“Initial effects of thinning and fertilization on soil CO_2 efflux in a cottonwood plantation in Iceland” by J. Á. Jónsson and B. D. Sigurdsson

The author´s wish to begin with thanking all the three referees for their constructive criticism, which has significantly improved the manuscript. Their comments can be seen in the following sections. Author´s replies are listed below each comment in red.

Anonymous Referee #1

General comments:
This paper is a good addition to research that began as an experiment in forest production in the southern coast of Iceland in the early 1990's. The authors show that soil respiration rates are affected by thinning and that temperature seems to be the driving environmental variable.

Specific comments:
Section 3.4: Presumably, the increase in soil temperature is a direct result of more incident solar radiation reaching the surface. At this high latitude, this effect would be maximized in the summer months with long day lengths as is seen in Figure 4. In summer, did the authors notice an effect of clear vs. overcast days? I recognize that clear days are not common in this area.

We agree. We believe the increase in soil temperature in thinned treatments to be a direct result of more solar radiation reaching the forest floor following the thinning. Since the soil temperature was measured at 10 cm depth it does not only fluctuate because of daily differences in irradiance (clear vs. overcast days), but responds to integrated changes of irradiance and air temperature conditions over several days. We, however, decided not to show this in a diagram.

All thinned trees were left on floor to decompose. Is this the commercial practice? Is the species used for other purposes and therefore they would be removed?

Yes, usually when pre-commercial thinning is carried out cut trees are left on site (hence the term “precommerical”). When commercial thinnings are done, normally at ca. 30 years of age under Icelandic conditions, the stems are removed but the tops and branches are left on site. A comment on this has been added to the manuscript.

In the conclusions, the authors state that fertilization increased soil respiration but thinning intensity decreased it even though all organic material was left at the site. I presume that the authors' intention here was to indicate that this additional organic matter would decompose and provide additional respiration. Would they expect decomposition to be an additional factor in the first year? Perhaps the litter caused soil temperatures to be reduced (less incident solar radiation) therefore reducing respiration – was this an effect that was considered?

In the conclusions our intention was to indicate that we expected immediate additional decomposition to take place in the first summer as a result of both higher soil temperatures and increased litter availability in the thinned treatments, contrary to what actually happened.

We did not consider the latter effect (partial shadowing by litter). It is very plausible that the soil temperatures would have increased even more if the litter was not covering the forest floor after the thinning, but as our measurements showed still the thinning increased soil temperatures in the summer (Fig 3). Therefore, the increase in incident solar radiation reaching the forest floor following reduction in LAI must have been higher than what was being blocked by litter.

The site is located in a high precipitation and high cloud cover region. The temperatures experienced are fairly low overall and the growing season is fairly short. Would the authors comment on the larger context of their respiration rates? Are these significant beyond the local Icelandic context?

This is a good comment. We added some discussion on this to the manuscript.

Technical corrections:
p.9260 line2: change 2nd to second
Done

p.9260 line11: insert comma "In 1990, : : :"
Done

p.9262 line2: Likely should be "Centre for Chemical Analyses"
Done

p.9262 line17: "calculations of LAI were limited to: : :due to the relatively small: : :"
Done

p.9262 line21-22: What does "receptively measured" mean?
"receptively" removed

p.9262 line22: ": : :with a closed-chamber: : :"
Done

p.9263 line5: change constantly to continuously
Done

p.9263 line8: ": : : and both were stored as : : :
Done

p.9263 line9: You have already mentioned air temperature was measured in each plot. Give the height of the measurement from the central tower. You may want to give the other instrument heights as well.

Done.

p.9263 line19: ": : :on measured air and soil temperatures."
Done

p.9264 line20: ": : :in 2004 were: : :" In this sentence, I assume that the temperature is an annual average; the precipitation is a total and the irradiance is: : :?

The temperature is an annual average but precipitation and irradiance (global radiation) an annual total. Now clarified in the text.

p.9265 line2-3: ": : :on a warm summer day with a temperature of 18_C in the unthinned treatments, the air temperature would be 19.3_C: : :" Also, remove the + sign in front of temperatures; it is assumed.

Done. We removed the + signs as suggested.

p.9265 line7: Insert commas ": : :treatments had, on average, 1%..."

Done.

p.9265 line12: "Soil respiration in the thinned treatments generally followed soil temperature and increased from spring: : :" Which soil temperature is plotted here? The un-thinned control, or an average of all treatments?

The plotted soil temperature in Fig 4. is an average of all treatments. Now clarified in the caption.

p.9265 line21-25: I needed to re-read this passage a couple of times with reference to Figure 6. I believe that the authors were speaking of comparisons between each of the treatments and the control. e.g. C-00 and F-00 and so on. Figure 6 clearly shows a decrease in respiration by treatment within the fertilized or non-fertilized plots. The statement of increased fertilization across the all three thinning treatments is therefore a bit confusing although correct when examined more carefully. Would it be better to rearrange the plot such that the C-00 and F-00 are side by side etc.?

The authors know that it can be confusing to read Figures from 2-Way factorial experiments (where comparisons can be made in different way). They still think that the comparison between different levels of thinning (within fertilization treatment) is more central than the comparison between fertilized and unfertilized; therefore they choose to arrange the figures as they are now.

p.9268: I agree. Beyond the study mentioned, long-term fertilization studies elsewhere have also indicated reduced respiration (e.g. Bowden et al., 2004. Forest Ecology and Management 196; 43–56).

We added the US reference to the Introduction.

p.9264 line7-8: It is stated that when compared across treatments, thinning had a negative effect on N concentration. More specifically, do the authors mean that compared with control, each of the thinned treatments was less than the control? It seems as though F50 had the lowest nutrient status and smallest error bound. Any thoughts? Similar clarification for the data in Figure 2. The statement is simply that compared to control, each thinning treatment individually was different from control.

The 2-Way factorial experimental setup made it possible to compare all three thinning levels together between the two fertilization levels (thinning as a main factor), but not just one at the time. This is the basic idea with this type of experimental setup and subsequent statistical analysis.

Figure 1 and other captions: Change existing to ": : :in unfertilized (C) or fertilized (F) treatments: : :"

Done

Figure 3 and throughout the m/s. The authors use both Irradiance and Global Radiation to describe the measured incoming shortwave radiation received above the plantation. From the Glossary of Meteorology: "Global radiation is the total of direct solar radiation and diffuse sky radiation received by a unit horizontal surface" and is measured with a pyranometer. Irradiance (radiant flux density) is "a radiometric term for the rate at which radiant energy is a radiation field is transferred across a unit area of a surface: : :in a hemisphere of directions". I prefer global radiation which refers explicitly to the solar (shortwave) part of the spectrum and is derived through a measurement device.

Text harmonized (“irradiance” replaced with “global radiation”).

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The author´s wish to begin with thanking all the three referees for their constructive criticism, which has significantly improved the manuscript. Their comments can be seen in the following sections. Author´s replies are listed below each comment in red.

Anonymous Referee #2

GENERAL COMMENTS:
Jónsson and Sigurdsson present results of a combined thinning and fertilization experiment in forested young black cottonwood plantation in Iceland. There is an interesting finding that thinning reduced soil respiration and leaf size. Furthermore, they show highly expected result that fertilization has positive short term effects to the plants. The author’s explanation the first finding would mean that soil respiration is dominated by autotrophic soil respiration. This would be a major scientific result and could be explained by very specific properties of the site. However, the authors do not discuss at all the significance of their finding. Neither do they estimate what their data interpretation would mean as the proportion of autotrophic soil respiration to total soil respiration. I question that would this kind of proportion be possible at all (eg. autotrophic soil respiration cannot be higher than total soil respiration and thinning should dramatically increase heterotrophic soil respiration). Furthermore, the authors have not considered that the finding could be explained by soil water content, which in my opinion could easily explain the finding. In either case, the quality of data interpretation is low and the authors are only able to show results that support what is already known.

The authors have added a discussion on the soil water content and they have tried to make the discussion that already existed on the significance of the first finding more clear (which has also has been shown in a number of other recent studies in Europe and N-America , as was pointed in the discussion). The authors agree with the referee that it was a surprising finding that soil respiration decreased after thinning, since the heterotrophic respiration was bound to increase because of higher litter input and higher soil temperatures. That is exactly why the authors believe that this manuscript should be published.

As the authors express themselves very clearly, the language is very good and the paper is short, it is very enjoyable to read. However, in scientific point of view the paper is definitely too short. The methods used are not explained in detail enough, neither is adequate literature explaining the methods being referred. I think the data would allow much richer discussion as well (see detailed comments), though it could remain speculative. On the other hand, the introduction, results and figures could be published with only minor changes.

The authors have improved the material and methods and discussion chapters based on the comments made by the referees.

The sampling design is very nice, but the study period is definitely too short. Furthermore, basic soil information such as pH, C:N, particle size distribution and amount of carbon per area are not reported or found in the referred literature. I think because of these reason, the authors fail to draw any general conclusions such as, what are the effects of thinning and fertilization to growth or carbon sequestration in forested fields in Iceland or that autotrophic respiration seem dominate the soil respiration in forested Andisol-sites in Iceland.

The study period was indeed short; we have responded to this comment by changing the title to “Initial effects of …”

In general, with some additional measurements (soil properties, longer-term effects) included and with major increase of content to materials and methods and discussion this manuscript could be published in Biogeosciences. However, I think it would require several major revisions and probably ill-proportioned intellectual contribution from the referees (see specific comments). Bearing in mind the average scientific quality of the papers published in Biogeosciences, I suggest this manuscript to be rejected. I hope that the authors will find a way to improve their manuscript so it could be published either in second submission in Biogeosciences, or in some other journal.

SPECIFIC COMMENTS:
Page 9260, line 13: Is ‘sod’ same as ‘peat’?

No, sod is just the surface layer of earth. In this case grass vegetation along with it´s roots.

9260 25-26: I don’t think it is clear that nitrogen availability is to ‘most limiting environmental factor for tree growth’. I think that for example low irradiation or short growing season length could also be the most limiting factor. Furthermore, I don’t find such a conclusion Sigurdsson et al (2001).

The correct reference was Sigurdsson 2001 (not et al.).

9261 13: In which form was the N applied? “Optimum proportions” is perhaps a bit too strong expression, as our knowledge is still quite limited.

The discussion on “optimum proportions” was actually based on quite detailed research. This has been further elaborated now in the text.

9261, 3-4: How long did you store the leaves in -18 C-degrees?

Eight months, now clarified in text.

9262, 9-10: How did you measure crown surface area? Please specify.

It was measured by measuring the max and 90° to max dripline diameters by measurement tape. The authors did, however, remove the description of growth variables that were not used in the present analysis from the manuscript to reduce confusion.

CHAPTER 2.5:
I could not repeat the measurements with this information. Could you explain how you actually did the measurements, or at least refer to a paper where it is explained? LAI can be defined as projection LAI, one-sided LAI, two-sided LAI or all-sided LAI (conifers). Please specify that you measured projection LAI, as that is what LAI-2000 measures. Do you have estimations of LAI of ground vegetation? You mention that “the calculations of LAI was limited to hemispherical area above 23-deg”. How did it affect the defined LAI?

Done. The LAI was not much affected by removing the 5th ring in the analysis, but it was removed because it also "saw" the trees in the neighboring plot and was therefore not measuring the correct LAI, even if it did not contribute much to the average LAI calculated.

CHAPTER 2.6:
9262, 19 – title: The more correct term would be 'soil CO2 efflux' rather than "soil respiration".

Done.

9262, 20-21: Does this mean that you had totally 192 collars installed? Did you break roots during the chamber installation? Breaking the roots may significantly disturb the soil and soil respiration.

Yes, a total of 192 collars were inserted into the ground. Inevitably some roots were broken during insertion, but they were mostly grass roots. To minimize root breakage, while still keeping collars in place, collars where only inserted ca. 3 cm down into the soil.

We have improved this part of the manuscript.

9262 21-23: I assume that you used a chamber. Please specify at least the volume and also height/diameter of the chamber, unless the chamber diameter is the same as collar diameter. Did you use a fan inside the chamber? If so, what kind of fan did you use, how it was positioned and how much power it used? Did you use a venting tube to prevent pressure changes inside the chamber? Was the chamber model SRC- 1 from PP-systems? Did you use CIRAS-1, CIRAS-2 or what? Are you aware that linear function can significantly underestimate the measured efflux (see eg. Kutchbach et al., 2007). As the closure was only 1.5 min I do not think it is an issue this case. However, the short closure time can be problematic, since chamber placement on top of the collar can disturb the efflux significantly. Did you see this and how did you take it into account? Did you for example exclude some of the first data points during the measurement?

We used CIRAS-2 gas analyzer along with SRC-1 chamber, now clarified in text. The chamber diameter is the same as collar diameter and there was a tight fit. We did not note any unrealistic measurements, so no data points were excluded. We are aware of the problem using linear function for longer measurement times, but as the reviewer stated our measurement times were 1.5 minutes. After having first tested different measurement times and looked at the issue of flow disturbance, we found 1.5 min to be the optimal time for measurements. A comment about this was added to the manuscript.

At what time of the day did you measure the soil CO2 effluxes? You mention that there was diurnal variation in soil temperatures, suggesting that there was diurnal variation in CO2 effluxes as well. Furthermore, you mention that the variation was larger in the thinned plots (9265 8-5), causing potentially systematic error to the comparison of the plots. As you continuously measured soil temperatures, you could use a simple exponential temperature response model (Lloyd & Taylor 1994; Tuomi et al. 2009) to calculate daily respiration and this way get rid of the possible problem. In my opinion, in modeling you could use a Q10 value of 2, which is anyway close to the actual short term temperature response. I do not recommend you determining Q10 values using measurements of long periods, as they may be confounded (see e.g. Davidson et al., 2006). Though the Q10 analysis has been criticized, it could perhaps be used in this study to show how much other factors than temperature affect soil respiration (Davidson et al., 2006).

Measurements were made between 9:00 AM and 17:00 PM, now clarified in text.

When statistical differences between treatments were tested, the statistical model used the measured soil temperature as a covariate. This has the same effect on the analysis as suggested here by the reviewer; i.e. systematic differences in fluxes due to differences in soil temperatures are removed before the treatments are compared. The difference between this method and the method suggested by the reviewer is that it uses a linear function instead of exponential one. Because of the relatively small differences in soil temperatures between treatments (always < 2 ℃), this is not a major issue. A comment was added about this in the M&M chapter.

9263, 1-2: Where did you measure the soil temperatures, at each collar?

Yes, now clarified in the text.

CHAPTER 2.7:
9263, 5-7: Did you also measure in non-thinned treatments?

Yes, soil and air temperatures were continuously monitored in the three thinning treatments which consisted of unthinned control, 50% thinned and 80% thinned. Now clarified in the text.

9263, 9-11: In general, only the measurements actually used in the analysis are mentioned in the materials and methods.

Additional measurements were removed (relative humidity, wind direction and wind speed) as well as some growth measurements (see earlier)

CHAPTER 3 – RESULTS:
A lot of data is not shown. I think that all the data mentioned but not shown should be published in a table or totally left out.

Yes – the authors added a table with data that was only described in text.

9264, 24: The average daily precipitation is quite obvious and in my opinion not needed, as you already report annual precipitation.

Average daily precipitation was removed.

9265, 1-2, 6-8. It cannot be stated that temperatures in Celsius-degrees change x %! Celsius degree is a unit in interval scale and units in interval scale cannot be multiplied or divided! To express how much temperature changed relatively, units in ratio scale (such as Kelvin) must be used. However, I do not see any point in doing that in this case.

The authors must admit that they have never been introduced to this issue before; and neither have apparently many other authors that report % changes in treatment temperatures, etc. We agree with the reviewer that converting all weather information to Kelvin in the present manuscript would maybe not be correct either.

9265, 9-10 I would like to know how much was the diurnal fluctuation of soil temperature in the thinned and non-thinned treatments. Figure or table is not necessary, you could just give some numbers how big the fluctuation on average is.

The authors added this information to the manuscript.

CHAPTER 3.5:
I would like to see a scatter plot where temperature is x-axis and CO2 effluxes at yaxis. Simple exponential response curve could also be fitted to the data and Q10 value calculated (keeping in mind that it is just apparent but not necessary real temperature response).

Done.

9265, 24-25: Using an expression “it was clear that” does not convince me. Perhaps you could just remove those five words?

Done

DISCUSSION:
9266, 3-5: What do you expect is the effect of high daily fluctuation to soil respiration?

This was met by limiting the measurements to the period of 9:00-17:00 and making a statistical analysis that used a linear correction to temperature (used Tsoil as covariate).

9266, 5: Perhaps instead of ‘LAI’ ‘leaf area’ should be used here, since LAI is just a way to describe nature but it does not actually exist in the nature.

“LAI” replaced with “leaf area”.

CHAPTER 4.2:
9266, 9: I do not think that leaf N concentration revealed that natural N supply was low and N was a limiting factor in tree growth, as you mention that it was already known (9260, 25-26).

We added “at this site”. This has been toughly studied at the very same experimental forest used for the present project and was reported in Sigurdsson (2001); Sigurdsson et al. (2001) etc.

9266, 10-11: Could you specify in which conditions Sigurdsson’s finding applies (one could understand that you are saying that Sigurdsson’s finding was universal).

Sigurdsson studied the same plantation as this study so we believe that even though the trees have grown his findings are still in force.

9266, 12-15: I agree that it is very difficult to see changes in tree growth after few months from the fertilization. It would be interesting to know what has happened after a couple of years after the fertilization.

Yes, indeed. But that was not done in the present study. That will be a future study.

I would like to see linking the observations of LAI, leaf size and leaf N together. Leaf size and LAI are definitely linked to each other, and it is interesting that fertilization increased leaf size, but not LAI! Leaf nitrogen content represents photosynthetic capacity, as proteins such as Rubisco contains a lot of nitrogen. As the light environment changes, also the optimal photosynthetic properties change and this could be seen in leaf N. I would very much like to see some estimates of the effects of thinning and fertilization to gross primary productivity at tree and stand level. These estimates could be further used in explaining the increased autotrophic soil respiration.

Leaf size was measured from the youngest fully expanded leaves on top shoots. To increase top shoot leaf size but not LAI with fertilization, leaves on top shoots represented a relatively small part of the crown.

We do not have any estimates on gross primary production in the first year, or the effect of thinning and fertilization thereon. However, we did measure tree height and diameter. Since no significant difference was noted on tree growth between treatments we assume that the same applies to gross primary production. In fact, the only tree growth response in the first year was observed in the mean leaf size, which was significantly increased with fertilization.

CHAPTER 4.3:
9267, 3: Perhaps you could use word 'hypothesized' instead of 'predicted'.

Done

9267, 6-9: What do you mean by saying that the results of Korhonen et al. were similar as Olsson et al? Please be more specific.

Both were on the fraction of autotrophic respiration in coniferous trees by girdling, even if Korhonen et al. also did many other auxiliary measurements that further supported their findings.

9267, 16-19: How about aboveground respiration of ground vegetation? Did you have them inside the collars? Report this in materials and methods! If you clipped them, how much do you think it affects the results? What do you mean by ": : :this effect was greater than any possible increase in heterotrophic respiration: : :" I find it surprising that soil respiration decreased in thinned treatments.

Ground vegetation was not clipped, but kept inside the collars when measurements were carried out. Hence, aboveground respiration of ground vegetation was included in the soil respiration (now clarified in materials and methods).

We too found it surprising that soil respiration decreased in thinned treatments. We believe reduction in autotrophic root respiration to be the primary cause for this (see comments to Reviewer#1), i.e. even though there might have been an increase in heterotrophic respiration the reduction in autotrophic respiration must have been more. Thinning reduced LAI which in turn reduced photosynthesis per area and most likely also the number of respiring roots per area.

In general I agree on your hypothesis that the reduction in soil respiration at the thinned plot could have been caused by decrease in autotrophic soil respiration. However, as far as I understand, this would mean that autotrophic soil respiration at the site is far higher than heterotrophic soil respiration. Can you give a rough estimate or upper/lower limit for summertime ratio between autotrophic soil respiration and total soil respiration based on this study? Do you have data of harvesting residue decomposition some years after the thinning? Could this data be used to give estimate of annual heterotrophic soil respiration?

Such an analysis is not possible from the data available from the present study. Therefore we will not attempt it here.

9268, 2-4: Specify what does N-supply limit (photosynthesis, respiration or growth). I by the way think that in general low N-availability increases autotrophic soil respiration. Should the limitation be of e.g. "low N-supply"? Note that litter decomposition and soil respiration are two different things, and that your results indicate that most of the soil CO2 efflux originates from autotrophic soil respiration! It may therefore be tricky to compare your results to results of Knorr et al. (2005b).

Done. We added to the discussion a note about this.

Do you have any information about microbial community in the soil, such as PLFAanalysis? As the soil is not natural forest soil and the soil type is rare, I hypothesize that the microbial community is not adapted to the current conditions. This would result in reduced growth for the trees due to lack of mycorrhizal connections or decreased decomposition. The latter would perhaps lead to enhanced carbon sequestration to the forest. However, it is hard to draw conclusions as there is no information about soil pH and C:N. Could non-typical microbial community explain the high fraction of autotrophic soil respiration of total soil respiration?

No – no such information exists.

9268, 8-14: Are you aware that high soil water content can limit biological processes (Skopp et al., 1990)? The hypothesis of limitation high soil water content is supported by the fact that precipitation at the site is very high compared to evaporation and you mention that soil water content was permanently near the field capacity! Do you refer to peat in page 9260, line 13? If there has been peat in the site before forestation, it would further indicate that high soil water content limits biological processes. This limitation could explain why thinned forests had smaller leaves, lower leaf N concentration and lower soil respiration, as transpiration is significantly lower at the thinned plots. Do you think that this could explain the results?

This point has now been addressed in the discussion. We don't believe that only 0.06 ha plots could yield such variable ground water level.

9268, 20-22: Are you saying that in the models autotrophic (soil) respiration and aboveground biomass or gross primary productivity are not being linked? I cannot give you references, but I think they are linked in (almost) all process based carbon cycle models of forests. Could you list some models (in reply to this report, not in the manuscript).

Good point; the linkage exists in most models that simulate the C-dynamics at single-tree scale (e.g. BIOMASS; MAESTRA); but not necessarily in those that use e.g. yield-table data to derive NPP (CO2-FIX).

TECHNICAL COMMENTS:
9260, 20: There is no verb in the latter clause.

OK

9262, 16-17: ": : :the calculations of LAI was: : :". Replace calculations with calculation.

We replaced "was" with "were", as suggested by referee #1

The chapter titles in results do not need the word 'measurements'. As the same titles are used in materials and methods as well (which helps reading), I think that the word 'measurements' could be removed from there as well.

Done.

9267, 9: Remove the word 'first' after Högberg et al. (2001), as after his study was published, the proportion was not anymore unexpectedly large.

Done

9267, 25 – 9268, 1: Since Hyvönen et al. (2007), meta-analysis by Knorr (2005b) has not been recent.

"recent" was removed

REFERENCES:
Davidson, E. A., Janssens, I. A., and Luo, Y.: On the variability of respiration interrestrial ecosystems: moving beyond Q10. Glob. Change Biol., 12, 154-164, doi:10.1111/j.1365-2486.2005.01065.x, 2006.

Lloyd, J. and Taylor, J. A.: On the temperature dependence of soil respiration. Funct. Ecol., 8, 315–323, 1994.

Kutchbach et al.: CO2 flux determination by closed-chamber methods can be seriously biased by inappropriate application of linear regression, Biogeosciences, 4, 1005-1025, 2007
Skopp, J., Jawson, M. D., and Doran, J. W.: Steady-State Aerobic Microbial Activity as a Function of Soil Water Content. Soil Sci. Soc. Am. J., 54, 1619-1625, 1990.

Tuomi, M., Vanhala, P., Karhu, K., Fritze, H., and Liski, J: Heterotrophic soil respiration - Comparison of different models describing its temperature dependence, Ecological modelling, 211, 182–190, 2008.

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The author´s wish to begin with thanking all the three referees for their constructive criticism, which has significantly improved the manuscript. Their comments can be seen in the following sections. Author´s replies are listed below each comment in red.

Anonymous Referee #3

General comments:
Jonsson and Sigurdsson present the results of a thinning and fertilization experiment within a young plantation. They present changes in the soil carbon efflux in the first growing season after treatment as differences with untreated control plots. The paper presents the observations and a discussion of the results and possible reasons for the found differences in the soil CO2 efflux between the treatments. It is relatively concise and to the point written, although at several points the reader is given results without showing the data and might be clarified by an extra table or figure.

A new table was added to the results chapter with this data.

The setup and idea of the experiment is clearly presented. However, reading the article I am left with the feeling that the measurements performed and presented in the article were not sufficient to convince. The conclusion that thinning leads to a lower autotrophic respiration is supported by the results and is probably the most plausible explanation, but a number of open questions is left.

The authors have now addressed a number of such open questions.

Although the paper is not strong I consider the results of this paper sufficient for publication. This type of measurements is absolutely needed for supporting the development of soil respiration models. I consider the scientific significance fair to good.

Specific comments:
Climate and soil temperature measurements It is unclear how many sensors were actually placed out in the field from the material and methods (page 9263- paragraph 2.7). In figure 4 it is presented that only two sensors were places in each treatment. This is a rather low number and not easy to use for analyzing temperature differences. Although the results show a temperature difference as expected between the thinning treatments it is not possible to analyze possible effects of the thinning residues left in the forest, which can actually act as isolation cover. Please add to the discussion a note on why the temperature difference between the treatments already starts during winter from December. Was there a snow cover during winter or not that it gives different soil temperatures.

A note about this was added to the discussion. Number of sensors per treatment clarified in section 2.7 of material and methods. The differences in soil temperature that existed between December and March are really small and could be caused by many small things, including differences in snow cover…

Soil respiration measurements The number of measurements of the soil CO2 efflux was 192 with 8 measurements in each treatment-plot. The number of repetitions in the season was only 4, which makes it a rather short period of time for the experiment. It is not clear from the paper when the measurements were made exactly during the day and or the order of measurements was different. The efflux of CO2 can change during the day and thus it can be important to spread the readings at the different treatments over the day. It is mentioned that the measurements were corrected for temperature differences, but it is unclear how exactly the correction was performed.

These points have been clarified; also the point how the temperature correction was done. For soil respiration measurements, see also answer to anonymous referee #2.

Differences in soil water content I would like to see a short note in the discussion on the possible effect of soil water content. It is mentioned that soil water content is at field capacity and that it is not expected to have affect on the tree growth and ecosystem processes. Thinning lowers the interception and the soil water content of the topsoil. How much is actually reaching the soil can be discussed as it depends on the forest floor vegetation and the thinning residuals left in the forest. Higher soil water content can also be a cause to a lower soil respiration

A note on this has been added to the discussion. A good point.

Page 9260 Site description: Please describe the forest floor. What was the type of vegetation? Were those included in the collars and chamber?

Details on surface cover added to site description. The ground vegetation was included in the collars and chamber during measurements, now clarified in material and methods.

Page 9262 Paragraph 2.6. Describe more about the collars and instrument. What was the size of the collars and the chamber? Assuming that the collars were placed after the thinning, were the collars randomly placed also under the thinned trees or between the thinned trees on the forest floor?

We used a model SRC-1 closed-chamber from PP-Systems, now clarified in text. The chamber is 150 mm in height and 100 mm in diameter. The collars were 5 cm deep with 100 mm inner diameter. They were randomly set out before thinning took place and therefore included both vegetation and litter residues.

Page 9267 line 4: Measurements of soil respiration took place during the months directly after the thinning. How much litter was actually added to the soil and within the collars at that time from the thinned trees? I can assume that the leaves were still on the thinned trees.

A note was added about this into the M&M.

Page 9268 Conclusions: Note that the results presented are from the very first months after thinning.

Now emphasized in the conclusions.

Technical comments:
Page 9258: be consequent in use of abbreviations: line 19 and 20 y-1 while eg on page 9267 year-1 is used.
Year-1 replaced with y-1

Page 9260 line 1: change ‘carbon efflux’ to ‘carbon dioxide efflux’
Done

Page 9260 line 19 Change ‘were monitored’ to ‘was monitored’
Done

Figure 5: add point in ‘(22 June to 22 September). Each bar’
Done