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Biogeosciences Discussions

Interactive comment on "Root biomass responses to elevated CO₂ limit soil C sequestration in managed grasslands" *by* W. M. A. Sillen and W. I. J. Dieleman

W. M. A. Sillen and W. I. J. Dieleman

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Dear reviewer,

I have adressed every comment you have outlined individually, and written my replies directly in between lines of your remarks. To improve readability of text and tables, I have also uploaded a PDF copy.

Interactive comment on "Root biomass responses to elevated CO2 limit soil C sequestration in managed grasslands" by W. M. A. Sillen and W. I. J. Dieleman

Anonymous Referee #2 Received and published: 12 March 2012 General Comments

on “Root biomass responses to elevated CO2 limit soil C sequestration in managed grasslands” by W. M. A. Sillen and W. I. J. Dieleman

The manuscript describes a meta-analysis exploring the interacting effects of elevated atmospheric CO2, fertilization, and other management practices on the allocation of carbon to aboveground biomass, belowground biomass, the soil microbial community, and the soil carbon pool in grasslands. Overall, meta-analyses have great value in synthesizing the results of various studies and providing an important step toward scientific consensus on topics that have been extensively researched in a range of study sites and conditions. This paper provides interesting analysis results and does so us-ing appropriate and accepted methods. I recommend publication after some details and clarifications are addressed.

Specific Comments Page 358, line 24: The current atmospheric CO2 concentration is approximately 393 ppm, not 380 ppm, according to the most recent data from the National Oceanic and Atmosperic Administration in the USA. I would state something like “greater than 390 ppm” in the manuscript since these recent data are still considered to be preliminary by NOAA. See: http://www.esrl.noaa.gov/gmd/ccgg/trends/

REPLY: Ok, changed this as suggested

Page 360, lines 3-5: This would be a good place to describe the study as a meta-analysis. While it is stated in the abstract, it seems appropriate for the introduction to briefly explain the approach that was used to explore the stated hypotheses, so that readers are not expecting an experimental paper. In addition, I agree with the comments of the other reviewer that the discussion of woody plants is distracting, and I think the following sentence is a weak justification for the focus on grasslands alone: “Because of these functional differences between grasslands and tree stands, and the management component involved in grasslands, we focused on elevated CO2 effects in grasslands only.” I do think the reviews that address woody plants should be referenced but perhaps they would be a better fit in the discussion. The introduction can

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then keep a focus on the global potential of grasslands as carbon sinks, which justifies the metaanalysis strongly.

REPLY: OK, removed the section on woody plants and include a section on the approach of our study: “In this study, we used meta-analysis to investigate whether CO2 elevation and/or nitrogen fertilization is likely to change carbon storage potential in managed grasslands. By quantitatively synthesizing data from 71 studies and stratifying data into groups (e.g. fertilized versus not fertilized, irrigated versus not irrigated), we evaluated how large effects were over all studies combined, and tested whether results were consistent between groups of studies. More specifically, (hypotheses) . . .”

Overall, the introduction could provide a stronger context for the specific hypotheses explored by the meta-analysis, which are quite mechanistic. It would be useful to add a paragraph that reviews the literature on known interactions between elevated CO2, microbial dynamics, and soil carbon. The stimulating effects of high CO2 and N additions on plant productivity seem to be adequately addressed.

REPLY: The authors suggest to insert the following paragraph in the introduction: Elevated CO2 tends to increase C allocation to root compartments (Luo et al., 2006; Rogers et al., 1994) as plants need more resources to sustain the enhanced growth (Bryant et al., 1983). In addition, plants also tend to increase root exudation in elevated CO2 (Drigo et al., 2008; Fitter et al., 1997; Lukac et al., 2009). As soil organisms tend to be C-limited (Hu et al., 2006; Zak et al., 1993), these C inputs could fuel the microbial community (Heath et al., 2005; Zak et al., 2000), leading to increased microbial biomass and respiration. However, when the N necessary to convert these C inputs into microbial biomass is lacking (Heath et al., 2005; Zak et al., 2000), these C inputs are mainly respired. Therefore, Rh can increase despite the lack of change in microbial biomass. As a consequence, effects of elevated CO2 on soil C content are unclear because both C inputs and decomposition processes are stimulated, and because the effect on microbial growth and functioning seems to be modulated by N availability.

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Page 361, line 5: Means were weighted by what criteria?

REPLY: Means were weighted by measurement error of the individual numbers. We have clarified this with the following sentence: "Weighted means were calculated for experiments with data from different years, with the measurement error on individual years as weighting factor"

Page 364, line 4: ": : :and partly refutes our 1st hypothesis." It would be better to briefly restate the first hypothesis, rather than counting on the reader to recall the order in which the hypotheses were proposed. This also applies to the later references to ": : :our 4th hypothesis".

REPLY: Rewritten to: " . . .which is in sharp contrast to most other studies and refutes our first hypothesis of an increased C allocation to root compartments.

Similar for further section on hypothesis 4: "Interestingly, when excluding experiments that were irrigated or where biomass was removed, root biomass was no longer significantly decreased by elevated CO2 (data not shown). This offered support to our hypothesis that plants deprived of their shoots by harvest, burning or grazing, allocate proportionally more energy to aboveground biomass for repair and regrowth, which could impair root growth by lowering the amount of C available for belowground biomass."

Section 4.1 of the Discussion seems to repeat some information found in the previous paragraph. Can these sections be combined under the 4.1 heading?

REPLY: OK, see earlier changes made to discussion as per Reviewer 1

Page 366, lines 17-19: "We found a striking similarity between soil C and microbial biomass responses (Figs. 1–2), and opposing trends between microbial biomass and soil C responses on the one hand, and root biomass responses to elevated CO2 on the other hand (Figs. 1–2)." This sentence is difficult to figure out. Do you mean "We found a striking similarity between soil C and microbial biomass responses to elevated

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CO2, but root biomass responses showed the opposite pattern."?

REPLY: This sentence will be deleted in future versions of the manuscript.

Page 366, line 23: Elaborate on what you mean by "priming".

REPLY: Priming is clarified (see next comment) in a new version of the manuscript.

Page 367, lines 6-7: ": : :possibly because of an increased cycling of C in the soil compartments" Be specific about the mechanisms you are proposing here. Increased cycling could be related to higher respiratory activity in roots or microbes, changes in root exudates, decreased lifespan of root tissues, or a combination of these factors.

REPLY: Authors added the following section to introduce the Soil C storage paragraphs: "While microbial biomass increased in elevated CO2, its lifespan is relatively short (Zak et al., 2000; Heath et al., 2005). Moreover, root biomass production generally increases under elevated CO2, but an increased root turnover (Lukac et al., 2009) can also result in an unchanged standing root biomass under elevated CO2 (as found in this study). As such, a large proportion of root production is converted to necromass. An increased microbial and root biomass turnover would produce a source of easily degradable C compounds that could stimulate microbial activity (Dieleman et al., 2010), and possibly priming older soil C pools (for a definition of priming, see Cheng & Jonhson, 1998; Fontaine et al., 2007; Kuzyakov, 2002). At the same time, elevated CO2 also stimulates root respiration (Lukac et al., 2009). Consequently, a multitude of effects can stimulate CO2 release from the soil, and can explain why an increased root and microbial biomass did not result in an increased soil C pool under elevated CO2."

Page 367, lines 9-10: "We suggest an important role for root biomass and dynamics and their response to nutrients under elevated CO2 concentrations, based on our findings above." An important role in what? This statement is vague.

REPLY: Authors have rewritten to (follows on the previous section (see previous comment): "Based on the findings in this study, we suggest root dynamics and their re-

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sponse to nutrients under elevated CO2 play an important role in the effect of elevated CO2 on soil C storage in these grasslands."

Page 367, lines 26-27: "Moreover, respiration rates can be reduced when terrestrial systems are fertilized with large amounts of N: : :" Add a phrase explaining the mechanism.

REPLY: Authors have rewritten to: "Moreover, respiration rates can be reduced when terrestrial systems are fertilized with large amounts of N through reduced microbial biomass and/or negative effects on decomposing enzyme functioning (Treseder, 2008; Janssens et al., 2010)."

Page 368, line 7: "N resp. C." What does this mean?

REPLY: Authors have rewritten to: "N respectively C".

Figures: I suggest the x-axes be elongated so that the values of the effects are easier to read along the x-axis scale.

REPLY: To the author's opinion, elongating the scale of the x-axis will not change the readability of the effects. Instead authors will rewrite the results section in a more quantitative manner with the exact percentage change note for all results. E.g. : "Aboveground biomass increased under all three treatments (i.e. elevated CO2 (+20%), N fertilization (+37%) and their combination (+36%)). Root biomass decreased when only CO2 levels were elevated (-17%), . . ."

Figure 5: The figure legend of this graph contains a better summary of potential mechanisms and interactions than the discussion. See if you can integrate this figure into the Implications section to summarize the main findings of the study.

REPLY: In accordance with the notes from Reviewer 1, we have merged parts of the discussion. The implications-section will be introduced by a section based on the figure 5 legend.

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Figure 6 is not particularly illuminating, nor is it introduced in the results section. Omit?

REPLY: Figure 6 is omitted

Technical Comments Page 360, line 19: “We constructed a database, consisting: : :” Eliminate the comma in this phrase.

REPLY: Ok, changed this as suggested

Page 362, line 19: “: : :aboveground biomass responded equally strong to different fertilizer types” Change “equally strong” to “similarly”.

REPLY: Ok, changed this as suggested

Page 365, line 11: “In addition, in the single factor fertilization treatment, aboveground biomass tended to respond stronger to NPK fertilizers: : :” Change “stronger” to “more strongly”.

REPLY: Ok, changed this as suggested

Page 366, line 25: “: : :when purely N was added to grasslands” Change “purely” to “only”.

REPLY: This section is deleted in a new version of the manuscript.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C315/2012/bgd-9-C315-2012-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 9, 357, 2012.