript. See the response to referee #3 to comments for page 6191, lines 10-15.

Third, although the authors mention in the discussion (!) that there was forest floor vegetation in the chamber collars used to measure the CO2 efflux, no description of it can be found. Is it possible that the variation in the ground vegetation could explain e.g. some of the differences in the CO2 efflux between the control and girdling plots prior to girdling? As stated in the title of Kolari et al. (2006), the "Forest floor vegetation plays an important role in photosynthetic production of boreal forests" and most probably also in the respiration. It should be stated more clearly already in the Mat&Met that the measured CO2 efflux also includes the above-ground plant respiration of the ground vegetation, i.e. Ra. In general, information about the forest floor vegetation in chamber plots and in the forest is needed: differences in species, biomass etc. between the control and girdling plots. Finally, using Rd, when actually measuring Rd + Ra is not really correct, so I recommend using a more descriptive term throughout the paper.

Ground vegetation contributes about 15% of the GPP of the forest (Kolari et al. 2006). We did not observe any visible differences between the plots. The dominant species was billberry (Vaccinium myrtillus) on both plots. We have now written in the materials and methods, that ground vegetation existed inside the collars.

By making following assumptions, we get an estimation of the significance of the variation of ground vegetation to the measured soil CO2 effluxes: 1) Annual aboveground respiration of ground vegetation is 30% of their GPP, 2) Rd caused by litter production of ground vegetation is also 30% of their GPP, 3) carbon used annually in growth equals carbon released in Rd from dead material of ground vegetation. Calculating carbon balance for ground vegetation by the assumptions listed, we got as result, that the 22% difference in the initial efflux rates would require over 3 times larger GPP of ground vegetation at the girdled plot. In terms of biomass the required difference would be even higher, because the effect of shading increases as biomass increases. As we did not observe differences between the plots, we conclude that ground vegetation can

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only play a minor role in explaining the initial difference of the CO2 effluxes at the plots. We have included this estimation in the discussion of the manuscript.

We used the same approach to calculate how much ground vegetation affects our estimate of Rr:Rs. The original annual Rr:Rs was 0.360, and the revised Rr:Rs is 0.322, showing 12% overestimation of the Rr:Rs. We have included revised values of the Rr:Rs in the manuscript and explained the calculation in materials and methods.

SPECIFIC COMMENTS:

Referee 2: The title of the paper, ": : : to the annual variation of carbon balance: : :" does not reflect the main message given in your paper. You do not actually show the annual variation of the C balance anywhere, but only give the numbers of the annual soil CO2 efflux and annual TER. My suggestion is to replace "carbon balance" with GPP in the title or change the title in some other way. Or in alternative, include in the NEE results, i.e. the real CO2 balance.

We changed the title according your suggestion to: "Contribution of root and rhizosphere respiration to the gross primary production of a boreal Scots pine forest"

Referee 2: Page 6186, line 18-19: Could Rs explain this difference, given that there were plants inside your chambers.

We conclude that Rs is the reason for the difference in the initial effluxes between the plots. See the response to the third major problem.

Referee 2: Page 6182, line 10: ": : :eddy covariance (EC): : :". Respectively, remove "(EC)" from p.6186, line 3.

We decided not to use the abbreviation at all.

Referee 2: Page 6183, line 25: In Kolari et al. (2006), all the chambers used were transparent, so given that the chamber used here was different from those, perhaps a bit more detailed description of the chamber design is needed here.

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We now clarified that the measurements were based on the principles described in Kolari et al. (2006), not that the measurements were similar.

Referee 2: Page 6185, lines 5-8: You also assume that the temperature response of the respiration is the same at both plots! Do you have any data on that? Can you give an estimate of how big an error you will introduce in the result if you assume a different temperature response for the control and girdled plots?

We do not assume that the temperature response of respiration is the same after the girdling, but we do assume that the temperature response (Q10) is the same for the plots, if neither of them had been girdled. This assumption can be derived from the two assumptions listed at Page 6185, lines 5-8. We reported that Q10 for Rd (girdled plot) varied from 4.0 to 4.4 and Q10 for Rs (control plot) varied from 4.4 to 6.4. Note here that the scaling has no effect on the Q10 values.

Referee 2: Page 6188, line 2: “ecosystem annual total soil respiration” is a bit confusing term, it is not clear what are you trying to describe with it.

As Rsa is defined in previous sentence as “mean annual total soil respiration of the SMEAR II stand (Rsa = 625 g C m-2 a-1) (Kolari et al., 2009)”, we now simply refer to Rsa here.

Referee 2: Page 6188, line 10: The title of the chapter is not very descriptive, since it deals with Rr, Rd and Rs. What do you mean by soil respiration here? More precision with the terms!

We have changed the title of the chapter

Referee 2: Page 6188, line 14 onwards: Define and explain how you calculated the temperature response values.

Temperature response calculation is now explained in materials and methods.

Referee 2: Page 6190, line 3-4: You state that “the effect of girdling decreased”. Could

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you simply say that at your site “the CO2 efflux at the girdled plots was larger in the second summer”? By the way, why was that?

We changed the text to “On the contrary, we saw that after one year from the girdling the ratio of measured CO2 effluxes at the control and girdled plots (Fc:Fg) decreased.”. The change in the ratio of the measured effluxes is more informative the change in CO2 efflux alone. The increase in Fc:Fg is discussed in chapter 4.4.

Referee 2: Chapter 4.2: 1) I don’t see what is the advantage of the Q10 analysis in this paper? Your conclusion is that Rr has much larger temperature sensitivity, but later on you explain that this is not really a sound analysis due to many confounding factors, mostly the supply of exudates, and that Rr should not actually be modelled with temperature. Later on, you base your further conclusion on this relationship by modeling the annual values of different respiration components using the temperature as a driver (Fig. 5). In my opinion, it would be more useful to try to model the Rr with the GPP which is, as you state several times in your manuscript, probably the most important driver of Rr. It will probably improve the fit in Fig. 5. You could perhaps try with the GPP of e.g. previous month, since you state elsewhere that “Rr followed GPP with a delay of several weeks”. Establishing such a relationship from the intensive experimental data you have would most probably benefit the modelers as well

See the response to the second major problem. We agree that it would be beneficial for the scientific community to explain Rr by GPP, but trying to assess it with this data and this manuscript is not feasible. However, we are planning to write another manuscript explaining Rr with GPP based on different data.

Referee 2: 2) Could you add some discussion on the significance of the differences in the transpiration rates explained in results? Why was the transpiration decreased? Did it affect the soil moisture at the girdled plots? How could this affect the soil CO2 efflux?

We decided not to present anything from the transpiration as an option from referee #3.

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The transpiration decreased because of the stomatal closure in leaves. We observed this on the girdled, but not on the control plots by measuring CO2 and H2O exchange of leaves with a portable gas analyzer system. We also measured soil moisture at both plots, but we used different instruments on the plots, which makes the comparison a bit hard.

Between years 1998 and 2006, annual transpiration in Hyytiälä was 149mm (Ilvesniemi et al., 2009). A 31% reduction in transpiration represents then 46 mm of water, of which part is lost as increased evaporation and runoff. As annual precipitation between 1998 and 2006 was 692 mm, the increase in soil water content should not be significant. Usually soil moisture explains soil respiration only in extreme cases (Skopp et al. 1990).

Ilvesniemi, H., Pumpanen, J., Duursma, R., Hari, P., Keronen, P., Kolari, P., Kulmala, M., Mammarella, I., Nikinmaa, E., Rannik, Ü., Pohja, T., Siivola, E., and Vesala T.: Water balance of a boreal Scots pine forest. Boreal Environ. Res., forthcoming issue (available online), 2009.

Skopp, J., Jawson, M.D., and Doran J.W.: Steady-state aerobic microbial activity as a function of soil water content. Soil Sci. Soc. Am. J., 54, 1619-1625, 1990.

Referee 2: Chapter 4.3: Since the GPP is affected by the ground vegetation but Rr is not, this proportion (21%) does not hold for the trees. Can you estimate, what is the contribution of the ground vegetation here? How large error do you introduce by measuring the GPP of all vegetation but Rr of only trees?

Based on the assumptions made in answer to major problem 3, the error estimate is 10-15%. We have included the error estimates in the revised manuscript.

Referee 2: Fig.1: Indicate whether the results have been scaled or not. If not, should this figure show only the scaled fluxes, since this data is what you interpret and use in the further analysis?

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Do you refer to figure 3? The caption is now clarified.

Referee 2: Fig 3: Indicate whether the data is scaled or non-scaled. Also elsewhere in the text, the use of original or scaled data should be clearly defined (e.g. p. 6188, line 4)

In Fig. 3 is the non-scaled data and in Fig. 5 the scaled data, now indicated in the captions. The purpose of Fig. 3 is to show the raw data we have been using for analysis to help the reader to evaluate the results.

Referee 2: I would like to see a graph with separate monthly bars for GPP, TER and Rstot, which is further separated into Ra+Rd and Rr.

We will consider making such a graph.

WE ALSO DID THE FOLLOWING TECHNICAL CORRECTIONS:

Page 6180, line 10: define Rd also in the abstract

Page 6180, line 21-22: : : :aboveground plant respiration and total soil respiration.

Page 6182, line 2: ": : :separating Rs to Rr and Rr: : :" correct

Page 6184, lines 23-25: Could you give the mean values of the soil CO2 efflux for the control and girdling plots before the girdling in the text, not only the ratio?

Page 6187, line 15-16: Move "was" after "(Rr:Rs)"; move "annual" before "root"

Page 6189, line 23: "% percent", remove another

Fig 4: The x-axis should be located at zero so that negative bars point downwards. The explanations in the legend text belong rather to the discussion.

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