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Biogeosciences
Discussions

Interactive comment on "Seasonality in a boreal forest ecosystem affects the use of soil temperature and moisture as predictors of soil CO_2 efflux" *by* S. M. Niinistö et al.

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Replies to the specific comments from Referee 1

Thank you for great comments and questions! We apologizes for the delay in reply. We wanted to reply to comments by the two reviewers at one go because the discussion would have closed immediately after the input from the second reviewer. There have been no comments from Referee 2 so far, so we decided to reply to the first reviewer's comments now.

p2, line 11:

Spring and early summer (belonging to snow-free period) refer to May and June but the sentence in question in Abstract on page 2, l.10-11 needs to rewritten so that the idea of figure 37 is included in this sentence - very useful that Referee 1 pointed the sentence out.

rephrased (page 2, l10-11): Soil CO2 efflux was greater during the late snow-free period i.e. in August–October than in the early period i.e. May and June at a given temperature

(In addition to the monthly analysis, this was shown also when data was grouped in two- or three-month groups (early versus late snow-free period), results not included in this shortened version of the manuscript.)

p4, line 1:

Linder and Lohammer's results from the boreal forests in Sweden are applicable to the similar climate of our study site in Finland. We have shortened the sentence in question too much, we will return the reference to Sweden, i.e. to imply that boreal conditions in the Swedish study in question are applicable to conditions at our site in Finland:

The new version of the sentence to be included in the manuscript: In a Swedish boreal pine, for instance, 95% of the annual carbon gain can be obtained during the six warmest months (Linder and Lohammer, 1981).

Although photosynthesis may have started on our site before melting of snow cover and soil frost in May or continued after the formation of soil frost and constant snow cover at the end of October at our site, we do not expect carbon gain to have been significant outside these six snow-free months of the years we studied. Experimental data from our site supports this too (e.g. Wang et al. 2004. Tree Physiology 24:19-34).

Since the measurements for this manuscript were made, some autumns have been warmer than before in the study region, but the original assumption is valid for the results presented in the manuscript. A recent study of northern ecosystems indicate

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that the recent warming of autumn/early winter months could lead to an increase of respiration in northern terrestrial ecosystems (Piao et al. 2008. Nature 451: 49–53) but we do not expect that the warmer autumn or early winter months would have increased photosynthesis or carbon gain because solar radiation is rapidly decreasing in autumn as before. Springs could be getting warmer too because of a changing climate and carbon gain could occur earlier, but at the time of the measurements we had no indication of that.

p4, line 11:

Our hypothesis was that (soil) temperature and soil moisture affect soil CO2 efflux but there are other seasonally varying factors as well so that a simple or complicated predictive model based on momentary soil temperature or soil moisture is not enough. Our hypothesis was based on literature, on points made before the hypothesis in Introduction. At the time of the experimental planning we did not have many of the methodological tools that have recently been developed and become affordable but we attempted to measure also other variables than soil CO2 efflux and soil temperature and moisture. For modelling purposes, these proved out to be difficult to make good use of, mostly because of different time steps and lags: For instance, our monthly sampling of aboveground litterfall was not frequent enough to be incorporated as a variable in soil CO2 models but also time lags that would have been appropriate to use with this independent variable were difficult to define. However, results from the litterfall sampling and later photosynthesis measurements (Zha et al. 2007. Ann. Bot. London, 99, 345–353) on our site helped to interpret results on the seasonality and they have been incorporated in Discussion.

Monitoring of root and mycorrhizal fungi production would have been important too but we did not have resources for it as it is very labour- and time-consuming, so we had to report our scattered observations on efflux bursts originating from fungal growth and in Discussion rely on root growth studies that had been made earlier in the same area/region by our colleagues. We have stressed the importance of root and mycorrhizal fungi production in context of modelling soil CO2 efflux at the end of Discussion too.

p.4, line 25: University of Joensuu -> University of Eastern Finland, quite so.

p.5, lines 13–18: I measured needle litterfall and soil characteristics reported on p.5 for this study. They have not been previously published.

p.5, question of plots with different tree densities: We are preparing another manuscript on spatial variability of soil CO2 efflux based on the same experimental set-up at our site in which we analyze differences between plots in more detail. Differences in plot averages of soil CO2 efflux between three plots were surprisingly small as can be seen in Fig. 1a, 1b,1c and 1d in which standard error bars represent variation between plots. Differences were also consistent; Plot 2 had the greatest efflux and Plot 3 the smallest. Inclusion of all three plots in this study on temporal variation of soil CO2 efflux permitted us to have more observations i.e. data points per day (temporal coverage or coverage of different combinations of conditions) and also gave the measured level of soil CO2 efflux and the developed models some spatial credibility. One of our aims was to provide an estimate of the level of soil CO2 efflux in this forest. A part of variation in soil CO2 efflux that was not explained by variation in independent variables in our models, was actually variation between plots. The variation between plots was not great, for only 10% of the variation in efflux remained unaccounted for in Model 8, for instance (Table 2), of which not all can be assumed to originate from differences between plots.

p.6, line 13: Description of the stand in Mekrijärvi for model evaluation: We will describe the additional stand in Mekrijärvi for model evaluation with more care as Referee 1 suggested. This would be good to be added on p.5 for example after the paragraph where the stands on plots in Huhus are described.

A new description to be added: "The additional measurements for model evaluation purposes were made in another Scots pine forest, at some 30 km distance in Mekrijärvi

Research Station in 2000. The measurement plot had the same surface area and an identical layout of 10 randomly chosen, permanent measurement points as the plots in Huhus had. The Mekrijärvi stand was close to the developmental stage of Plot 3 in Huhus, the pole-stage, but pine trees were younger, i.e. 25 years in average. Trees were also smaller, with an average diameter of 5.1 cm. The stand had a clustered structure with 4500 trees ha-1. The Mekrijärvi site on podsolized sandy loam, with thinner litter and organic layers, was somewhat poorer in nutrients compared to Huhus, but similarly a well-drained site. Surface vegetation and soil are described in more detail in Niinistö et al. 2004."

p.6, line 15: Are there sufficiently winter measurements?:

We will add a mention to Discussion that because of low frequency of monitoring, there is some uncertainty in winter fluxes.

Winter measurements were not made as frequently as measurements during the snow-free period as Referee 1 pointed out. This was due to the focus of the study and resource allocation following it: We focused on the snow-free period of the year in this study but wanted to be able to present some annual estimates of soil CO2 efflux, mainly for model comparison purposes but also for a comparison of the level of measured efflux with other field studies. The measurements in winter were more time and labour consuming, work of two days for one set (day) of measurements that one could not carry out alone. We had a possibility to have a field assistant during the snow-free period to help us with measurements but not in the winter. On the other hand, we expected the day-to-day variation to be smaller in winter than during the snow-free period so that fewer measurements would suffice.

In the context of annual sums, we noticed that the assumed level of winter efflux had a greater effect on the annual sum than for instance correcting for a clear underestimation of efflux in July-August (efflux peak period) had (p.15, l. 6-8). With this we wanted to show also how the uncertainty in winter fluxes affected annual estimates.

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As a reply to Referee's question, we analyzed some additional measurements made in Novembers of the years 1997-1999. They had been discarded because a better methodology of larger chambers, longer efflux measurements combined with measurements on CO2 concentration in snow was later used, as reported in the manuscript. The November measurements were made on thin snow cover of the time either on summertime collars or next to them depending on snow depth, with the same methodology than summertime efflux measurements but with two minutes longer duration of measurement.

Plot averages of efflux in snow covered conditions in November ranged from 0.014 gCO2 m-2h-1 at -10C (air temperature), i.e. in cold conditions, to 0.073 gCO2 m-2h-1 at 0C the following week. Although a number of observations is too low (n=13) to make reliable conclusions, there was a clear trend of an exponential relationship between efflux and air temperature (R2 of the linear regression between LnEfflux and air temperature=0.94). With the help of this relation and average air temperature for November, we could estimate that efflux would have been 0,032 gCO2 m-2h-1 in average in Novembers (close to the average of November measurements of three years in snow-covered conditions of 0.035 gCO2 m-2h-1). Using the average of 0,032 for November, the estimated sum of winter CO2 emissions during the winter 1998-199 was 6% smaller than without inclusion of separate estimates for November, and the estimated annual sum of emissions was 1% smaller (summertime estimates based on quadratic temperature model or combined quadratic temperature and degree days model).Corresponding percentages for the winter 1999-2000 were 12% (smaller sum of winter emissions with November measurements) and 3% (smaller annual estimate of emissions).

Average air temperature could have worked as a predictor for soil CO2 efflux especially in November when the snow cover is formed; colder weather would mean a frozen crust on soil and milder weather a softer and possible more porous snow cover - i.e. in addition to soil temperatures, characteristics of the insulating layer could have affected measured emissions. However, the measurement methodology used in these November measurements was not as reliable as the one used later and the number of observations was too small for any kind of statistically valid relation with environmental factors.

Thus, we will rely on the winter measurements already presented in the manuscript although we agree entirely that the winter estimates could have benefitted from more frequent measuring, especially from reliable measurements made early in the winter. Luckily, conditions in December and January were closer to those of late winter with a deeper snow cover (*) so the differences between an actual efflux and estimates based on measurements made in February-April can be assumed to be smaller for December and January than for November. Fortunately, an inclusion of estimates for November did not have, in average, a great effect on estimated sum of winter emissions but the level measured in February-April (p. 9, l. 25-26) had still the greater effect (i.e. as reflected in difference between estimates for the winters 1998-1999 and 1999-2000 and in difference between annual emission estimates, p.15, l.5). We will add a mention to Discussion that because of low frequency of monitoring, there is some uncertainty in winter fluxes.

(*) Snow depths on 15th of each winter month (averages 1971-2000 for the region, measured 25 km from our study site): Nov: 7cm, Dec: 28cm, Jan: 49cm, Feb: 62cm, Mar: 67cm, Apr: 45cm (Drebs. et al. 2002) Drebs et al. 2002. Climatological Statistics of Finland 1971-2000. Climatic Statistics of Finland 2002 (1):1-99.

p.6, line 17:Clearing of snow prior to measurements in winter:

It is good to consider the effect of cooling/freezing of soil because of clearing of the insulating snow cover before chamber measurements of efflux in winter. In practice, it was not possible to clear all of the snow on the continuous moss cover, thus snowy and dormant moss remained as an insulating layer.

Also, the measurement were made on rather mild winter days, which made freezing of soil because of clearing of snow less likely. Air temperatures ranged from -4.2C to -1.5C at the time of measurements made in 1999 and from 0°C to +6.5C in 2000. Differences to the soil temperatures measured continuously at a site were small: In 1999, soil temperatures ranged from -1.2C to -0.3C in the surface i.e. organic layer and from –0.3C to -0.1C at the depth of 10 cm in mineral soil (i.e. some 18–19 cm from the moss surface) at the time of efflux measurements. In 2000, surface soil temperatures were 0C and temperature at 10 cm in mineral soil was +0.7– +0.8C at the time of efflux measurements.

p.6, line 27:

Questions raised by Referee 1 on warming and cooling temperatures are very appropriate. We have briefly discussed this on p. 18, starting on l. 17, as one possible explanation to differences between May and October. Yet the difference between June and August or September would have been largely unaffected by this phenomenon.

The same phenomenon happened on individual days in May AND October, when a warming of surface soil layer during the day did not result in a similar increase in actual soil CO2 efflux as the temperature model would have predicted. The latter example is also mentioned on p. 18 with a mention that this kind of conditions produced the greatest relative overestimations of the temperature response models.

We tested temperature models also with soil temperatures measured deeper in soil (5 cm from the top of the mineral soil, i.e. some 9 cm from the surface of the litter and organic layers and 13 cm from moss tops) but the temperature of surface layer was a better predictor of the efflux. Also for temperature of the surface layer we had the auxiliary data next to each measurement collar measured at the time of efflux. A great proportion of efflux could have been assumed to originate from the surface layer which also supported the use of the surface temperature in modelling.

The weather station and the Vaisala station:

The weather station and the Vaisala station are one and the same, sorry about the confusion. This weather station manufactured by Vaisala was operated by our research group and located on our study site, between Plots 1 and 3; 50m north-northwest from the center of Plot 1 and 50m south-southeast from the center of Plot 3. We will correct the manuscript so that the weather station is the only term used and we will add that the weather station was located between Plots 1 and 3 at our site.

Soil moisture measurements: how relevant to measure outside plots? Any proof of minimal spatial variability?

The study area was on well-drained sandy till which made it relatively homogenous in respect of soil water content. Terrain was mostly very flat but parts of Plots 1 and 2 were on a gradual slope. Locations of soil moisture measurements were chosen with the microtopography of Plots for soil CO2 efflux measurement in mind: Plot 3 was located on flat terrain thus soil moisture was measured next to it on flat terrain whereas the micro-topography of Plots 1 and 2 was more variable so soil moisture sensors in the area next to Plot 1 were placed on gentle slopes and small depressions that were similar to those found on Plot 1. Tensiometers were located some 2 to 3 meters from the plot boundaries to avoid excess trampling inside the plots because of the maintenance and measurement of tensiometers and to leave more room for soil CO2 efflux measurement collars. Precipitation sensors were placed in the vicinity of tensiometers next to Plots 1 and 3 as well. Also water-content reflectometers were placed in the same area with the tensiometers next to Plot 1 which saved two quite large pits being dug next to soil CO2 efflux measurement collars within the plot boundaries.

Soil matric potential measurements (in kPa, with tensiometers) made next to Plot 1 correlated strongly with measurements made next to Plot 3 on soil CO2 efflux measuring days: For 1999, Spearman's rho was 0.97 ($p<0.001$, $n=45$), Pearson correlation 0.97 and according to a linear regression analysis, variation in soil matric potential next to Plot 3 explained 97% of the variation next to Plot 1.

Distance between these two small areas for soil matric potential measurements was some 90 m. As the correlation was strong between these two measurement areas, it would be safe to assume that these areas for soil moisture measurements could represent the temporal pattern of soil matric potential or soil moisture also on the plots for soil CO2 efflux measurements that were located only some meters away from soil moisture measurement locations.

A point-to-point variation would not have affected the modelling results because in modelling, plot averages were used for the plots for soil CO2 efflux measurements (3 plots/ study area) as well as for instance for tensiometer plots (2 tensiometer areas/ study area or later in modelling an average of two tensiometer areas equalled a study area average).

A comparison between different methodologies (thus different locations within the study area as well) was encouraging too and supported our conclusion that spatial variability did not prevent the use of soil moisture measurements next to the plots to represent temporal variation of soil moisture inside the plots: Correlation between soil moisture of uppermost mineral soil determined from gravimetric samples of uppermost mineral soil and from tensiometer measurements of soil matric potential (average for the study area) was also good (Spearman's rho 0.85) although because of the destructiveness of the sampling, gravimetric sampling could not have been repeated on same spots week after week and it covered a larger area around and between soil CO2 efflux plots. Correlation was also good between soil moisture measured by water-content reflectometers and that measured by tensiometers (Spearman's rho 0.89-0.97 for different depths).

Degree days and temperature sum:

Degree days and temperature sum are the same thing, we will correct p.8, l.25: Degree days i.e. temperature sum was calculated according to....

p. 9 line 5: We will add a reference (Zar, 1999) to show that comparison of regression coefficients and intercepts with a help of Student's t-test has been recommended for instance by Zar (1999). Reference to be added: Zar, J.H.: Biostatistical Analysis, Prentice-Hall, Upper Saddle River, NJ, USA, 1999. We have used the same method in a previous paper on effects of atmospheric CO2 enrichment and air warming on soil CO2 efflux when we compared the treatments (Niinistö et al.2004. Global Change Biology 10:1363-1376).

p10 line 28: At the beginning of the study we worried about a possible wear of surface cover, for instance, inside the collars so at the beginning of the second snow-free period we chose 5 new measurement locations on each plot in random and continued to measure 5 old locations too (randomly chosen out of 10 original points). These 10 points (5 new + 5 old) were measured twice a measuring day but 5 additional, "disregarded" old points were measured on two of the plots once a day for an analysis of spatial variability. There was not enough time per day to continue to measure 15 points twice a day on all three plots. Preliminary analysis on spatial variability did not show differences between old and new collar locations so for this study submitted now to Biogeosciences we used an average of 10 points per a plot as an observation except in the detailed analysis on temperature response in July (p10 l28-p11 l3). Because of finding no evidence on the effect of wear or old/new collars to the efflux, we have removed this description from the manuscript at the moment to shorten the text that has been quite long and has received comments suggesting shortening.

p 15: Effects of tree species, age and soil nutrient conditions on soil C efflux?

These are very interesting questions. In Discussion, we have attempted to relate the level of soil CO2 efflux measured in our study firstly to measurements made in other boreal Scots pine forests and secondly to measurements in other kind of boreal forest stands.

There is not yet enough data on the effect of tree species on soil CO2 efflux in northern forests because most of the studies in upland forests in comparable latitudes to ours

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have been made in Scots pine forests that are the most common type of forests. We have included a citation to Domisch et al. (2006) who have studied a Norway spruce site in our region but there is not yet enough data to do a proper comparison between tree species.

In our region, tree species and soil nutrient condition are linked because fertile sites have in general Norway spruce as the main tree species and less fertile sites have Scots pine but very little is known for instance on the effect of soil nutrient status on soil CO2 efflux in Scots pine forests along a nutrient gradient. Previous studies elsewhere in boreal and temperate forests have shown a reduction in soil CO2 efflux with a fertilization treatment. Reduction has been thought to possibly originate from a smaller mass of tree root and from changs in understorey vegetation/ground vegetation. On the other hand, productivity is smaller on sites that are naturally less fertile compared to sites with higher level of nutrients in soil so one could assume that both carbon uptake and release could be smaller on less fertile sites. Also ground or understorey vegetation could play a role in the effect of nutrient status as well as in the effect of age; there is a different species composition according to the site fertility and growing conditions for understorey are different in stands of different age and development stage.

There is a recent study on the effect of stand age on soil CO2 efflux in managed (similarly to ours) Scots pine stands in Finland in which a slight declining trend with a stand age could be seen (Kolari 2010) but more research would be helpful. Kolari, P. 2010. Carbon balance and component CO2 fluxes in boreal Scots pine stands. Dissertationes Forestales 99. 43 p. http://www.metla.fi/dissertationes/df99.pdf

These are indeed very interesting questions that require further research. To avoid our Discussion getting any longer, we plan to leave these questions to our manuscript in preparation (on spatial variability) and focus on seasonality of efflux in the discussion of this current manuscript under the review.

p17 2f: Bands measuring tree diameter growth were a part of our experimental set-up but not yet at the beginning of the study.

p 23 l 7: We will correct the sentence to read: In addition, watering experiments could give valuable insight into the role of soil moisture also in boreal forest ecosystems.

Background of the original sentence was "boreal forest ecosystems that have been thought to be humid". According to an earlier hypothesis, current at the time of the planning of our experiment, boreal forests were humid so the role of soil moisture were to be small. This was thought to be true in the Finnish conditions where summer droughts in forests were not common as they were in boreal forests in more continental boreal climates. The third year of monitoring of soil CO2 efflux at our site was surprisingly dry: Precipitation in June–August was 49% of the 30-year average and August of that year was the driest in 30 years. Our three-year monitoring period covered thus "rather average", "wet" and "dry" snow-free periods. Based on our results, it would be, however, interesting to carry out a watering experiment in a wet year too to see if one could distinguish between the effect of advancement of growing season i.e. phenological development and that of high soil moisture. This idea also is behind the last sentence in Discussion - together with an acknowledgement to excellent watering experiments on temperate grasslands, such as by Liu et al. 2002, for instance, for which a mention has been omitted to shorten the text.

Liu X, Wan S, Su B, Hui D, Luo Y (2002) Response soil CO2 efflux to water manipulation in a tallgrass prairie ecosystem. Plant and Soil 240: 213–223.

Fig. 4 caption: We will correct the caption to read:

Fig. 4. Modelled temperature response of pooled three-year data on soil CO2 efflux (1997–1999). Solid lines illustrate linear regression model for natural logarithm of soil CO2 efflux, LnFlux and a dashed line indicates quadratic regression model for LnFlux. A dotted line indicates the Lloyd & Taylor (1994) version of the Arrhenius function fitted for soil CO2 efflux observation data. Observations are plot averages. Soil temperature (Ts) was measured next to collars with hand-held probe. Equations for the models in

the upper graph are given below the figure. Q10 of the linear regressions was calculated as Q10 = e10× b1, b1 from the temperature response model formulated as LnFlux= b0 + b1×Tsoil. All relations for which R2 is given were statistically significant, as were regression coefficients and constants ($p<0.001$).

p 37, Fig. 5A: Yes, we agree.

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