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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Spatial and seasonal variability of heterotrophic and autotrophic soil respiration in a winter wheat stand" *by* N. Prolingheuer et al.

Anonymous Referee #1

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The manuscript by Prolingheuer et al. submitted to Biogeosciences is an interesting study about the spatial and seasonal variation of the two components of soil respiration. The partitioning of soil respiration into the autotrophic and heterotrophic components and how these vary in space and time is an important research topic. In this manuscript the authors study one soil respiration in one growing season of a winter wheat stand and investigated the temporal and spatial variability of heterotrophic (Rh) and autotrophic respiration (Ra). Based on this study, the authors report that seasonal changes are controlled by temperature and moisture on Rh, whereas the spatial variability of soil respiration is represented mainly by the spatial variability of Ra.

Main comments The manuscript present specific objectives in page 9414 but these objectives are independent from a clear scientific question based on first principles. I believe that the manuscript could be improved by adding the original research question that motivated the study and by the expectations or hypotheses that were formulated at the beginning of the study. Finally, I believe that there is an overlap on the objectives 2 and 3 as objective 3 could be included under the broad premise stated under objective 2. Adding a scientific question and the expectations to justify the objectives will give more clarity to the study.

The study covers only one growing season across 3 months. Knowing that there are climate interannual variations and potentially annual changes within a plantation I wonder if the conclusion of the study is strong enough for a generalization of the results.

There is no indication about when the treatment for root exclusion was done. As this is a plantation one could think that initially there were no live roots in the soil and that the collars prevent roots from growing in that zone. However, there is no measurement about initial spatial variability in terms of fluxes or organic carbon in the soil, especially when considering that variation of heterotrophic respiration is randomly distributed in the study plot.

The authors report that soil respiration was dominated (69%) by Rh leaving only nearly 30% of the contribution to Ra. Furthermore, Rh spatial distribution is random but not that of Ra. Thus, I understand that most of total soil respiration is controlled by a random distribution of Rh and not by the organized spatial structure of Ra. This may be a matter of wording and the key may be found in the conclusion section (page 9156) where the authors recognize that the temporal variability of Rh is somehow "stable" with time but not that of Ra. Also some confusion in the wording is found in the conclusion where there is stated that the Ra shows a strong spatial dependence attributed to the heterogeneity of local root development. Maybe an explanation about how the heterogeneity of root distribution leads to a strong spatial dependence in contrast with the random distribution of Rh (where there is no heterogeneity of roots but maybe

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heterogeneity in the substrate pools for heterotrophic activity).

I appreciate that the authors used the AIC for model selection, but there is no indication in the objectives about the interest in understanding the empirical relationships between the drivers and soil respiration. This is why I think a clear scientific question is needed to better understand the goal and objectives of the study…(see lines 1-5 in page 9140 for a description of drivers).

I encourage the authors to test the differences between Ra and Rh or Rs with statistical tests in order to support the statements that one is higher than the other.

I found section 3.2 important but the way it is written is confusing as it lacks support from a scientific question/objective and a mechanistic explanation about the differences observed among the models. First I found strange that the authors discussed models that did not perform well. The use of the AIC is for model selection, so why discuss the models that were not selected (this is not a modeling paper)? Second, the authors decided to discuss several models but there is not a mechanistic explanation of why some models explains more variability and are more parsimonious than others…a clear example is the confounding effects with temperature. I think it is not appropriate to choose some models for one interpretation and others for other interpretations, especially if a selection procedure has been done.

Finally, the study was done during one growing season at an agricultural plantation. I encourage the authors to do not over interpret their data and to consider the limitations of a one-time sampling experiment. Furthermore, the authors compare their results with multiple vegetation types to explain differences between Ra and Rh. Maybe the discussion should be focused on agricultural crops and only in a minor extent as a comparison with other vegetation and land use types.

Specific comments

Lines8-10 page 9139- To my understanding autotrophic respiration is considered to

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be by roots and mycorrhizal associations even if the mycorrhizal fungus by itself is considered a heterotrophic organism.

Section 2.5.1 — is this section needed? Maybe a few citations about variogram estimation is enough along with lines 15-21.

Line 7 page 9148. The authors sate that fluxes in August may be a result of a lower organic carbon content in the soil. The information about organic carbon is not included in the study and would be very useful to understand the spatial and the temporal variations explained here.

The authors state that the TDR probes were not working properly (lines 12-19 page 9149.For how long this problem persisted? When was this corrected? Does the problematic values were used in the empirical models?

Lines 19-28 page 9149 – this is somehow repetitive from the introduction.

Interactive comment on Biogeosciences Discuss., 7, 9137, 2010.

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