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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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Thank you for the encouraging review and helpful questions which will help to clarify many points in the manuscript. Some of the questions raised by Referee 2 were the same that Referee 1 brought to the discussion earlier so we have used the text prepared for Referee 1 in replies to Referee 2 as well.

We fully agree with Referees 1 and 2 that the question of the measurement depth of soil temperature used in this manuscript merits a more thorough discussion. We have now asked for an extension to the dead line at Biogeosciences Discussion so that we can look into this question in detail. We will post another reply to referees that will include

our reply to the temperature questions and our related suggestions for additions to be made in the manuscript. In the following, we reply on questions and comments on other topics raised by Referee 2.

METHODOLOGY

OTHER SEASONALLY VARIABLE VARIABLES

Our study would have benefitted of having more seasonally variable variables that could have been used in identifying factors related to temporal variation of soil CO2 efflux and in modelling of soil CO2 efflux as Referee 2 quite correctly pointed out.

We will add two sentences to Discussion (for instance, to page 22, row 12): Inclusion of additional, seasonally variable variables such as litter input in our models proved to be difficult because of different intensity of measurements. Also, time lags after which these factors could be considered to influence soil CO2 efflux, were difficult to define.

We discuss this topic in more detail in the following:

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 litter fall 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 litter fall sampling and later photosynthesis measurements (Zha et al. 2007) on our site helped to interpret results on the seasonality and they have been incorporated in Discussion. Monitoring temporal variation 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.

Common problems with many of the variable candidates we considered (and monitored or attempted to monitor) were 1) too low frequency of possible observations and 2) unknown time lags/mechanisms after which these factors could be considered to influence soil CO2 efflux. For instance, monitoring phenology of trees and dwarf shrubs using traditional visual point observations could produce only few observations per snow-free period such as bud burst, stages of leaf/needle elongiation, berry formation etc. To incorporate these to models based on some 12 weekly observations of soil CO2 efflux and temperature for 6 months would have been difficult. Measurements of tree diameter were continuous for the snow-free period but unfortunately growth was so slow and annual increment very small (1 mm) that we could not get reliable observations from day-to-day or week-to-week growth that could have been used. In Discussion, however, we used available information on phenology in discussing our results (such as timing of tree diameter growth and litter input patterns) although we did not succeed to include them as variables in our models and analyses.

In the following, we will discuss Referee 2's suggestions in detail:

PAR or solar radiance is an example of a variable that we would have had more observations but its influence to temporal variation of the combination flux such as soil CO2 efflux would not have been entirely direct. How plants used assimilated carbon would have an influence: From other studies carried out in our research group, we had learned that trees in our type of ecosystem tended to have high respiration rates (compared to photosynthesis) during the formation of new needles early in the summer i.e. June so that the maximum of photosynthesis (and carbon gain) was usually observed in July when new needles were fully grown - not earlier although amount of PAR could have usually been the highest already in late June around the vernal equinox. Thus,

high solar radiance would not have automatically meant high carbon gain by trees if monitoring frequency similar to soil CO2 efflux measurements was to be used. Similarly, the effect of PAR would have been indirect through dwarf shrubs and actual flux of carbon from tree or dwarf shrubs to soil unmeasurable with the methods we had in hand. For many possible lags in the effect of photosynthates to be possibly seen in soil CO2 efflux, the intensity or frequency of our soil CO2 efflux measurements was not great enough (e.g. lags from hours to a couple of days).

Yet, PAR at the ground level would have had a direct effect through moss photosynthesis although only dark respiration of mosses was included in surface soil CO2 efflux measurements (estimated 10% of the average flux in 1999 assuming a similar optimum of water moss content for dark respiration as for photosynthesis). However, we tried to find ways to include environmental factors such as solar radiation in our analysis of factors explaining temporal variation of soil CO2 efflux but did not succeed with the intensity of the soil CO2 efflux measurements that was feasible with our experimental set-up and equipment. In Discussion, we use our observations on temporal patterns of tree photosynthesis at our site to explain temporal pattern of soil CO2 efflux in the summer although we were unable to use PAR in actual analysis or modelling. In addition, the effect of soil temperature on temporal variation of efflux was quite overwhelming - most likely partly because soil temperature acted as a proxy for many covarying processes in the ecosystem - so to distinguish effect or existence of additional factors was challenging.

Root growth would have been one of the most interesting factors to be included in the analysis and we fully agree with Referee 2 with this comment of his too. Monitoring dynamics of root and mycorrhizal fungi production and their inclusion in soil CO2 efflux models could have also revealed interesting similarities and dissimilarities with temperate forest ecosystems, for instance. Unfortunately, we did not have resources for monitoring temporal variation of root growth or microbes such as fungi during our experiment because root and microbial analyzes were very labour-intense and thus quite expensive. We had one full-time researcher, Sini Niinistö, and one part-time local field worker in summertime.

Luckily, our colleagues, Finnish forest root scientists Prof. Heljä-Sisko Helmisaari and Dr. Kirsi Makkonen had previously studied temporal variation of root growth with monthly sampling near Mekrijärvi Research Station in the same area and we could use their results in Discussion. To include monthly values of root growth in modelling analyses (with more frequent soil CO2 efflux monitoring and strong temperature dependency) would have been difficult as we discussed above in relation to phenology observations but at the time monthly monitoring would have been most likely the only (methodological) possibility had we had the financial resources for it. I also discussed with Prof. Helmisaari about her recent results and views on the matter, and in relation to our results, and wrote our Discussion accordingly.

For microbial dynamics, especially fungi, we could use excellent Swedish studies in Discussion which was fortunate because it seemed that there were not many studies on temporal variability of microbial activity in boreal forest ecosystems but many microbiology studies concentrated on one point in time and to microbial diversity, for instance. In addition, studies in boreal forests in Sweden are most often carried out in conditions that are similar to ours i.e. climate, vegetation, length of growing season, soil properties and forest management are comparable.

We were aware of the importance of roots at planning stage of the experimental setup but because of limited resources as explained above, we took and analyzed root samples only at the end of the experiment in October 1999.

The samples were taken from each measurement collar of soil CO2 efflux and cores were divided into organic humus layer (collected for its entire depth, 0.5-4cm) and mineral soil samples taken at depths of 0-5 cm and 5-10 cm in mineral soil for root analysis. Roots were sieved, washed, identified under a microscope and divided into classes according to guidelines from root scientists (Helmisaari and her group): fine

roots (all species), living pine roots, living dwarf shrub roots, dead pine roots and dead dwarf shrub roots. Each of the pine and dwarf shrub classes was further divided into four diameter classes. This took several months of laboratory work, so we managed to get two plots analyzed for their root mass in the organic layer and in the uppermost 5 cm of mineral soil.

We are currently preparing a manuscript on spatial variability of soil CO2 at our site for which we have preliminary results that show a statistically significant ($p<0.05$) correlation between soil CO2 efflux in autumn 1999 and mass of fine roots, all living roots and all roots in the organic layer at the end of October 1999 ($n=19$). No correlation was found between efflux and mass of fine ($p=0.72$) or living roots ($p=0.42$) in the upper 5 cm of the mineral soil but a weak correlation between efflux and all roots ($p=0.06$).

Combination studies of simultaneous monitoring of soil CO2 efflux and root and mycorrhizal fungi production in boreal forests, preferably for multiple years, would be excellent in increasing our understanding of the temporal/seasonal variation of soil CO2 efflux. Perhaps results from this kind of studies, that are very resource-demanding, will soon be available. The first girdling studies (Högberg et al. 2001, Högberg and Högberg 2002, Bhupinderpal-Singh et al. 2003) were an excellent example of studies that resulted in new information on temporal variation/mechanisms behind forest soil CO2 efflux. They had an additional advantage of being carried out by a group that included microbiologists.

We also measured a distance to the nearest trees for each measurement collar of soil CO2 efflux on our plots and this proxy as suggested by Referee 2 too has successfully been used in analyses for the manuscript on the spatial variability. In the analysis for the manuscript on temporal variation (this manuscript under a review for BG), we used plot averages in building predictive models on temporal variability of efflux which made benefitting from information on the location of individual collars in relation to trees (fixed, non variable distance) difficult. In addition, distances did not vary from a measurement to another or from a season to another because there was no ingrowth (new trees) and mortality was zero except in the young and dense stand of Plot 3 in which there were some dead trees at the end of the experiment. Even on Plot 3 the mean distance to the closest trees did not vary much at all, i.e. although one tree might have died in vicinity of a collar during the course of experiment, another tree was so close that the average distance to the three closest (living) trees remained almost the same. Model evaluation data was measured only on Plot 1 in 2000 which makes it difficult to relate an average distance to the trees on a plot to an amount of underestimation of efflux in July or something similar in order to find out that could one use tree data to find support for seasonal differences that would be related to root exudates, for instance.

We regret that we did not monitor soil moisture during the first snow-free period of the experiment in Huhus. We had some methodological difficulties in the rush to get everything started and running so we had to rely in labour-intensive and non-continuous method of tensiometers that we started to test at the end of August of the first year, too late for that year to be included in modelling analysis with multiple independent variables explaining temporal variation of soil CO2 efflux. In the consequent years, however, we put effort on monitoring of moisture of different soil horizons and moss cover, compared three different methods, worked on the effect of stoniness on different methods and carried out a soil-specific calibration for soil content reflectometers. Now, we would add an irrigation study on top of if we had a chance - perhaps at our site in Mekrijärvi that we used for model evaluation because getting enough water to irrigate a forest plot could be feasible there but not in Huhus.

Bhupinderpal-Singh et al. 2003. Tree root and soil heterotrophic respiration as revealed by girdling of boreal Scots pine forest: extending observations beyond the first year, Plant Cell Environ, 26, 1287–1296.

Högberg et al. 2001. Large-scale forest girdling shows that current photosynthesis drives soil respiration, Nature, 411, 789–792.

Högberg and Högberg. 2002. Extramatrical ectomycorrhizal mycelium contributes one-third of microbial biomass and produces, together with associated roots, half the dissolved organic carbon in a forest soil, New Phytol., 154, 791–795.

Makkonen and Helmisaari 2001. Fine root biomass and production in Scots pine stands in relation to stand age, Tree Physiol., 21, 193–198.

Zha et al. 2007. Total and component carbon fluxes of a Scots pine ecosystem from chamber measurements and eddy covariance, Ann. Bot. London, 99, 345–353.

SOIL TEMPERATURE

We fully agree with Referees that the question of the measurement depth of soil temperature used in this manuscript merits a more thorough discussion. We have now asked for an extension to the dead line at Biogeosciences Discussion so that we can look into this question in detail. We will post another reply to referees that will include our reply to the temperature questions and related suggestions for additions to be made in the manuscript.

SOIL MOISTURE MEASUREMENTS

We appreciate the concern expressed by Referee 1 and 2 about the spatial credibility of soil moisture measurements and we will add the following sentences to the end of 2.3 (Soil moisture measurements, p.8, r.6) about the correlation between soil matric potential measurements taken next to Plots 1 and 3:

"Soil matric potential measurements made next to Plot 1 correlated strongly with measurements made next to Plot 3. For the year of great fluctuations in soil moisture,1999, Spearman's rho was 0.97 and Pearson correlation 0.97 ($p<0.001$, $n=45$) between soil matric potential averaged for soil column between depths 5 and 30 cm on soil CO2 efflux measuring days. According to a linear regression analysis, variation in soil matric potential next to Plot 3 explained 97% of the temporal variation next to Plot 1."

In the following, we describe our soil moisture measurements in more detail:

The study area was on well-drained sandy till which made it rather homogenous in respect of soil water content. Terrain was mostly very flat but parts of Plots 1 and 2 were on a very 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.

We measured soil matric potential with a minimum of 8-10 tensiometers per a tensiometer plot which made a total of 18 tensiometers, placed at different depths from 5 to 30 cm. Because of the drought in 1999, we increased the number up to a total of 30 at the beginning of (dry) September 1999.

Precipitation sensors connected to the weather station were placed in the vicinity of tensiometers next to Plots 1 and 3 as well.

Also water-content reflectometers (Campbell's CS615's) were placed in the same area with the tensiometers and precipitation sensors next to Plot 1 which saved two quite large pits being dug next to soil CO2 efflux measurement collars within the plot boundaries. We wanted to avoid severing root and mycorrhizal connections within plots.

The sensors in two locations were connected to a data logger. At time of installation, we considered the distance of some 90m from the area next to Plot 1 to the Plot 3 to be likely too great for reliable functioning of water-content reflectometers. Campbell Scientific mentioned an increased risk of damage due to lightning with cable lengths over 50m. We did have a couple of sensors damaged because of lightning and had to replace them over the years but this happened despite of choosing to be on the safe

side with cable lengths by choosing 15m. However, this meant that we measured soil moisture continuously with a total of 8 sensors at two locations in the vicinity of Plot 1 and not on all measurement plots for soil CO2 efflux. Number of sensors had to restricted because of they were expensive.

Plot 1 was favoured (as a location for Campbell's sensors instead of Plot3) for two reasons: First, we had placed our plots in the forested area according footprint calculations of a source area for an eddy covariance measurement at the weather station. Midpoints of Plots 1 and 3 had been placed at the distance from which the most flux measured by eddy system was estimated to originate from (50 m), with a distinction that Plot 1 was placed to the compass direction that was the prevailing wind direction in the greater area whereas Plot 3 was placed in the opposite direction. Plot 2 was placed in the same direction with Plot 1 in relation to the eddy covariance measurements, but 50 m further from Plot 1. Although eddy covariance measurements were not running full time in the first years of the experiment, the idea originally was to place the plots for soil respiration measurements so that they could be used in context of ecosystem respiration. Second, Plots 1 and 2 were located in the same stand thus it made sense to have the continuous measurement of soil temperature and moisture in that stand.

Fortunately, 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, we think it 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.

Advantages of water-content reflectometers was that they covered a good sized volume with their 30-cm long rods, that the measurements were continuous, thus available for estimating annual soil CO2 efflux and that their data could be used in other studies at the site because they did not require us to be present as tensiometers and gravimetric sampling did. They also did not have problems with freezing (water inside the tensiometers) and drought (water disappearing quickly inside tensiometers) that tensiometers had which made them unreliable at times, i.e. more gaps in data that we would have wished for. We did install more tensiometers during the unexpected drought in 1999 (worst in 30 years) and got enough readings.

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). At the final stages of modelling, as presented in Table 2, temporal variation of a study area average, i.e. a site average, of soil moisture was used to explain temporal variation of plot averages of soil CO2 efflux.

We did separate analyses in which we used soil moisture observations derived from tensiometer measurements next to Plot 3 in connection with soil CO2 efflux observations (plot averages) from Plot 3 and soil moisture observations based on tensiometers next to Plot 1 with plot averages of soil CO2 efflux from Plots 1 and 2 but these stand-specific soil moisture values did not make a difference in the modelling outcome compared to site averages of soil moisture, i.e., they did not increase or decrease the relative importance of soil moisture as an independent variable and they did not have an effect on R2 of the model. Although actual soil water content on Plots 1 to 3 would have been more accurately represented with sensors next or within each of these plots, it seemed that sensors next to the plots and the site averages of these sensors depicted temporal trends accurately enough or the differences between tensiometer plots next to Plot 1 and 3 were so small that we did not gain from our attempt to use plot-specific soil

moisture observations in modelling. Thus, we either used area averages of tensiometer plots next to Plots 1 and 3 or "area" averages from water-content reflectometers placed close to Plot 1.

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 but not within soil CO2 efflux plots along chosen transects but in random in the vicinity of a transect. 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).

WINTER MEASUREMENTS

We appreciate Referee 1 and 2's comments on winter measurements. We will stress in Discussion that because of low frequency of monitoring, there is uncertainty in winter fluxes. (to be added on page 15, row 24). We will do some other additions to the manuscript too that are related to winter measurements, please see further below:

Winter measurements were not made as frequently as measurements during the snow-free period as Referees 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. If we were to carry out the study again, we would put more effort on winter measurements.

As a reply to Referee 1 and 2's question, we have gone through some additional, rather scattered 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.

To be added in Discussion:

"Auxiliary measurements in Novembers 1997-1999 (results not shown) 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 indicated that using observations made only during winter months with a well-insulating, i.e. thick snow cover could have lead to an overestimation of winter fluxes by some 10 % and of annual sum by some 1–3% depending on the winter in question. Thus more measurements with large chambers especially early in the winter would have been needed to acquire more accurate estimates of winter flux."

In addition, we will add to Methods:

1) "Winter measurements were supplementary measurements that were not used in modelling but were carried out to have annual estimates of soil CO2 efflux." (to p.6, r.15).

2) "Larger chambers that covered a surface i.e. source area of 3600 cm2 each and long measurement times were used to capture low winter fluxes for which the smaller surface area of the chamber that was used during the snow-free period could produce more erratic values". (to p.6, r.16).

3) that winter measurements were made "along a permanent transect on Plots 1 and

3" (added to p.6, r.15)

4) that the chambers were "placed directly on ground and sealed with some snow along the edges " (to be added on p.6,r.17).

We saw in practice that in conditions of low efflux the surface area of the chamber used for the snow-free period was so small that there was an increase of only some ppm's in the CO2 concentration in the headspace. Because the efflux can be slightly erratic in the field, although this was not a fault of the PP System's equipment which performed without a hitch during a calibration campaign with a steady efflux in laboratory-like conditions (Pumpanen et al 2004), we looked for a more reliable method to measure winter fluxes and collaborated with a research team at the neighboring Department of Biology. The team in question had successfully measured winter fluxes in peatlands and littoral zone of the boreal lakes, including upland soil. They lent us the equipment and we exchanged favours by helping each other with the work in field occasionally, i.e. transport of chambers in snow, snow shovelling, and measurements in field.

The small surface area of the standard chamber for the snow-free period would have also meant that the relative edge effect would have been greater too compared to the winter chambers that we used as "winter chambers" had a 45 times bigger surface area each. The edge effect would have had an influence in winter: In Novembers 1997-1999 when we tried to measure with the standard system in conditions of (little) snow, we were sometimes unable to use the actual permanent collars because of hardened ice bits and such making the use of the normally tight seal difficult. When placed directly on the frozen ground, unevenness of the ground would have a bigger effect on seal/edge effect with a small chamber than with a large, heavy winter chamber. Yet, the main benefit that we saw with the large chambers and long measurement times that they allowed for, was the better capturing of low fluxes because of the larger source area they covered.

We were happy, though, to see that low fluxes were of the same magnitude with the

two methods quite as Referee 2 pointed out. We do not think that using different chamber sizes (and different measurement periods) for the winters and snow-free periods caused inconsistencies in results because the winter measurements were not used in modelling, i.e. they were not combined with the measurements for the snow-free periods in analyses - except in summing up annual estimates for which they were intended for. 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 - and that also low fluxes in winter are worth of putting effort to.

We will continue to 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 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.

SPECIFIC COMMENTS AND TECHNICAL CORRECTIONS (numbered from 1 to 14)

1. p. 5, l.8. In chapter 2.1 we describe the study area, study site. LAI was measured as an average for different stands forming the study area on sandy soil, all stands Scots pine stands of different age. Plots for soil CO2 efflux are described in the next paragraph, 2.2.

We will look into possibility to assessing LAI for plots in Huhus and in Mekrijärvi with the help of needle mass models that have been built in our research group.

2. p.6, l.13. We will describe the additional stand in Mekrijärvi for model evaluation with more care as Referees 1 and 2 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. In addition, we will look into possibility to assessing LAI for plots in Huhus and in Mekrijärvi with the help of needle mass models that have been built in our research group.

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."

3. p.6, l.23. We will add: "..air of undisturbed snow packs was sampled with a syringe and a metal straw that had an opening at the end. The CO2 concentration of air sample was analyzed with infrared gas analyzer in the laboratory as described above."

4. p.7, l.11. We will correct: "... gravimetric water content of moss and upper soil layers including litter layer, organic i.e. humus layer and 0-10 cm of the uppermost mineral soil...." (mineral soil had accidentally been dropped of at some point)

5.p. 8, l.21-25. We will modify the sentence to read and add one sentence to explain. We will add also formulations of the temperature models that will help to understand the text as Referee 2 suggested:

"Natural logarithmic transformation of CO2 efflux (LnFlx) was used for both the linear and quadratic models tested because it linearized the temperature response and corrected for heteroscedasticity as noted previously (Howard & Howard 1993; Wang et al. 2003). Heteroscedasticity was observed when residuals of temperature response of untransformed CO2 efflux increased greatly as temperature increased but natural logarithmic transformation of CO2 efflux corrected this. A linear model was formulated as LnFlux= b0 + b1·Tsoil and a quadratic model as LnFlux= b0 + b1·Tsoil + b2·Tsoilˆ2. For comparison purposes, the Lloyd and Taylor (1994) version of the Arrhenius function, with three freely determined parameters, was fitted to the pooled three-year data (Fig.4)."

We will add also a corresponding additional reference that had been omitted when shortening the manuscript earlier:

Howard DM, Howard PJA (1993) Relationships between CO2 evolution, moisture content and temperature for a range of soil types. Soil Biology & Biochemistry, 25, 1537–1546.

We tried many different formulations of temperature response (as well as moisture response) during our analyses although they all are not discussed in the manuscript.

6. p.9, lines 13-14 and Fig. 1. There was a fault with the range of x-axis in Figs 1b, 1d and 1f. Their x-axes have now been made to start on the 1st of May 1999, as Fig.1h was. X-axis of Figs 1a,c,e,f starts on the 1st of May 1998 correspondingly.

In addition, there was a slight alignment error with the right column of subfigures in Fig.1 which caused that the drop in soil CO2 efflux on the 20th and 22nd of July 1999 in Fig.1b was not aligned with a drop in soil matric potential on the 22nd of July 1999 in Fig.1f. It was very good that Referee 2 spotted the error in alignment and in range of x-axes.

Soil matric potential was not measured on the 20th of July but observations from soil water-content reflectometers and gravimetric sampling of mineral soil and organic layer on that day and on the 22nd show that the drop in soil moisture was present for these two days with a drop in soil CO2 efflux. Soil water content reflectometers yielded values of 0.08 and 0.09 m3m-3 for these two days, low compared to 0.12 m3m-3 of the previous measurement days of soil CO2 efflux, July 15th that had been as warm (soil surface temperatures around 19-20°C) as the 22nd of July. We will add the date to the sentence in results, p.9, l.13-14.

So, a decline in soil CO2 efflux did not precede the decline in soil moisture in reality but unfortunately errors in alignment of graphs gave that expression. We will make tick marks longer on x-axes and try to widen the graphs too.

7. p.12, l.4 We will correct: It should be 0.08 m3m-3 as Referee 2 suggested.

8. We will add to Methods as suggested, on p. 7, l.11: "Water content of moss was monitored so that the contribution of dark respiration of moss layer to the surface soil CO2 efflux could be estimated."

We will add a few words to the sentence in question in Discussion too: "..... and assuming a similar optimum of moss water content for dark respiration as has been found for photosynthesis (see Silvola 1985, 1992)."

9. p.16, lines 6-7: I thought that diffusion through the upper "crust" of soil surface layer (that was most likely frozen although a thin layer, at least it felt frozen when I measured a thickness of snowpack with the metal straw) was so slow that CO2 that

had been accumulated in soil because of melt-freeze layers in snow that restricted diffusion above soil surface, could have been reflected in efflux that I measured after the removal of snow. Another possibility would have been to leave the measurement locations cleared for a day or something similar but there would have been a risk of freezing of deeper soil layers.

I will rephrase the sentence in question in Discussion: "The more layered snowpack early in 2000 could have caused CO2 to accumulate in soil more than it had under better-aerated snowpack in 1999, which could have still been reflected in measured efflux after a snow removal because of slow diffusion of gases through a frozen surface of soil."

10. That is a mistake in Table 2 (referring to the distance from the moss tops, which was closer to 6 cm, not 5cm), thank you for pointing it out. We will correct: "soil temperature at 1-2 cm in the organic i.e. humus layer (Tsoil)". We will correct the same in the caption for Fig. 1 and in the x-axis title in Fig.4.

11. We have now corrected the error in the range of x-axes in Fig.1b, d, f as explained above and will make tick marks more visible in all subgraphs in Fig.1. We will try to widen the subgraphs too.

12. We will correct the graph so that the unit on y-axis will be in g CO2 m-2 h-1.

13. and 14, figs 4 and 5a,b: After correcting units in Fig.3c and 3d as suggested by Referee 2, all untransformed soil CO2 efflux values in graphs are in gCO2 m-2h-1.

We prefer to keep Fig. 5b and lower set of graphs in Fig 4 as they are, with natural logarithmic transformation of the efflux because the temperature responses depicted in these graphs are linear only when related to LnFlux, not when related to untransformed efflux. Linearity of these temperature responses allowed for a statistical comparison of regression coefficients and constants as was done in Fig. 5b. If we drew exponential curves instead, it would be difficult to see how these could be statistically compared.

Interactive comment on Biogeosciences Discuss., 8, 2811, 2011.