

## ***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 #2**

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### GENERAL COMMENTS:

The experiment in general is sound and, as the authors state, extremely interesting since there are few or no publications on the measurements of  $R_r$  with simultaneously measured NEE above the canopy. The data offers a great opportunity to get a more detailed insight into to the carbon cycling inside a forest ecosystem.

There are, however, some problems with the manuscript. First of all, the many assumptions done in the study concerning the difference in the “base  $CO_2$  efflux” between the control and treatment plots and the unexpectedly large difference in the annual  $CO_2$  efflux on the control plot between this and the earlier study (Kolari et al. 2009) com-

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plicate 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).

Second, as the strength of the paper is the combination of several flux components like GPP and  $R_r$ , I would like to see an effort to explain the annual course of  $R_r$  with GPP, or even that of  $R_s$  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).

Third, although the authors mention in the discussion (!) that there was forest floor vegetation in the chamber collars used to measure the  $CO_2$  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  $CO_2$  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  $CO_2$  efflux also includes the above-ground plant respiration of the ground vegetation, i.e.  $R_a$ . 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  $R_d$ , when actually measuring  $R_d + R_a$  is not really correct, so I recommend using a more descriptive term throughout the paper.

In addition, a language check is needed.

Therefore, I recommend accepting this paper after major revision.

### SPECIFIC COMMENTS:

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  $CO_2$  efflux and annual TER. My suggestion is to replace “carbon balance” with GPP in the title or

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change the title in some other way. Or in alternative, include in the NEE results, i.e. the real CO<sub>2</sub> balance.

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

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.

Page 6184, lines 23-25: Could you give the mean values of the soil CO<sub>2</sub> efflux for the control and girdling plots before the girdling in the text, not only the ratio?

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?

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

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

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.

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!

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

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

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.

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 CO<sub>2</sub> efflux?

Page 6190, line 3-4: You state that "the effect of girdling decreased". Could you simply say that at your site "the CO<sub>2</sub> efflux at the girdled plots was larger in the second summer"? By the way, why was that?

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?

Fig.1: Indicate whether the results have been scaled or not. If not, should this figure

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show only the scaled fluxes, since this data is what you interpret and use in the further analysis?

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)

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.

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.

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