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Biogeosciences Discussions

Interactive comment on "Distinct patterns in the diurnal and seasonal variability in four components of soil respiration in a temperate forest under free-air CO_2 enrichment" *by* L. Taneva and M. A. Gonzalez-Meler

L. Taneva and M. A. Gonzalez-Meler

mmeler@uic.edu

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Response to Anonymous Referee #2

We greatly appreciate the careful and insightful review from the referee. Concerns raised by his/her comments pointed to clear weaknesses in the description of methods and results that has resulted in a much clearer and improved manuscript. We also appreciate the reviewer's comments highlighting the fact that soil respiration changes cannot be attributed to a single component and to the fact that lack of measurable

changes in soil respiration does not mean its auto or heterotrophic components are not changing. Specific comments to reviewer's concerns follow.

Ref-2 #1. While the authors present differences in the day and night patterns and growing season trends in the different Rs components they studied, I think the ms can be improved by also including an analysis into any attempts to try to explain the observed patterns. The authors do mention some analysis with respect to soil temperature and moisture effects, but do not go into details. Furthermore, the lack of correlations between Rs components and for example soil temperature and moisture to which they refer, may be due to the fact that they use 10 cm deep soil temperature and moisture measurements only. One would expect different components to perhaps respond to different driving variables, as mentioned in discussion and introduction. For example, respiration from the litter layer, one would expect to be correlated more with air temperature as compared to mineral soil temperature at a 10 cm depth, likewise precipitation events may be more important compared to soil volumetric water content in this case. Have the authors considered these aspects in their analysis?

The lack of correlation between the components of soil respiration and abiotic factors during the day and night is interesting but not surprising. Seasonal trends of CO2 efflux were influenced by temperature and moisture at seasonal time scales but at diurnal time scales these interaction were not significant. We did explore in detail the moisture and temperature dependent functions of soil respiration and its components for the growing season of 2004. In general, there was a lack of correlation between soil respiration and air or soil temperature at diurnal time scales. The components of soil respiration also showed little sensitivity to diurnal changes in soil temperature. These results are consistent with the observations that soil respiration rates changed very little diurnally (except for June where night respiration was slightly lower than daytime respiration). The diurnal changes in the components of soil respiration were also inconsistent with changes in air or soil temperature: for instance Rsom decreased at night in June but remained the same or slightly increased the rest of the growing season when

compared to daytime rates. A reason for this patterns is that the interactions between plant activity (photosynthesis, water and nutrient uptake) and soil metabolism occur at various time scales: legacy of plant root diurnal processes may enhance Rsom rates at night compensating for short-term temperature responses. We have incorporated these more detailed aspects of the analyses in the results and discussion sections as appropriate

Moisture had little control on these diurnal variations of soil respiration in part because moisture varied very little between the daytime and nighttime measurements. Also moisture may control soil respiration and its components when is above or below certain thresholds and during the growing season of 2004 moisture varied very little seasonally. We also chose to use volumetric water content because it is a more integrated measurement of soil moisture than precipitation events (which may impact soil moisture in different degrees for short periods of time). We also timed our measurements to avoid the influence of short-term effects of proportionally large precipitation events on belowground fluxes and interactions to be able to compare seasonal data.

Litter respiration deserves special consideration. The reviewer makes an important point regarding the influence of surface variables on litter decomposition. We rechecked our previous analyses to test dependency between temperature and moisture on forest floor (litter) respiration. Again, there was no significant effect of temperature on the diurnal variability of this component of soil respiration. Moisture on the forest floor (up to 3cm) had a higher effect on litter decomposition but was still not significant ($p>0.1$). The major reason we think can explain the lack of significant influence of temperature and moisture on litter respiration may be in part due to litter processing and in part methodological. Litter respiration was the smallest component of soil respiration measured and had proportionally the largest standard errors making more difficult to detect changes. Also, litter respiration is expressed in units of ground area as all the other components of soil respiration. However, and unlike other soil C pools, forest floor mass changes substantially over the curse of the growing season

with a input pulse mid to late season and a decrease in mass and chemistry over the course of the season. Therefore the seasonal increase in litter respiration may be due to its increased mass (late spring inputs) and changed chemistry, masking the temperature and moisture dependent functions. Unfortunately we don't have forest floor mass/chemistry values associated to our experiments. In the revised manuscript we have incorporated this caveat to our discussion and have made clear that temperature and moisture may affect litter respiration rates when expressed on a per mass units. We thank the reviewer for helping us improve this part of the manuscript.

Ref-2#2. The statistical analysis used to analyze the data set included the application of mixed effects models. The authors should elaborate more on the results. In the current ms they seem to only present %-differences and some p-values. I think the readers would benefit more from a more detailed explanation of the types of models they fitted to their data and the results of these fits – which of the variables turned out to be statistically significant in explaining their trends. Were temporal driving factors mentioned on line 24 (P 2.9) tested together or separately, which ones were important? Was data controlled for temporal autocorrelation and were temperature and moisture considered in the analysis, especially in explaining seasonal trends? Which brings me to their observation that Rs in 2003 was higher compared to 2004, yet no explanation of this result is provided or discussed (P 3.1). Seasonal /interannual variability is not discussed in detail.

We have provided more information on the random effects cluster analyses. Most analyses were done using "doubly-repeated measures" because the dataset have multiple or repeated measures at each point (both root and non-root respiration) and also within each ring. Observations are clustered for each pair of comparison (for instance root versus non-root, root versus som decomposition) and pairs of observations are clustered within rings. Multivariate analyses were also done using random-effect regressions and seasonal and diurnal abiotic variables. We have clarified the specific points made by the referee on the revised manuscript.

Ref-2#3. Be more clear in what you measured and presented, Rs components : Rr, RL, Rsom is one part of your experiment; separating Rsom into old and young part is another part. Right now it all seems mixed up together and unclear in Abstract, Intro and Results (ex. 3.3.1 goes better with 3.4 and 3.5)

We have amended the document to clarify the separations. Separation of CO2 efflux into post-treatment and pre-treatment C pools (Figure 2) is independent of the autotrophic or heterotrophic organisms oxidizing these large pools (section 3.1). The separation of the components of soil respiration were done independently using different flux and isotope models explained in 3.4 and 3.5). Once Rsom rate is obtained a further separation of Rsom into two age components (pre- and post-treatment C flux) was made assuming most of the pre-treatment C oxidation is heterotrophic.

Ref-2#4. Not sure about journal policies, but I doubt it is allowed to reproduce word for word what has already been published previously without reference. Your sections 2.1 and 2.2 read exactly as in your 2008 publication in Soil Biology and Biochemistry.

Reduced and cited the reference.

5. Figures/Tables: Table 2 and Figure 1 and 2 seem repetitive; Table 3 – which year of data is presented 2003? 2004?or mean of both? – figure 3 seems like a repeat of Table 3;

Figure 1 is the only place where overall soil respiration rates over two years for both ambient and elevated CO2 are shown. Figure2 is the only place where efflux of post-treatment C is shown. Table 3 shows the rates of the components of soil respiration (neither shown in either fig 1 or 2) and their relative contribution (not shown anywhere else). Figure 3 shows the rates of the components of soil respiration as in table 3 (except for pre-treatment C rates). We could eliminate rates from table 3 but we think is best to keep the numerical rates in the case further investigators find the numerical information useful.

Figure 1 could be better interpreted if also include soil moisture and temperature variability at the time.

Moisture and temperature information are given in Table 3. Considering that 2 years two treatments and two diurnal rates are shown we think adding more information would add unnecessary complexity to the graph.

Figure 4 – show legend for black/white bars. Suggestion = Might be better interpreted if you normalized your emissions for each case and then compared normalized values. Otherwise, explain the temporal trend observed (i.e. decrease over the growing season in post treatment, but increase in pre treatment).

We have clarified explanations of these trends in the discussion. Relative “normalized” values are given in table 3.

6. Technical/editorial comments: the writing can be improved. a. Some of the sentences are too long (ex. L 8-12,Abstract; lns 3-10 and 15-18,

Amended as suggested

Introduction; ln.2-5, page 2880 is too long + the sentence after is not clear; ln. 15-20, pg. 2893)

Amended as suggested.

b. others are confusing/unclear: ex. Paragraph 3.1. line 11 = should “-5%” read +?; ln15-17, pg 2879 unclear, remove one of the “both”; ln.5-9, pg. 2877 unclear, rephrase;

Amended as suggested.

ln 9-10 Abstract – disagree – I think there was more emphasis on biotic controls of Rs as opposed to abiotic lately.;

We fixed the inconsistency.

ln.13-14, pg.2878 unclear; ln.5-9, pg. 2880 unclear; ln.5-6, pg. 2884 unclear; ln.

21pg.2888 “significantly”; ln. 25 pg.2888-2889 no clear; ln.21, pg. 2889 – results refer to enriched or ambient? ; paragraph 3.5 too long and repetitive; ln. 8-10 check the works by R. Vargas, M. Carbone, D. Gaumont- Guay, P.Jassal, G.Saiz;

We have incorporated these comments in the revised manuscript and merged 3.5 with 3.6.

ln.18, pg. 2893 “in situ” refers to which measurements? My understanding separation was done based on lab incubations of separated sieved soil samples – that is not “in situ”. Please clarify;

Incubations were done in the field but not in the soil. In situ is not appropriate here and has been removed.

ln.26 pg. 2893 to ln.2, pg.2894, not clear; ln.5-6 pg.2894 “presence” or “absence” not clear; process-based Q10 approach? Not clear – ln.12 p.2894.; ln. 8-10 – not clearly shown in the ms, as stated.; ln.10-13. Pg. 2895 – reference?; ln.26-27, pg. 2896 unclear; ln.1-4, could skip.;

We have clarified these points.

sections 3.1. and 3.2 seem to be repetitive;

They aren’t as one refers to seasonal and the other to diurnal variation in Rs. We have further clarified the text.

ln. 13-18, pg.2894, from what I recall, in Subke et al 2006 publication, studies that separated Rs components using C-13 methods, were not consistent with the other methods – you used C-13 here, so how does your study fit with the rest?

We have addressed these issues in the revised manuscript.

7. Interesting points to keep and elaborate on: 21-22, pg. 2895; ln.20-23, pg.2896; ln.10-14 pg.2897. 8. References – did not check. Thank you.

Interactive comment on Biogeosciences Discuss., 8, 2875, 2011.