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Biogeosciences Discussions

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6, C2915–C2923, 2009

Interactive Comment

Interactive comment on "Effects of thinning and fertilization on soil respiration in a cottonwood plantation in Iceland" *by* J. Á. Jónsson and B. D. Sigurdsson

Anonymous Referee #2

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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.

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 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.

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.

BGD

6, C2915–C2923, 2009

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’?

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).

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.

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

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

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?

CHAPTER 2.6:

9262, 19 – title: The more correct term would be ‘soil CO2 efflux’ rather than “soil respiration”.

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.

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?

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).

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

CHAPTER 2.7:

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

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

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.

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

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.

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.

CHAPTER 3.5:

I would like to see a scatter plot where temperature is x-axis and CO_2 effluxes at y-axis. 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).

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

BGD

6, C2915–C2923, 2009

DISCUSSION:

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

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.

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).

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).

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.

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.

CHAPTER 4.3:

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

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.

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.

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?

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).

Do you have any information about microbial community in the soil, such as PLFA-analysis? 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?

BGD

6, C2915–C2923, 2009

Interactive Comment

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?

9268, 20-22: Are you saying that in the models autotrophic (soil) respiration and above-ground 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).

TECHNICAL COMMENTS:

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

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

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.

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

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

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BGD

6, C2915–C2923, 2009

Interactive Comment

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