Aguilos et al. present an interesting study in which peat soils were exposed to higher temperatures in order to test how soil respiration, and particularly its heterotrophic andautotrophic fraction respond to warming. Their study confirms the few previous studies that suggested a sustained positive climate feedback from these soils in response to warming and can provide interesting information about the response of both heterotrophic and autotrophic soil respiration. 'Can', because I think the analysis needs revision to strengthen this part.

Although I like the experiment, I think that several points should be improved. First, the English is far from perfect, and therefore I suggest asking a native English speaker to correct it.

We will subject our revised manuscript to an English-editing company prior to resubmission.

Further, Materials and Methods are not entirely clear (see specific comments below) and, most importantly, I think the analysis of the results need to be improved by using a slightly different approach for the calculation of basal respiration rate and Q10.

In the current study, the authors plotted the soil respiration data of the entire growing season (August-November 2007 or April-November 2008 and 2009) versus temperature to obtain 1 fit per year (i.e., 1 basal respiration rate and Q10 value per year). The key problem with this method is that seasonal variation in, for example, microbial community and root growth (the latter for control plots only of course) are also encompassed in the temperature response.

As demonstrated by for example Curiel Yuste et al. (2004), seasonal changes in (amongst others) plant phenology can largely affect this apparent temperature sensitivity (which is thus rather a compilation of temperature effects, phenological effects, etc.). As a consequence, the temperature sensitivities obtained from such calculations are useless when one is interested in, for example, interannual variability of the temperature response of soil respiration (a topic

discussed in the current manuscript) which would be strongly affected by confounding factors such as plant phenology.

Moreover, it is not appropriate for calculation of the temperature sensitivity of Ra via comparison of trenched versus control plots (as made in the current manuscript). The primary reason for the latter is that the seasonal Q10 of the control plots encompasses changes in root growth. This means that part of the difference in temperature response between trenched and untrenched plots is simply due to differences in belowground allocation of photosynthates and root growth that very likely covary with temperature on a seasonal scale. Hypothetically, it is even possible that Rroot did not respond to temperature at all, but that root biomass and root exudates covaried with temperature, inducing an apparent increase of Rroot on a seasonal scale.

Much more information and discussion on this topic can be found in Curiel Yuste et al. (2004), Davidson and Janssens (2006) and Vicca et al. (2009). To deal with this problem, authors could analyze their data similar to Vicca et al. (2009), fitting regressions for shorter periods (e.g., one regression per day). Like that, a seasonal course of the basal rate and the Q10 are obtained. This approach has several advantages. Besides being a must to obtain trustworthy basal rates and Q10's for Ra in this case, the Q10 computed in this way does not depend on plant phenological responses that covary with temperature, which makes it much more meaningful for interannual comparisons. Moreover, this approach also allows detecting seasonal variation in basal respiration rate, Q10 and in the warming response of the basal respiration rate and Q10 (in this case, for both Rh and Ra).

Given these major issues raised, we would like to convey our agreement on your comments:

1. That the issues you have raised were all valid and have been backed-up by other reviewers and we do not oppose your contentions.
2. That we agree that seasonal variation of microbial community and root growth are encompassed in the temperature response when we plotted soil respiration rate of entire growing season against the soil temperature to obtain 1 fit per year.
3. That your recommendation to follow what Vicca et al., (2009) did in fitting regressions for shorter periods to obtain trustworthy seasonal course of basal respiration rate and Q_{10}'s especially for autotrophic respiration is worthy to follow.

But before we jumped into the recomputation of our results based on your recommendations, kindly reconsider the succeeding explanations given hereafter:

a. Please understand that our major objective of conducting the experiment was to observe the response of soil heterotrophic respiration to elevated temperature (so that is why we had the unwarmed-trenched and warmed-trenched treatments). Please note that both chambers are soil heterotrophic chambers. If we come to evaluate it closely, we are more interested and gave more emphasis on the effect of warming to soil heterotrophic respiration and their temperature sensitivity.

b. That we just want to prove the major assumption made by model simulations whether it is true or not that the observed temperature sensitivity of soil respiration under the present climatic condition would still hold true in a future warmed climate. We evaluated this by comparing the curves between the unwarmed-trenched and warmed-trenched chambers only.

c. That we are more concerned on the behavior of the temperature-response curve in a future warming temperature rather than the contribution of each soil respiration components and their interannual variability.

d. The interannual variability you observed in our paper was purposely provided in order to show that even interannual variations in Q_{10} did not change much but basal respiration did increase thus, diving into a conclusion that if we predict the soil heterotrophic respiration rate in future warmer environment using the current relationship between soil temperature and heterotrophic respiration, the rate can be underestimated.

e. That because we had the opportunity to further determine the contribution of each soil respiration components, hence, we simply considered the control to be total soil respiration while the unwarmed-trenched chambers for the soil heterotrophic respiration. This has then provided us a chance to also compute for the autotrophic respiration by getting the difference of both treatments.

f. That given the difficulty in obtaining the Q_{10} and basal respiration rate for the autotrophic respiration without its confounding effects as what you emphasized, we have decided to remove Q_{10} for autotrophic respiration and its subsequent comparison with the Q_{10} of soil heterotrophic respiration from our results and discussion. In addition, we discussed the possibility on the shortcomings for partitioning hetero- and autotrophic respirations using data obtained by trenching method.

Following your and other reviewer's advice, we determined daily Q_{10} and R_{10} in the equation, $F_c=R_{10} \times Q_{10}^{((T_s-10)/10)}$, for each chamber by least-squares method using hourly soil respiration and temperature data within 15 days moving windows (previous and following 7 days each). Figure I shows the seasonal variation in Q_{10} and R_{10}, and Table I shows the summary of statistics of each treatment.
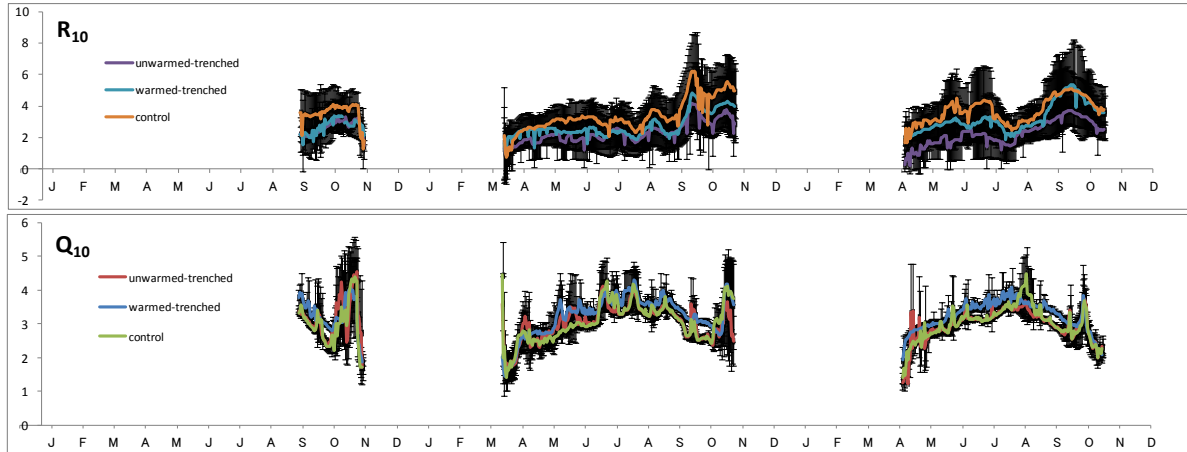
Figure I. Seasonal variation of Q_{10} and basal respiration rates at 10ºC (R_{10}). Colored lines indicate the daily average of 5 chambers for each treatment and vertical bars denote standard deviation for each day per treatment.

Table I. Temperature sensitivity (Q_{10} and R_{10}) of soil CO_2 efflux rate for each treatment. Whole data obtained from 5 chambers at a certain period (3 years or each year) were used to analyze the significant difference among treatments. Values were shown as average $\pm$ standard deviation (number of data). Tukey-Kramer HSD test was used to check the significant difference among treatments and values in a row followed by different superscript letters denote significant difference ($p < 0.001$) among treatments.

	Unwarmed–trenched	Warmed–trenched	Control
	Q_{10}		
3 years	3.03 ± 0.65^a (2275)	3.24 ± 0.60^b (2304)	3.03 ± 0.64^a (2307)
2007	3.20 ± 0.90^{ab} (311)	3.29 ± 0.75^a (303)	3.04 ± 0.78^b (312)
2008	3.03 ± 0.66^a (1095)	3.25 ± 0.65^b (1123)	3.05 ± 0.65^a (1114)
2009	2.98 ± 0.49^a (869)	3.21 ± 0.47^b (878)	3.00 ± 0.58^a (881)
	R_{10}		
3 years	2.50 ± 0.89^a (2275)	2.94 ± 1.52^b (2289)	3.59 ± 1.45^c (2307)
2007	2.65 ± 0.57^a (311)	2.68 ± 1.09^a (288)	3.64 ± 1.15^b (312)
2008	2.41 ± 0.97^a (1095)	2.77 ± 1.42^b (1123)	3.47 ± 1.55^c (1114)
2009	2.55 ± 0.86^a (869)	3.24 ± 1.69^b (878)	3.72 ± 1.42^c (881)

These analyses showed that warming treatment increased not only the heterotrophic and basal respiration rate but also Q_{10}, thus we changed the manuscript including the title according to these new results. However these results still support one of conclusions that "if we predict the soil heterotrophic respiration rate in future warmer environment using the current relationship between soil temperature and heterotrophic respiration, the rate can be underestimated". The new title is "Soil warming in a cool-temperate mixed forest with peat soil enhanced heterotrophic respiration rate and temperature sensitivity".

In agreement with your recommendation and the suggestion from Reviewer #2, fitting regressions for shorter periods and for each chamber has the advantage of showing the seasonal variation and the daily standard deviation for each treatment. In addition, we can check the significant difference among the treatments, so we decided to use the newly computed results. All new results were reflected in the revised paper.

Specific comments

Materials and methods:

p. 6419: from the information given here, it is not entirely clear to me what the experimental site looks like. As I understood, part of the vegetation was harvested, but it remains unclear which species were present (at what density) and which species were dominant at the moment of measurements.

To clearly describe the site, *2.1 Site Description* on p. 6419 (lines 5-14) was revised into:

"In late 1970's, an artificial forest was established in the site. To mimic its original vegetation, the site was planted with *Abies sachalinensis, Picea jezoensis, Quercus crispula, Betula ermanii, Betula platyphylla* var. *Japonica* and *Acer mono*. At present, the tree height is 10-15 m, and the density and basal area are 831 stems ha^{-1} and 20.7 m^2 ha^{-1}, respectively. Evergreen dwarf bamboos (*Sasa senanensis* and *Sasa kurilensis*; 1.5 m in height) had formed dense undergrowth, however, in order to set up the chambers for the respiration measurement, the dense *Sasa* bamboos inside the 1,480 m^2 fenced experimental site were clearcut in October, 2006, keeping overstory trees intact. Remaining bamboo roots after cutting were left to decompose until the chamber installation in July, 2007 to diminish the influence of residual decomposing roots. The forest floor was maintained bamboo-free throughout the study period".

p. 6420: I wonder if trenches of 30 cm are actually sufficiently deep to assume that what is measured is Rh only. Can authors provide any indications for that? Was rooting depth measured?

In order to prove whether trenching depth is sufficient or not, we established three 15×15 cm^2 plots beside the chambers in control plots. We collected $15 \times 15 \times 15$ cm^3 soil blocks each for the three layers (i.e. 0-15, 15-30, 30-45 cm deep) and coarse and fine roots were collected on each soil block, then washed and ovendried to determine their biomass contents. The root biomass for each layer was 664±64SD, 156±22, and 41±8 gDW m^{-2}, for 0-15, 15-30, and 30-45 cm deep,

respectively. Based on the new results, we considered that the contribution of roots below 30 cm deep to soil respiration could be minor at trenched treatments.

Supporting our result, a previous soil coring and minirhizotron study made within Teshio Experimental Forest have shown that the fine roots of both bamboos (*Sasa senanensis* and *Sasa kurilensis*) and prevailing trees (the same species within our site) were concentrated in the surface soil (0-15 cm) and decreased with increasing soil depth (Fukuzawa et al., 2007, Ecological research, 22, 485-495). This similar pattern was observed in our site.

In addition, we collected soil core samples beside the chambers in control plots at 5, 10, 20, and 40 cm deep in the soil with 5 replicates to determine C contents and stock within the surface 30 cm soil layer, following to the recommendation from reviewer #2. Accordingly, we deleted the sentences on the soil carbon content at P6419L15-25 and P6429L11-13 in the previous manuscript, then we added new sub-sections "Soil and root biomass measurements" in the Material and method section, and "Soil carbon content and root biomass" in the Results section to show this newly observed data as follows.

"2.5 Soil and root biomass measurements

In August 2011, soil sample cores of 100 cm^3 (5 cm in diameter) each were collected beside the five chambers in control plots at 4 depths (5, 10, 20 and 40 cm) to evaluate the soil carbon content and density in the study area. Dry bulk density was obtained by weighing the samples after 2 days of oven-drying at 80 ˚C. Soil carbon content was analyzed using an automatic NC analyzer (Sumigraph NC-900, Sumika Chemical Analysis Service, Japan) attached to a gas chromatograph (GC-8A, Shimadzu Corp., Japan). Three homogenized soil samples with 49 to 52 mg weight were analyzed to get the average for each core.

In addition, root biomass (> 0.5 mm in diameter) was measured every 15 cm soil layer down to 45 cm deep at three of the five points where the soil cores were sampled. Soil blocks with 15×15×15 cm were collected at each layer, and roots in the blocks were collected. The root samples were washed and oven-dried at 80 ˚C for two days and weighed."

"3.1 Soil carbon content and root biomass

The soil carbon content was 99±32 SD, 111±32, 188±22 and 233±45 g kg^{-1}, at 5, 10, 20, 40 cm deep, respectively and evaluated soil carbon density at surface 30 cm soil layer was 17.6±1.6 kgC m^{-2}. The root biomass was 664±64, 156±22, and 41±8 gDW m^{-2}, for 0-15, 15-30, and 30-45 cm soil layers, respectively. The root biomass sharply decreased with the increase in depth and >95% of the roots in the collected soil was in the surface 0-30 cm soil layer."

However, the large proportion of heterotrophic respiration to total soil respiration obtained in this study might be partly caused by incomplete trenching, thus in the discussion section, we acknowledged the possibility by mentioning the disadvantages caused by trenching method (please refer to the 1st item/remark on the Discussion section of your comments).

p. 6421: The information about the measurement system is confusing. Because information about, for example, enclosure time or equilibration time is lacking, I had a look at the papers of Liang et al (2003, 2004) to which authors referred. These studies mentioned an equilibration time of ca. 20 minutes. Were chambers in the current experiment also closed for 20 minutes? Did that affect air (and soil) temperature?

Enclosure time for each chamber was 240 s (i.e. 4 min; p6421 20-22). 4-min measurement for each of the 15 chambers took 1hour to complete hourly measurement of the whole 15 chambers, and the effect on the air temperature is considered to be minimal.We revised a part of 2.3 *Soil CO2 efflux and environment measurements* in page 6421 (15-23) as follows:

… "During measurements, air in the chamber was mixed by two micro fans (MF12B, Nihon Blower Ltd., Tokyo, Japan), air inside the chamber was circulated through the IRGA by a micro-diaphragm pump (5 L min^{-1}; CM-50, Enomoto Ltd., Tokyo, Japan), and the rate of change in CO_2 and water vapor mole fraction were measured by the IRGA. Over 1 hour, 15 chambers were closed sequentially giving each 240-second closing time. The opening and closing were controlled by the data-logger, and the logger also acquired data output from the IRGA at 20 s intervals during each chamber measurement. Consecutively, CO_2 efflux rate was evaluated every hour for the 15 chambers during the snow-free periods".

I also noticed that authors used a different equation for flux calculation than Liang et al (2003, 2004) did, but the reason for this different equation is not given. I suppose it has to do with the measurement of water vapor that is possible with the improved system. Please clarify.

Liang et al., (2003) developed a multi-chamber system for measuring soil-surface CO_2 efflux. The system was flow-through, "steady"-state design, in which a certain amount of air was sent to and exhausted from the closed chamber throughout the measurement, and they measured CO_2 amount incoming to and outgoing from the chamber and employed this equation for the evaluation:

$$F_c = (C_S - C_R)\frac{Q x 10^3}{V_{air}}\frac{1}{A}$$

where C_R and C_S are ambient and chamber CO_2 concentrations (µmol mol^{-1}) measured from the inlet and outlet of the chamber, respectively; Q is volume flow rate through the chamber (m^3 s^{-1}); A is soil surface area covered by the chamber (m^2); and V_{air} is molar volume of air (mol m^{-3}).

However, because of the pressure-related problems in the measurement caused by the difference between inflow and outflow rates of air, they improved the system using a flow-through, "non-steady"-state design, which measures the change in CO_2 concentration in a closed chamber and modified the equation into (Liang et al., 2010):

$$R_S = \frac{VP(1-W)}{RST}\frac{\delta C}{\delta t}$$

where V is the effective chamber-head volume (m^3), S is the measured soil surface area (m^2), P is the air pressure (hPa), T is the air temperature (K), and W is the water vapor mole fraction (mmol mol^{-1}) inside the chambers; $\delta C/\delta t$ is the rate of change in the CO_2 mole fraction (µmol mol^{-1} s^{-1}) calculated by the least-square method, and R is the gas constant (8.314 Pa m^3 K^{-1} mol^{-1}).

In our case, we applied almost the same system with Liang et al. (2010), however we also added correction term caused by the evaporation from the soil surface ($\Delta W/\Delta t$). This equation is

the same with Takagi et al. (2009), and that of Li-Cor, LI6400-09 soil respiration measurement system as follows,

$$F_c = \frac{kPV}{S(T+273.15)}\left(\frac{\Delta C}{\Delta t} + \frac{C}{(1000-W)}\frac{\Delta W}{\Delta t}\right)$$

where k is a constant (120.28=1000/8.314); V and S are the volume (m^3) and area (m^2) enclosed by the chamber, respectively; P is the atmospheric pressure (constant at 101.325 kPa); T is the average air temperature (ºC) in the specific chamber that measured at about 25cm height in the center of the chamber; C and W are the average CO_2 (μmol mol^{-1}) and water vapor (mmol mol^{-1}) mole fraction, respectively; and $\Delta C/\Delta t$ and $\Delta W/\Delta t$ are the rate of changes in CO_2 and the water vapor mole fraction over time (s), respectively.

That is why we stated that in section *2.3 Soil CO$_2$ efflux and environment measurements* on p. 6421 (lines 2-5) as follows,

"The flow-through, non-steady-state automated chamber system was set-up. The system was originally designed by Liang et al. (2003 and 2004), however was improved to measure the rate of change in CO_2 and water vapor over time in a closed chamber (Takagi et al., 2009; Liang et al., 2010)".

We did not revise this sentence.

p. 6423: Using equation 4, the basal rate at 0 ºC is calculated. However, this temperature is the minimum of all measurement temperatures and is therefore not a good reference temperature to use for comparison of the basal rate. I suggest using a dif-ferent equation, which allows calculation of a basal rate at, for example, 15ºC (which is about the mean soil temperature during soil respiration measurements). The function that can be used is: Fc=Rr*Q10^((Tc-Tr)/10), with Fc the measured soil respiration rate at time c, Rr the basal rate at reference temperature Tr (e.g., 15 ºC), Q10 the temperature sensitivity and Tc the temperature at time c.

We used the equation you recommended in the revised manuscript, however we set 10ºC as the reference temperature. The higher reference temperature increased the variance in the soil CO_2 efflux rate among chambers for each treatment, however, we found significant increase in the basal respiration rate (at 10 ºC) by the experimental warming.

Discussion:

I think the discussion needs a section on the methods used here. Trenching is not a perfect technique for partitioning of Rh and Ra. Kuzyakov (2006) demonstrated the pro's and contra's of different partitioning techniques, and highlighted the shortcomings of trenching experiments. It's important for readers who are not experienced with this partitioning to realize this. I'd like to see discussed how the disadvantages of this technique may affect (warming responses of) basal rates and Q10 of Rh and Ra.

Following to your recommendation, sub-section *4.3 Contribution of heterotrophic respiration to the total soil respiration* was changed as follows:

"Our result showed that heterotrophic respiration rate (not associated with warming) governs the total soil respiration rate given its 71% contribution. Several studies report the similar contribution of heterotrophic respiration such as, 67% for a mixed hardwood forest in Massachusetts (Bowden et al., 1993); 77% for a lowland old-growth beech (*Nothofacus*) in New Zealand (Tate et al., 1993); >70% for *Picea abies* stands in Northeast Bavaria, Germany (Buchmann, 2000); and 56 to 69% for a subalpine forest dominated by lodgepole pine (*Pinus contorta*) trees in Niwot Ridge, Colorado (Scott-Denton et al., 2006).
However, because we applied trenching method to separate heterotrophic and autotrophic respirations, it could have altered the microorganism activities and thus, decomposition of soil organic matter due to the absence of living roots (Kuzyakov, 2006), which may had caused the underestimation of the observed heterotrophic respiration. On the other hand, because trenching itself could stimulate root decomposition and soil respiration and the phemenon can last for more than 6 to 9 months (Zhou et al., 2007), the data obtained in 2007 could have overestimated the contribution of heterotrophic respiration to total respiration. Uncertainties still remain as to the contribution of root respiration below the trenching depth (30 cm), although our root biomass survey shows minor contribution of the root below 30 cm to the whole root biomass within the surface 45 cm soil layer. Contribution of roots in deep layer could cause an overestimation of the heterotrophic respiration in the unwarmed-trenched treatment, and underestimation of the autotrophic respiration estimated as the difference between the two treatments (unwarmed-trenched and control). Given these disadvantages in the method, our estimated values may include uncertainties to some extent."

p.6424: It seems more logic to discuss first differences in soil respiration across years, followed by basal rate and Q10 which provide more detailed information about observed differences (instead of the other way around as in the current version).

We revised the manuscript according to your recommendations, however, following your and reviewer #2's helpful suggestions, we fully revised especially sub-section "3.2 Soil CO$_2$ efflux and the warming effect" as follows, by adding Figures I, II, III, and IV, and Table I in this response, and deleting Table 1, Figures 1, 2, 3, 4, 6 and 8 in the previous manuscript. Figures II, III, and IV are shown at the last part of this response. However, we remained Figure 5 just to show the overall temperature sensitivity for each treatment (but the regression equation was

changed to the new one). We also revised related sentences in the manuscript by describing new methods to determine seasonal variations in Q_{10} and R_{10}.

"3.2 Soil CO_2 efflux and the warming effect

Soil CO_2 effluxes in all the treatments roughly paralleled to the seasonal variation of soil temperature. Increasing the rate at the start of growing season in spring until summer and decreases towards leaf fall in autumn (Figure IV). Soil warming increased the heterotrophic respiration rate consistently across the entire measurement period ($p < 0.001$). The efflux rate of control chamber was almost the same with that of warmed-trenched chamber in 2007, but was intermediate between the effluxes of warmed and unwarmed trenched chambers. Mean heterotrophic respiration rate was 4.67, 5.87, and 6.91 (μmol C m^{-2} s^{-1}) during snow-free period in 2007, 2008, and 2009, respectively, at warmed-trenched treatment, showing increasing trend from 2007 towards 2009. This increase is likely caused by the increasing temperature during this period. Across all seasons within the 3-yr warming period, soil CO_2 efflux was greatest in the warmed-trenched chambers (Figure IV). Warming increased the efflux by 74% (or around 25% per ºC) (mean 6.11±3.07SD μmol C m^{-2} s^{-1}) compared with that of the unwarmed-trenched treatments (mean 3.52±1.74μmol C m^{-2} s^{-1}) ($p<0.001$), while the control chambers obtained 4.98±2.44 μmol C m^{-2} s^{-1}.

The difference in soil CO2 efflux between unwarmed-trenched and control chambers showed that heterotrophic respiration contributed 71% of the total soil respiration and the remaining 29% was assumed to be the autotrophic respiration (Figure 7 in the previous manuscript). Autotrophic respiration peaked in advance (June to July) from that of heterotrophic respiration (August) in both 2008 and 2009. For over 20-month period, total soil respiration rate reached 2.74 kgCm^{-2} wherein 1.94 kgCm^{-2} of it had been contributed by heterotrophic respiration. Calculating for an equal period of measurement from 22 April to 19 November for both 2008 and 2009 showed that total soil respiration rate dropped from 1.20 kgCm^{-2} in 2008 to 1.13 kgCm^{-2} in 2009 while soil heterotrophic respiration decreased from 0.86 kgCm−2 in 2008 down to 0.81 kgCm−2 in 2009. A higher average soil temperature in 2008 (15.5 and 15.6 ºC for control and unwarmedtrenched treatment, respectively) than that in 2009 (14.8 and 15.0 ºC, respectively) was observed from June to September, and this could cause the decrease in the soil respiration rates in 2009. The rate of decrease in the total soil respiration from 2008 to 2009 (0.07 kgCm^{-2}) was primarily driven by the decrease in the soil heterotrophic respiration (0.05 kgCm^{-2}).

An exponential function described the relationship between the soil CO_2 efflux and soil temperature for each treatment (Figure 5 in the previous manuscript, but changing the regression equation). Q_{10} values in unwarmed-trenched, warmed-trenched and control were 2.44, 2.44, and 2.54, respectively, with little difference between unwarmed- and warmed-trenched treatments. Meanwhile, basal respiration rate at 10ºC in soil temperature (R_{10}) differs among treatments with a higher R_{10} in warmed-trenched chambers (3.59 μmol C m^{-2} s^{-1}) compared with unwarmed-trenched chambers (2.75 μmol C m^{-2} s^{-1}). Control chambers showed the highest (3.91 μmol C m^{-2} s^{-1}) owing to the contribution of root respiration.

Seasonal variation in Q_{10} and R_{10} (Figure I) showed that Q_{10} tended to be high in summer except very high values obtained jut after snow melt and before snow accumulation, while R_{10} tended to be high in Autumn. Comparing whole 3-yr data set, R_{10} in warmed-trenched chambers (2.94 ± 1.52 μmol C m^{-2} s^{-1}) was significantly higher than that in unwarmed-trenched chambers

$(2.50 \pm 0.89$ μmol C m^{-2} s^{-1}) (Table I). Control chambers showed the highest R_{10} $(3.59 \pm 1.45$ μmol C m^{-2} s^{-1}) owing to the contribution of root respiration. The significant difference caused by warming was observed in 2008 and 2009, but not in 2007. The increase in R_{10} was observed in 2009 compared with that in 2008 for all treatments.

Similar to the case for R_{10}, warming increased the Q_{10} significantly, if we compare data in whole 3 years, 2008, and 2009, while there was no significant difference between unwarmed-trenched and control treatments for the 3 years. Contradictory to the case for R_{10}, Q_{10} in 2009 was lower than that in 2008 for all treatments. The short term determination and averaging of Q_{10} and R_{10} in Table I increased the values compared with the case obtained as in Figure 5."

p.6427, l.11-16: I find it inappropriate to extrapolate results of the growing season to obtain annual estimates. Winter temperatures are outside the range of temperatures under study, plants are dormant during winter, and snow cover and freeze-thaw cycles can have important impact on soil respiration.

We opted to delete this portion in *3.2 Soil CO2 efflux and the warming effect* on p. 6427 (lines 11-16) from the manuscript which states:

…"When we assume the non-growing season respiration rates to obtain an annual respiration rates by using the soil temperature data throughout the study period (Fig. 1) and temperature-respiration relationships (Fig. 6), the annual total and heterotrophic respirations were 1.43 and 1.03 kgCm^{-2}, respectively, in 2008, and 1.39 and 0.98 kgCm^{-2} in 2009. Additional rates were 16 to 19% of the annual total respiration rates and did not alter the growing season inter-annual tendencies".

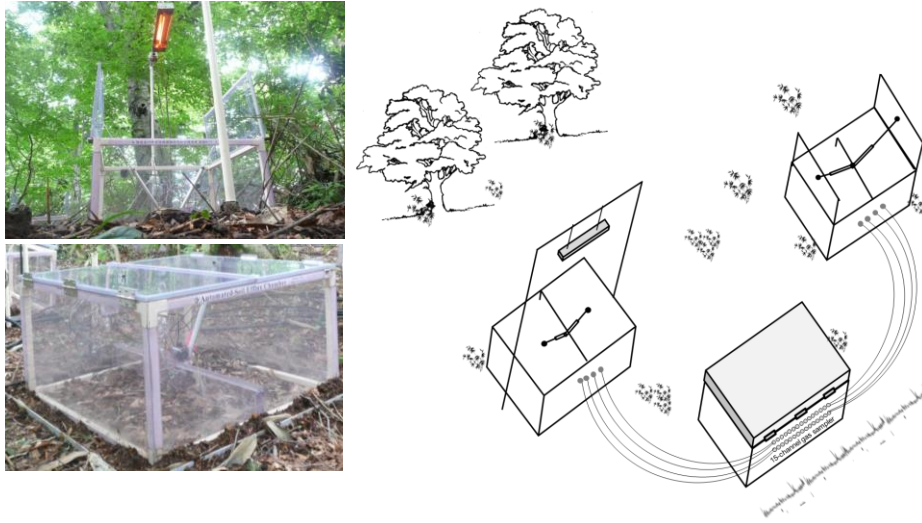Lastly, we show new figures (Figures II, III, IV) following reviewer#2's recommendation.

Figure II Images of warmed-trenched plot (upper left) and a closed chamber with trenching (lower left), and a schematic illustration of the multi-channel automated system.
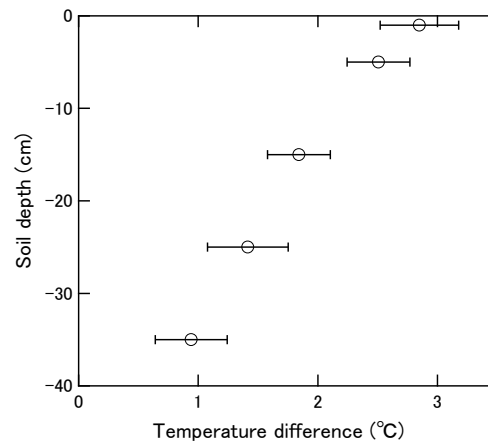

Figure III Difference in the soil temperature profile between unwarmed- and warmed trenched treatments from 22 August to 7 September 2011.
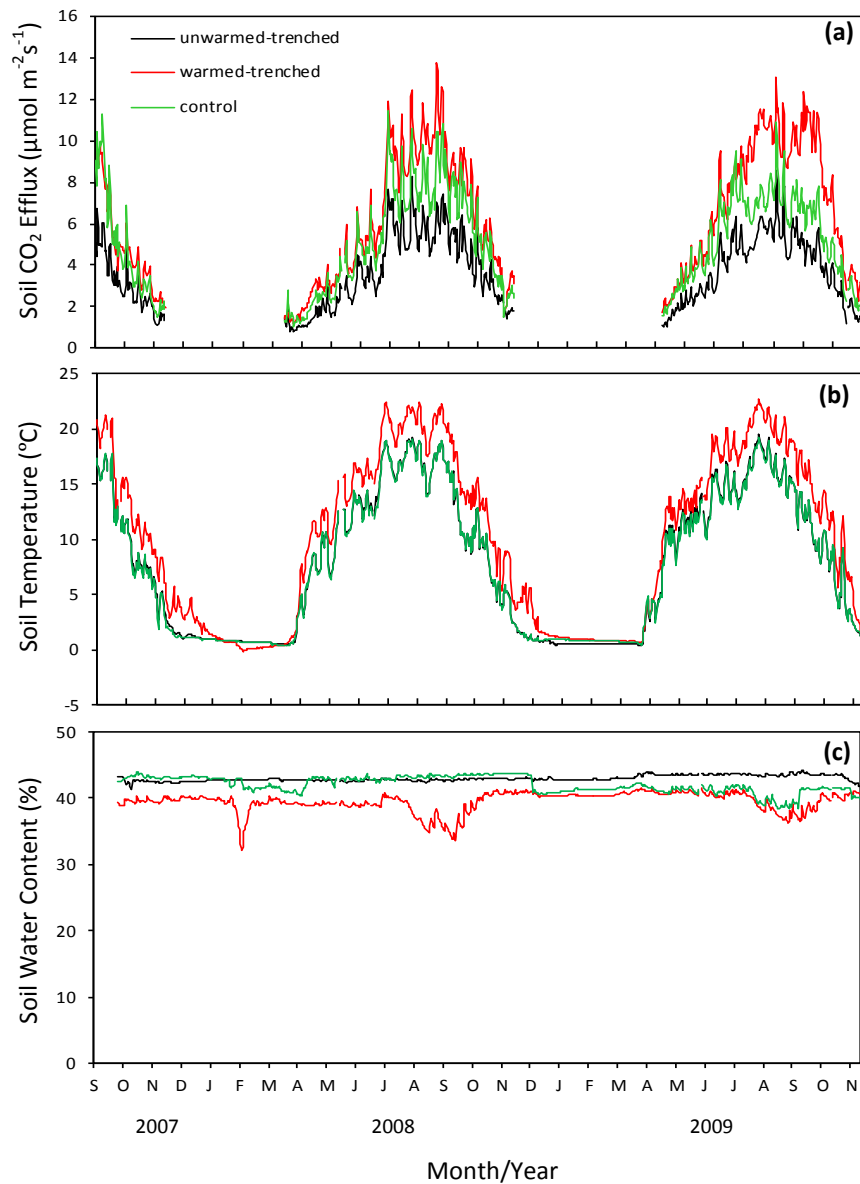
Fig IV Interannual variation of (a) soil CO_2 efflux; (b) soil temperature; and (c) soil water content in unwarmed-trenched, warmed-trenched, and control treatments during the study period in 2007–2009. All data are daily averages.

Thank you very much.

Respectfully yours,

The Authors