

***Interactive comment on* “Carbon losses from pyrolysed and original wood in a forest soil under natural and increased N deposition” by B. Maestrini et al.**

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We thank the reviewer for the constructive and useful comments. We have responded with detailed explanations for each the suggestions/comments. In addition, we specified any edits/changes made to the revised manuscript. Below we show the reviewer’s comments *in italics*, and our responses follow in normal font.

This study by Maestrini et al. focuses on carbon loss pathways of a laboratory produced pyrolysed pine wood (i.e. referred to as pyrolysed organic matter, PyOM, by the authors) and the original pine wood in a Cambisol under a temperate forest. The most interesting and novel aspect is that by using the ¹³C-enriched (2.03-2.05

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atom %) pyrolysed wood and original wood, the authors were able to (i) determine mineralization rate of PyOM- and wood-C in total soil respiration, (ii) quantify the loss of dissolved organic C in PyOM and wood via leaching, and (iii) assess the impact of PyOM and wood on native soil organic C (SOC) mineralization. Then by fitting the one-pool decay model to the initial and remaining quantity of C after one year, the authors estimated mean residence time (MRT) of C in POM and wood, with and without N deposition. It should be noted that all these processes were studied in a forest gap location under field conditions in the absence of root and root-derived C inputs.

This study by Maestrini et al. presents complementary findings to those reported by Singh N et al. (2014, GCB 20, 1629-1642) on the transformation and stabilization of PyOM and original wood organic matter (13C and 15N labelled) in the same experiment. It will be informative for readers if the authors include a brief discussion on the implications of their combined findings (current study and Singh N et al. 2014) from the same experiment for C storage in the forest soil.

We have briefly summarized the results from Singh et al. (2014), which was recently published in *Global Change Biology*. In particular we highlighted the following aspects in our revised discussion:

1) Losses of PyOM and changes in quality of PyOM.

Our result of PyOM-C loss is consistent with the findings from Singh et al. (2014). We added the following to the discussion section on the loss rate of PyOM-C:

Section 4.4 “Singh et al. (2014) measured the recovery of PyOM in the same experiment and found that PyOM losses were less than 1%, in agreement with our findings. After ten months in situ Singh et al. (2104) observed a change in the chemical composition of the remaining PyOM in soil. They reported that the relative contents of the benzene polycarboxylic acids (i.e., a biomarker specific to PyOM) in the soil had changed; and observed a decrease of the contribution of the highly condensed BPCA rings (B6CA). Nonetheless at the present stage it is not possible to conclude

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if the decrease of the relative contribution of the different BPCAs can be linked to the mineralization rate of PyOM (Singh et al. 2014).”

2) Influence of added inorganic N on PyOM-C loss during 10 months in situ.

Our findings of lower PyOM-C losses with increased N additions were not consistent with Singh et al. 2014. In the present study we observed a significant decrease in CO₂ loss from PyOM with added N compared with no added N, while in Singh et al. 2014 this difference was not significant when looking at total recovery of added PyOM in the soil after 10 months.

In Section 4.2 we noted that Singh et al. (2014) measured the soil recovery of PyOM in the same experiment and found that PyOM recovery was not significantly different. In our paper, we were able to have a more direct measurement of PyOM-C loss as CO₂. Thus, with better resolution, we were able to quantify this N effect that was not possible when looking at soil recovery in Singh et al. 2014.

Moreover in our discussion section, we discuss possible mechanisms responsible for the decrease of PyOM mineralization under increased N, and discuss them together with findings from other studies, employing the same PyOM (Santos et al. 2012) or the same soil (Maestrini et al. 2014). Particularly we discuss the “N mining theory” where the added N meets the N demand of microbes. According to this theory without this additional N, under ambient N heterotrophs are forced to “mine” the SOM of the substrate to obtain N, and relate it to findings from Singh et al. (2014) on recovery of PyOM-N in microbial biomass and from Santos et al. (2012) on the effect of N addition on PyOM-N mineralization. Particularly Singh et al. 2014, did not find an appreciable quantity of PyOM derived N in the microbial biomass suggesting that PyOM is not likely to represent a major source of N for microbes, and thus not confirming the N mining hypotheses in our case.

Furthermore we discuss the possibility that N addition depresses the activity of phenol-oxidase and relate it to findings from Maestrini et al. (2014) who did not observe a change in the potential activity of phenol-oxidase under increased N in an incubation experiment carried out in the same soil.

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3) PyOM translocation vertically down the soil profile.

Singh et al. (2014) observed a small amount of PyOM had moved below the surface soil application zone. Approximately 1% of applied PyOM-C was recovered within the 10-15 cm soil depth. However it was not possible to resolve the chemical composition of the PyOM that had leached down the profile or whether the PyOM would accumulate there. A similar question was posed by Leifeld et al. (2007).

Our results showed negligible losses of PyOM-C as DOC. This combined with the results from Singh et al. (2014) are an important findings that suggests that PyOM is more likely, at least over the first year, to accumulate in deeper horizon than being lost from the soil as DOC. We have added this to section 4.3.

4) Priming effect.

Singh et al. (2014) did not observe a change in native soil organic carbon from added wood or PyOM by examining the total native soil C. However, Singh et al. 2014 did observe a decrease in free light fraction C with added PyOM compared with the unamended control and added wood treatments. We have revised the section on priming effects and have included results published very recently including the review paper on priming effect from Maestrini et al. (2014, DOI:10.1111/gcbb.12194), the article by Singh and Cowie (2014), and the findings from Singh et al. 2014 mentioned above.

We applied the theory by Singh and Cowie (2014) on the relationship between positive priming effect induced by PyOM and the size of the labile fraction estimated by examining the labile pool size derived from two-pool decay models of PyOM-C turnover from published incubation experiments.

Thus we posit that the positive priming effect observed by Singh et al. (2014) in the free light fraction could be related to the presence of a small labile fraction in PyOM.

A revised section on priming effect is reported here below:

“We did not observe a significant change in native soil organic C mineralization due to the addition PyOM or wood (Figure 4). Singh et al. (2014) measured the recovery

of total native soil organic C in the same experimental after 10 months and observed no significant changes in the soil organic C content, but observed a decrease in the free light fraction C content with added PyOM compared with the unamended control treatments.

Also Abiven and Andreoli, (2010) who incubated PyOM and litter in the same soil, or Santos et al. (2012) who incubated the same substrate in different soils did not observe a priming effect in either experiment.

Maestrini et al. (2014) conducted a meta-analysis on priming effect induced by PyOM on the short term and found that a short-term positive priming effect is reported in most of the cases. Singh and Cowie, (2014) observed a correlation between the short-term priming effect induced by PyOM and the quantity of labile C present in PyOM, determined using a two-pool model.

The quantity of labile C present in our PyOM, determined by using as a proxy for labile C the two pool model derived from an incubation experiment (Santos et al. 2012) was similar to the quantity of labile C present in the wood-derived PyOM pyrolysed at 550 °C used by Singh and Cowie, (2014) (1.1-1.5 % vs 1.3%). In both cases this addition did not induce a priming effect when measured as CO₂ efflux. Therefore our results suggest that a short-term priming effect induced by added PyOM may be caused by the presence of labile C in PyOM.

Luo et al. (2011) suggested that the increase in pH following from PyOM addition may contribute to the positive priming effect induced by PyOM. In the present experiment, soil pH did not differ between PyOM and control treatment 10 months after the addition of the substrate (personal communication). Nonetheless we cannot rule out the hypotheses that PyOM temporarily increased the pH of soil at the beginning of the experiment as observed in Maestrini, Herrmann, et al., (2014). In a field experiment Major et al. (2010) found that PyOM addition increased native soil organic C respiration and attributed it to soil respiration following from the increased root respiration due to the increased plant productivity in presence of PyOM.

In the present experiment it is possible that PyOM triggered the activity of microbes

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which increased the mineralization of the accessible organic matter (the free light fraction), but that this change was too small to be observed in the native organic matter derived CO₂ or in the bulk native soil organic matter recovery after 10 months.

Furthermore, this study is one of the few studies that have measured C losses via direct mineralization and priming effects of both PyOM and original feedstock biomass. Like Whitman et al. 2013, OG, 64, 76-83) who proposed the C credit-debit concept (i.e. the ratio of C remaining in PyOM after its production and soil application to the C remaining in original feedstock after soil application) and Herath et al. (2014, GCB, doi: 10.1111/gcbb.12183) who incorporated the priming effects of PyOM and original feedstock in the C credit-debit calculations, the authors could make similar efforts to determine to the net C balance of producing, applying, and storing the PyOM. These calculations may also address the relevant comment made by N. Gurwick.

We agree with the reviewer and N. Gurwick that the approach of C credit/debit may be useful to determine the effectiveness of PyOM to store C. However since the priming effect observed in the present study was not significant we did not include it in the debit/credit C ratio. We therefore decided to limit our analysis to the C debit or credit ratio as determined by equation 4 in Herath et al. 2014. The variation of the indicator over time was reported in figure 1 of the present answer, this indicator proposes that after 2.3 years the Credit/ratio will be higher than 1.

We have added the following lines to section 4.1 on PyOM and wood stabilization:

“We predicted the ratio between the PyOM-C remaining in soil and the one remaining in its parent material (wood) over time using equation 8 and found that their ratio would be greater than one after two years. This may have important implications when computing C budget of post-fire sites, stressing the risk of an overestimation of the losses by forest fires when PyOM is not taken into account. Nonetheless caution should be used when applying this indicator as it does not take into account the influence of fire events on plant growth.”

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This is high quality research and addresses the knowledge gap on how much PyOM decomposes and for how long it can reside in soil under field conditions. The authors have clearly highlighted this knowledge gap issue in their Introduction. Perhaps the authors could cite the one of the few long-term laboratory studies (Singh et al. 2012, EST, 46, 11770–11778), which clearly demonstrated that the MRT of PyOM-C changes with the length of incubation, and conclude in their discussion that longer-term incubation studies (whether in the field or in the laboratory) are required to obtain robust MRT estimates of PyOM in soil.

We agree with the reviewer on the importance of the publication mentioned, we will stress in the introduction the importance of long-term studies. In fact the length of the observation period may influence model parameters. We believe the strength of the present research resides in its experimental design (field experiment, measuring different loss pathway) rather than in the contribution to the development of robust models for PyOM stability. Moreover, this paper presents results from the first year in situ, which may be much more accurate at predicting MRT than laboratory studies which cannot represent the energy and climate conditions in a forest soil.

Furthermore, it is known that the one-pool exponential model tends to underestimate the MRT of PyOM particularly in short-term incubation studies that would reflect mainly the mineralization of relatively labile compounds. It will be good if the authors could explore the possibility of using different models e.g. a two-pool exponential model or a model with multiple pools or a model incorporating temperature functions to reflect the influence of environmental factors on PyOM mineralization.

We explored the possibility of applying different models to our data, (see figure 5 of this answer), nonetheless the shape of our PyOM decomposition curve does not follow any of the three model tested (linear, double-exponential, single pool exponential), maybe because of the interaction of the seasonal variations with the decomposition

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patterns (as also the reviewer suggest in the following point).

Therefore we decided to choose the method adopted by Singh et al. 2012, that assumes that PyOM decomposition follows a single pool decomposition model and calculate it using the initial and final quantity of PyOM remained.

For example, in the current study by Maestrini et al., there is a clear pattern of slower mineralization with increasing time in the first few weeks and then PyOM-C mineralization increased possibly in response to increasing temperature in the summer season. This behaviour of PyOM-C mineralization further supports the argument that that longer-term studies of several years are required to obtain robust estimates of MRT of PyOM, which generally mineralizes very slowly than non-pyrolysed OM.

Yes we agree that longer-term studies are necessary to quantify the losses of PyOM. For the current experiment it was not possible to measure PyOM losses further but we are planning to monitor losses of PyOM from the soil in the future by measuring the isotopic signature of the soil amended with PyOM in the remaining mesocosms in the field.

In this paper, I have noted a few issues where the Keeling plot was used to determine the $\delta^{13}\text{C}$ of the soil-respired CO_2 . The main issue is the application of a diffusional correction factor of -4.4‰ to the soil ^{13}C - CO_2 efflux. However, this correction is only applied where $\delta^{13}\text{C}$ in soil profile CO_2 is measured followed by its conversion to $\delta^{13}\text{C}$ of soil-respired CO_2 at the soil surface by applying the diffusional correction factor (e.g. see Mortazavi et al. 2004, BG, 1, 1-9; and Cerling et al. 1991, GCA, 55, 3403). If the authors agree on this point, then they need to correct their data reported in Figs. 2-4 and Fig. 2S, although these corrections may make only small changes to the reported conclusions.

Figure 2-4 and 2S was changed removing the -4.4 correction. As supposed by the reviewer this did produced minor changes in the figures: please see Figure 2-4 in the present response.

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The other related issue is that the authors collected three CO₂ gas samples from the enclosed chamber over a 9-min period, which would limit 1/CO₂ values (x-axis of the Keeling plot) in the narrow range, and extrapolation of the regression line from this narrow range to the intercept may provide a more uncertain d¹³C of respired CO₂ than when the range of 1/CO₂ values is extended (see Ohlsson et al. 2005, SBB, 37, 2273–2276). It will be great if the authors could provide some further justification to their approach (in addition to discarding the data when R² was < 0.9) to determine d¹³C of the respired CO₂ with greater certainty. In relation to this, the authors need to report their d¹³C data of the soil-respired CO₂ from the control and amended plots, including the d¹³C value of the control soil (ideally in the main paper but at a minimum in the Supporting Information).

Thanks for mentioning the Ohlsson et al. 2005 paper as it clearly demonstrated the importance of having a wide CO₂ range in Keeling plots. We considered your publication together with the meta-analysis from Pataki et al. 2003, which shows that the error in the intercept due to the narrow range of the CO₂ analysed dramatically drops after the 50 ppm interval threshold (Pataki et al. 2003, GCB 17(22), fig. 5).

We analysed the distribution of the CO₂ ranges in our keeling plot (see Figure 5 of the present response) and found that 90% of our Keeling plots had a CO₂ range from the ambient CO₂ concentration to the highest sample higher than 60 ppm. Such a result was reachable thanks to the adoption of a relatively small chamber (800 ml).

We added the following text to in the section on materials and methods: "Pataki, (2003) shows that the error in the intercept due to the narrow range of the CO₂ analysed, i.e. the difference in CO₂ concentration between the highest and the lowest point, drops after exceeding the 50 ppm interval, meaning that the quality of the keeling plots is much higher above these CO₂ concentration ranges . We analysed the distribution of the CO₂ ranges in our keeling plot (see Figure 4 supplementary) and found that

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90% of our Keeling plots had a range higher than 60 ppm, therefore we are confident that closing the chambers for a longer period of time would have lead only a minor decrease of the uncertainty in the intercept."

Specific and technical comments: - *Page 4, line 4. Suggest replacing 'reduced' with 'attributed'.*

Agreed, text was edited accordingly

- *Page 4, after lines 7-9. This pattern of overestimation of turnover time (or underestimation of MRT) during short-term incubations was clearly shown by Singh et al. (2012,EST, 46, 11770) by comparing MRT at different incubation periods over five years.*

We added the citation suggested by the reviewer.

- *Page 4, lines 11-13. There is actually a lack of both short- and long-term field experiments on the topics studied in this and Singh et al. (2014) papers and are needed to understand the C storage and stabilization implications of PyOM in particular.*

We agree with the reviewer on the importance of field studies both long and short term, therefore we removed the word short-term in this sentence.

- *Page 4, lines 14-16. Losses of PyOM from the applied soil layer to deeper layers can occur in dissolved and particulate forms, and this was also indicated by the authors in Singh et al. (2014). It will be informative for readers if the authors briefly discuss the wider results of both studies (the current study and Singh et al. 2014) from the same experiment.*

We have added additional information from Singh et al. 2014 as described in the prior response.

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- Page 5, lines 15-19. *A number of possible mechanisms have been suggested by Singh and Cowie (2014, Scientific Reports, doi:10.1038/srep03687) monitoring priming effects of a range of biochars over a 5-year period in a laboratory incubation study, and the size of positive priming was related to the lability of biochars. Considering wood derived PyOM would have limited amount of labile organic components and particularly the tested biochar decomposed at a much slower rate, so this suggests the limited positive priming potential of the tested biochar, and furthermore biochar-clay interactions would tend to stabilize native soil organic matter over time. I suggest that the authors should provide further insights into the discussion section that why their study did not observe significant positive priming by the wood-derived PyOM.*

As specified previously, we have revised the section on priming effects to include papers published during the review process for this manuscript (i.e., Singh and Cowie, 2014, Maestrini et al. 2014, Singh et al. 2014). We considered the hypothesis that PyOM may contain a labile C fraction that could stimulate heterotrophic microbial activity and accelerate native SOM mineralization. We also compared our results to relevant PyOM incubation studies that examined priming effects from this PyOM material (Santos et al. 2012) and in this forest soil (Maestrini et al. 2014b).

-Page 6, lines 1-8. *In the Introduction section, the authors provided background information on PyOM mineralization, leaching losses and priming effects but there is no literature review presented on such effects of non-pyrolysed woody material. It would be worthwhile to briefly review any published findings on the decomposition of woody materials (in the presence or absence of N deposition) and address the question of why PyOM was compared with original wood e.g. to understand C storage implications in the system context of producing and applying PyOM to soil.*

We agree and have added findings from meta-analyses on wood decomposition

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(Jannsens et al. 2010; Knorr et al. 2005) and results from experimental studies that were carried out in temperate ecosystems (i.e., Bebber et al. 2011, Hagedorn et al. 2012).

-Page 7, line 3. “without coarse roots entering”? Were there any roots present in the collar? I thought the experimental chambers were located in a forest gap of 0.5 ha, so both fine and coarse roots were not present anyway.

When the mesocosms were extracted 10 months after litter addition and 6 months after field installation (Singh et al. 2014), we did not find any living roots that had entered the mesocosms through the “meshed” windows. The aim of the meshed windows was to allow the soil inside the mesocosm to have similar chemical, biological and physical conditions to the soil outside the mesocosm. In addition, this approach has allowed for fungal and faunal movement to/from the surrounding soil in previous experiments (i.e., Bird and Torn, 2006 Biogeosciences). The mesh size (0.7 mm) in this mesocosm design was intended to allow for only fine root entry and to prevent coarse roots. The field site for this experiment is a forest gap that was naturally created in 2006 by a storm event. The growth of new plants during this time period was at an initial phase. Any vegetation growing inside the mesocosms was cut when observed on a regular basis. Overall, we posit that our field site represents fairly typical gap conditions where you would likely see recent fire-derived PyOM deposition (e.g., climate, sparse vegetation, etc.). We acknowledge that the lack of active rhizodeposition into the mesocosms could have an effect on C cycling process – and have noted the possible implications of this in our edited manuscript. However, our main intent was to locate our 10-y long field study at a forested site that would be representative of a location where PyOM would be naturally deposited after a fire event.

-Page 9, lines 5. Where is the data for solution pH and EC?

These data were added to the supplementary materials, and also attached to this

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response (Figure 7 and 8).

-Page 9, lines 23-25. Is this a right approach? Why can't the authors leave this as missing values in the statistical analysis?

We agree and reintroduced the missing values for CO₂ efflux, where these were substituted with the average of that sampling date. This resulted in minor changes to figures 2-4 (corresponding to figures 2-4 also in the present response). The final estimate of mineralization of PyOM-C to CO₂ changed from 0.54±0.7 to 0.52 ± 0.5 (ambient N), and 0.27 ± 0.07 to 0.27 ± 0.08 (under increased N). For just the unamended treatment we used the procedure by Major et al. (2010) where missing values of the isotopic signature of the unamended control were substituted with the average blanks from the other plots of the same sampling date. In the DOC data we substituted the missing values with the average of the same treatment at that sampling date. The adoption of this approach was necessary in order to proceed in the analysis of the cumulative losses as DOC.

- Page 10, line 9. Replace 'soil CO₂' with 'soil-respired CO₂'.

We agree and edited the manuscript accordingly.

-Page 10, line 14. Replace "mineralization rate of the native soil organic matter" with "mineralization rate of the native soil organic C" throughout the manuscript; this is what the authors have measured but not the mineralization of other components (e.g. organic nitrogen) in soil organic matter.

We agree with the reviewer and changed the manuscript accordingly.

-Page 12, line 4. The data on soil water conductivity and soil water pH have not been reported in this paper.

EC and pH have been reported in figures 7 and 8, respectively.

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-Page 12, Lines 6-17. It seems a bit odd that the authors did the repeated measures ANOVA using SPSS, but then they did the "individual comparisons" in R ("agricolae" package). The SPSS repeated measures package would have had the capabilities to do posthoc pairwise comparisons using Tukey's approach.

At the time the ANOVA analyses were performed, we did not have a version of R that could perform the repeated measures ANOVA. The graphs were produced using an R script that performed post-hoc tests on the factor time and indicate on the graph which sampling dates did differ significantly.

-Page 13, first paragraph. The authors should also report the measured d13C values that allowed them to calculate the fraction of soil respiration derived from the substrate (e.g. see Figure S6 in the Supporting Information of Singh et al. 2012, EST, 46, 11770); this will allow the readers to acquire further insights into the 13C isotopic values during respiration and to know how different they are from the native or added C substrates.

These data have been added to the supplementary materials and are reported here (figure 9 of the present answer).

-Page 13, lines 23-25. Change "native soil organic matter" to "native soil organic C" and delete "calculated as soil respiration minus: : :: : :in the control treatment", as this is already explained in the method section. Agreed, the manuscript was edited accordingly.

-Page 13, lines 27-28. Are you talking about positive or negative priming here? Clarify. Here we speak of priming effect, irrespective of the direction. This was changed to "no significant positive or negative priming was observed."

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Page 14, line 20. The authors should also mention that roots were not present in the experimental blocks in the field i.e. PyOM mineralization was studied in the absence of plants in the field.

We changed this to: “We estimated PyOM turnover time in a field experiment under excluded root conditions to be 191 ± 24 years.”

Page 15, lines 12-14 need to be reworded; rhizodeposition would be present, whether biomass is increased or not, and any biomass increase would increase the magnitude of rhizodeposition. Furthermore, the mean annual temperature in the study of Major et al. (2010, GCB 16, 1366-1379) was 26C, relative to 10C in this study, so this could be also one of the reasons for a higher MRT in this study than Major et al. (2010). Furthermore, the reported MRT value by Major et al. (2010) was around 600 years using a two-pool exponential model, so the authors need to mention the model they used to estimate MRT of 89 years in the study of Major et al. (2010).

We used the same approach as Singh N. et al. (2012) to compare the mean residence to all studies. We calculated turnover times based on two data points for each study, the initial stock of PyOM-C and final PyOM-C remaining at the end of the experiment.

Therefore the calculations we did to obtain the MRT of 89 years in Major et al. 2012 was:

Given that they observed a 2.2 percent loss over 2 years:

$(100-97.8)=100 \cdot e^{-k \cdot t}$; $-k \cdot t = \ln(100/97.8)$; $k = \ln(100/97.8)/2$; $k = -0.43$; $MRT = -(1/k)$; $MRT = 89$ years.

We added to the manuscript that we adopted approach from Singh et al. 2014. On this point, Gurwick, who commented the present manuscript, observed in his review (2014) that it exist a problem of “common currency” when comparing studies on PyOM stability.

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-Page 15, line 26. Replace the conference citation by Fang et al. (2014), EJSS, 65, 60-71. Edited accordingly

- Page 17, line 2. Delete 'organic matter' between 'soil' and 'respiration'; Major et al. measured soil respiration that included CO₂ emission from root and soil C sources. This needs to be clarified. Furthermore, Major et al. attributed the increased soil respiration to greater root contributions from increased pasture productivity in the presence of PyOM.

As previously noted, this paragraph was edited to address this concern.

Page 17, line 8. In this study, the PyOM was mixed with the soil in the top 1 cm depth and only a small proportion was moved below the applied layer (Singh et al. 2014). I suggest that the authors need to expand discussion and identify other factors (in addition to the addition rate) that may have led to only small positive priming by the woody PyOM; it may be related to the lower lability (or alternatively high stability) of a wood-derived PyOM (see Singh and Cowie 2014); the increasing addition rate would also increase the net amount of labile C being added along with the bulk PyOM; this needs further investigation in different soil types, as clay content and the related biochar-clay interactions may work against the positive priming of native soil organic C by PyOM.

As previously noted, this paragraph was edited to address this concern.

-Page 18, line 1. mention 'sandy' before 'Oxisol'.

Added.

-Page 20, line 3. "effect of N additions" on what? litter? added organic matter?

This sentence has been clarified in the revised manuscript.

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-Page 20, line 8. Delete “per year”, as “in the first year” is mentioned in the previous line.

Added.

- Table 1. *did the authors measure the aromatic-C content of PyOM and original wood? These data should be reported. What was the $\delta^{13}\text{C}$ value of native soil organic matter?*

Details on the chemical properties of our PyOM are reported in (Chatterjee 2012) as noted on page 7, line 26. The Chatterjee et al. 2012 paper presents a very detailed analysis of the chemical characteristics of our PyOM and its precursor, wood, based on ^{13}C ss NMR among other approaches.

Data on the isotopic enrichment of BPCAs from the PyOM used in this study have been reported in Yarnes et al. 2011 – this has been noted in the methods section. The ^{13}C enrichment of BPCAs is similar to the overall ^{13}C enrichment in the PyOM and the wood. The enrichment of the native SOM was 1.07 atom% and has been added to Table 1.

-Table 2. *Please check the caption to confirm if the “Eq. (3)” was used? It does not seem right. Also, $\delta^{13}\text{C}$ values of DOC need to be included e.g. as volume-weighted averages (see Major et al. 2010).*

We thank the reviewer for their careful reading of the manuscript. It is not equation 3 but equation 1. We have corrected this error and presented ^{13}C - DOC as volume-weighted averages (table 1 in this response).

-Fig. 4 *caption needs to be corrected; the empty symbol represents the treatment under ambient N and the full symbol represents the treatment under increased N.*

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Corrected.

Figures attached to the present reply: Figure 1 of answers: C credit/debt ratio. The black lines (left axis) represent remaining PyOM (continuous line) and wood (dashed line) as estimated by one single pool, two point decomposition model. The red line (right axis) represents the C credit ratio, when this is higher than 1 this means that more C is stored in the soil compared to the C initially present in the feedstock.

Figure 2 of the present answer: Fraction derived from PyOM and wood after the removal of the -4.4 correction (corresponding to figure 2 of the submitted manuscript). The figure represent the fraction of soil respiration derived from PyOM (a) and from wood (b). The sampling dates within the same treatment with different letters (on the top) are significantly different ($p < 0.05$ TukeyHSD test). The first line of letters refers to the ambient N treatment and the second to the increased N treatment. Error bars represent the standard error of the mean ($n = 3$). Asterisks (*) indicate when the fraction is significantly different from 0 (ambient and increased N treatment were pooled together, $n = 6$, $p < 0.05$).

Figure 3 of the present answer: after the removal of the correction factor -4.4 (figure 3 in the submitted manuscript). The figure represent Mineralization rate (in $\mu\text{molCO}_2 \text{ m}^{-2} \text{ s}^{-1}$) of wood (a) and PyOM (b). Empty symbols represent the treatment under ambient N and full symbols under increased N. Asterisks (*) indicate the dates when the addition of N significantly affected substrate decomposition (paired t test, $p < 0.05$). Error bars represent the standard error of the mean ($n = 3$).

Figure 4 of the present answer: Priming effect after the removal of the correction factor -4.4 (figure 3 in the submitted manuscript). Priming effect induced by PyOM F(aig)urac n4d wood (b) in $\mu\text{molCO}_2 \text{ m}^{-2} \text{ s}^{-1}$, derived from Eq. (2). Full symbols represent the treatment under ambient N and the empty symbols represent the treatment under increased N. Error bars represent the standard error of the mean ($n = 3$).

Figure 5 answer: Distribution of the range of the Keeling plots examined in the present study.

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Figure 6 of the present answer: Comparison of different models applied to our decomposition study, the three models basically overlap to one single lines. the adoption of single or double pool models does not improve the fitness of the model (see residual sum of squares reported above).

Figure 7 of the present answer: Conductivity measured on the different sampling dates.

Figure 8 of the present answer: pH measured on the different sampling dates.

Figure 9 of the present answer: Isotopic signature of the $\delta^{13}\text{C}$ measured in the present experiment.

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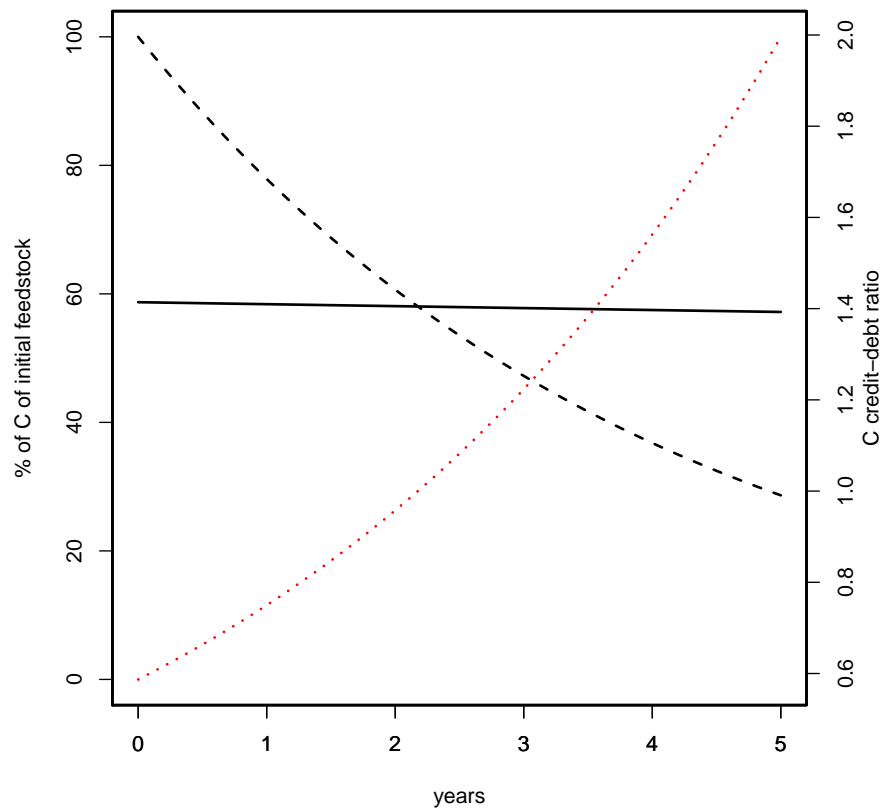


Fig. 1.

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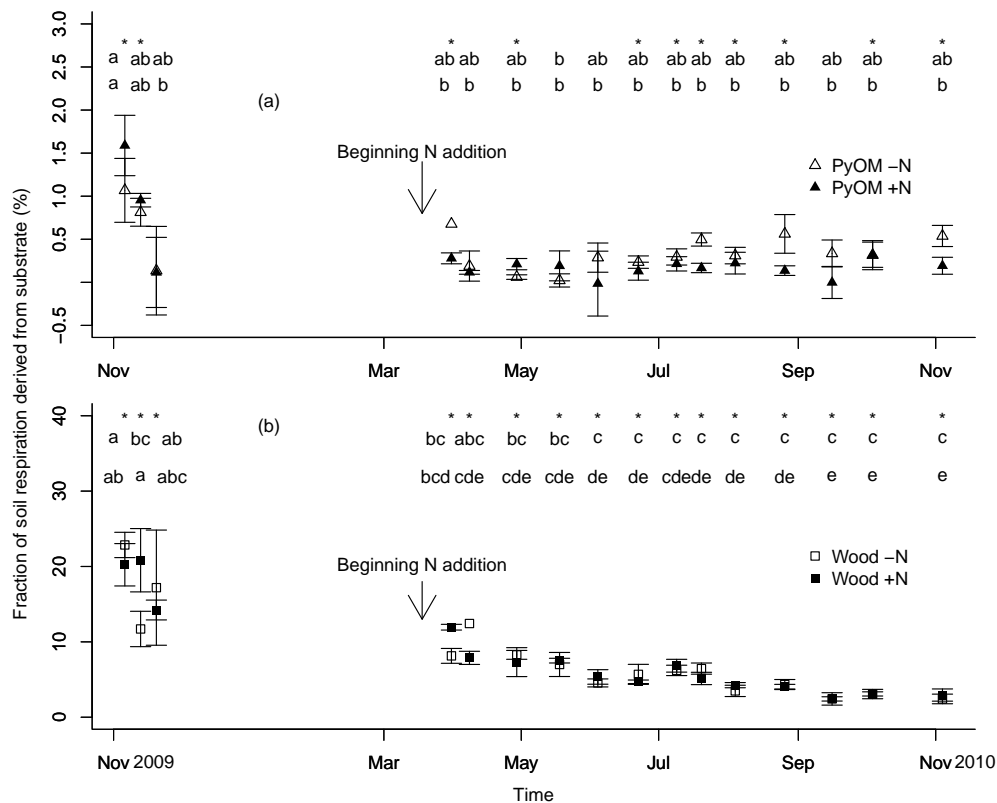


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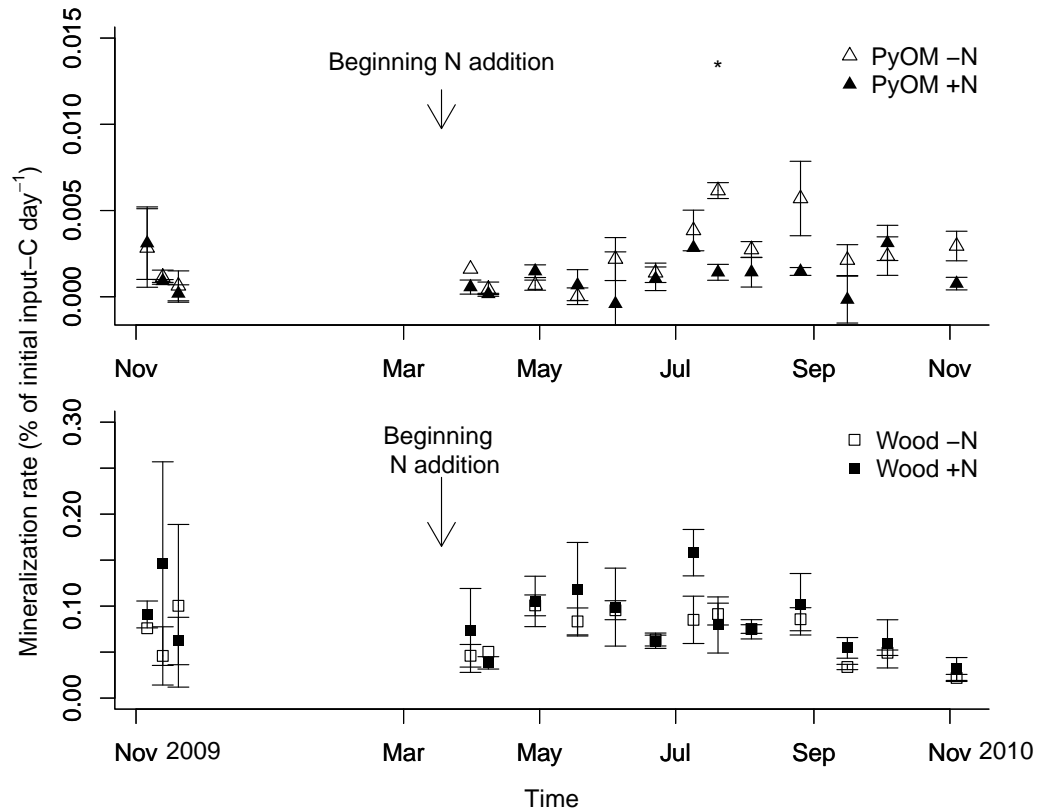


Fig. 3.

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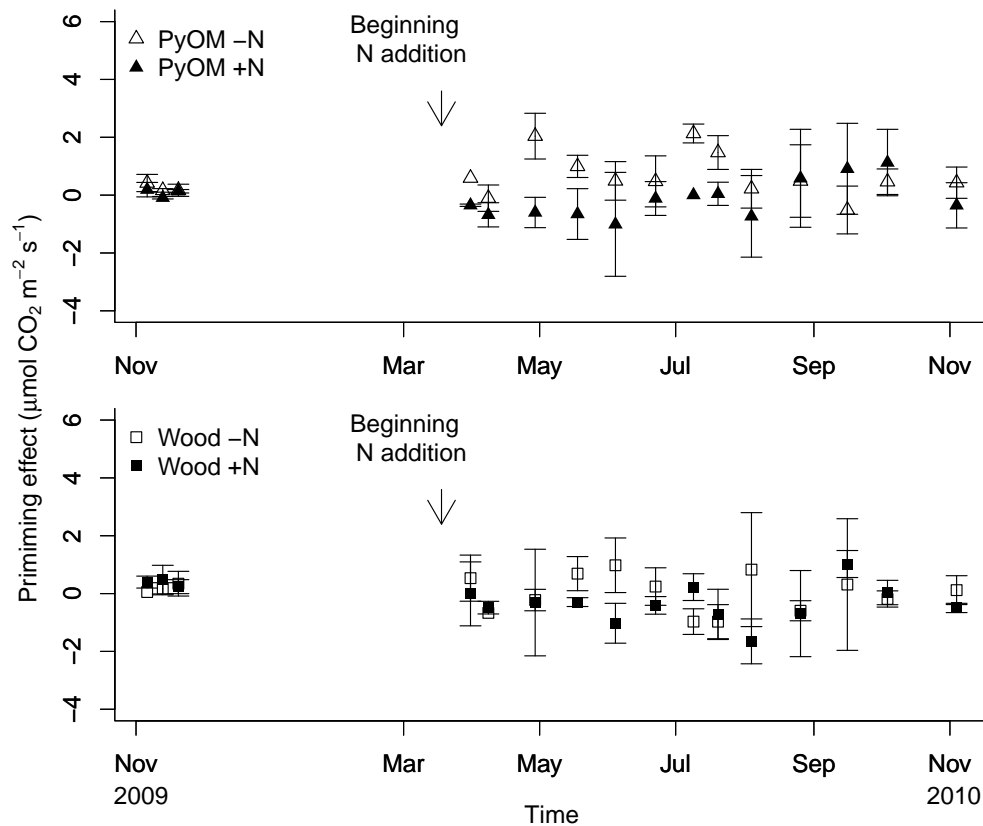


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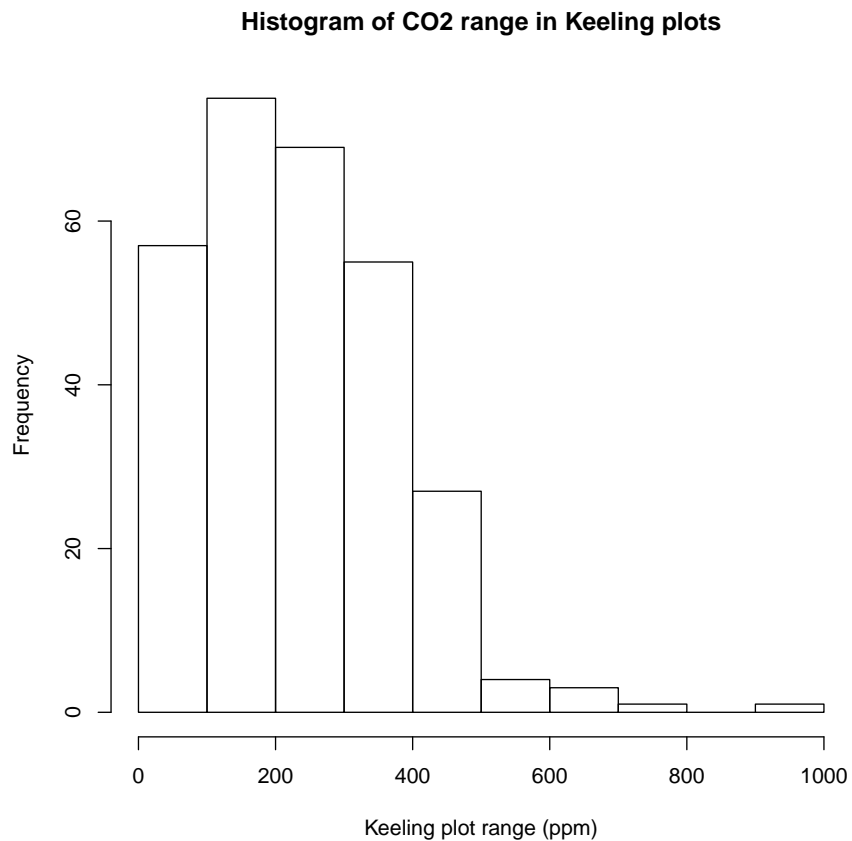


Fig. 5.

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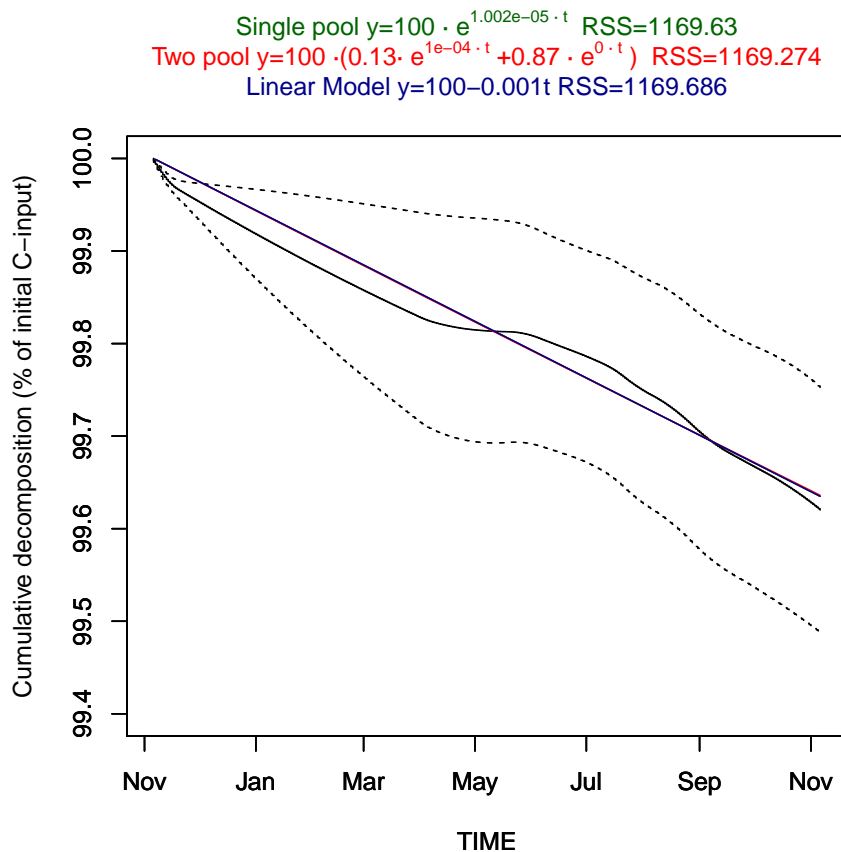


Fig. 6.

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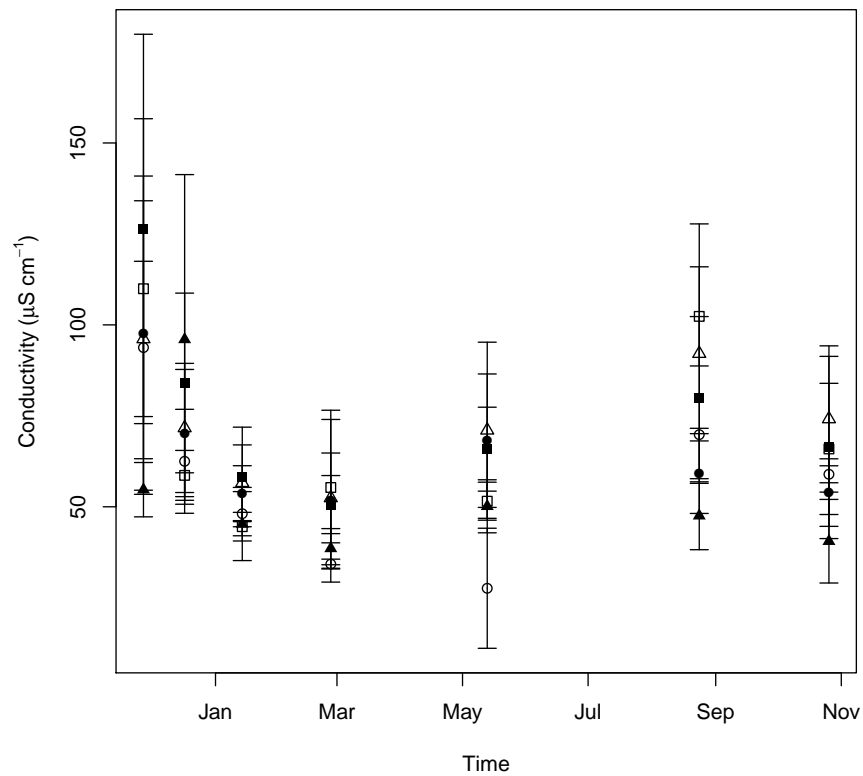
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Fig. 7.

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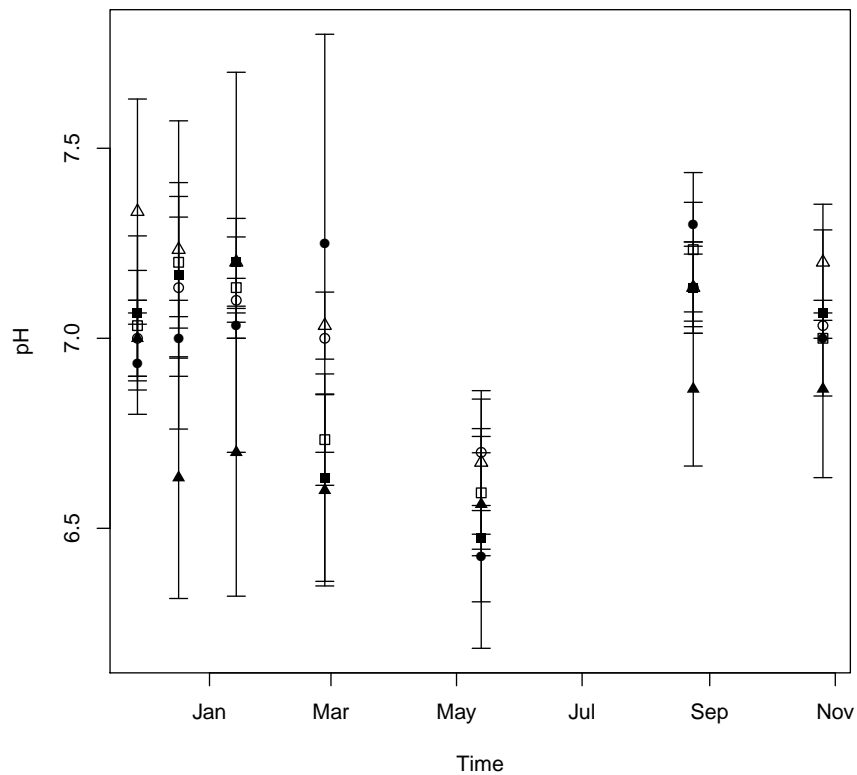


Fig. 8.

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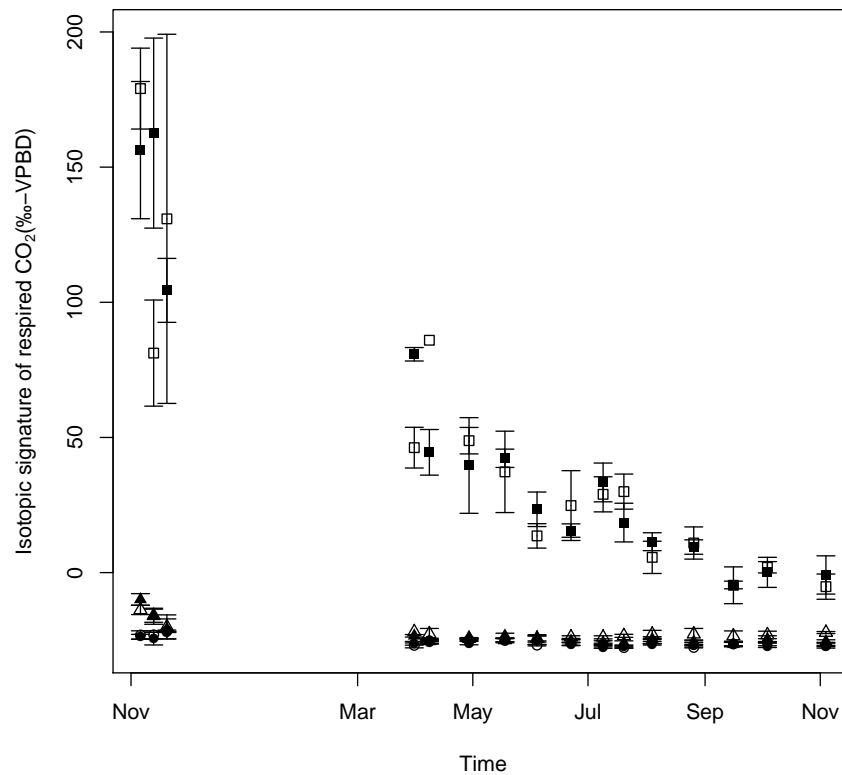
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Fig. 9.

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