Biogeosciences
Open Access
Discussions

Interactive
Comment

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.

B. Singh (Referee)

bp.singh@dpi.nsw.gov.au

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 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-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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.

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, GCBB, 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.

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. Furthermore, it is known that the one-pool exponential model tends to underestimate the MRT of PyOM particularly in short-term incubation

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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. 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 behavior 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.

In this paper, I have noted a few issues where the Keeling plot was used to determine the d13C of the soil-respired CO2. The main issue is the application of a diffusional correction factor of -4.4‰ to the soil 13C-CO2 efflux. However, this correction is only applied where d13C in soil profile CO2 is measured followed by its conversion to d13C of soil-respired CO2 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.

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).

Specific and technical comments:

Page 4, line 4. Suggest replacing 'reduced' with 'attributed'.

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.

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.

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.

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.

Page 6, lines 1-8. In the Introduction section, the authors provided background infor-

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

mation 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.

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.

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

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

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

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.

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

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.

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.

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.

Page 13, lines 27-28. Are you talking about positive or negative priming here? Clarify.

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.

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).

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

Page 17, line 2. Delete 'organic matter' between 'soil' and 'respiration'; Major et al. measured soil respiration that included CO_2 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.

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.

C2685

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.

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

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

Page 20, line 8. Delete "per year', as "in the first year" is mentioned in the previous line.

Table 1. did the authors measure the aromatic-C content of PyOM and original wood? These data should be reported. What was the d13C value of native soil organic matter?

Table 2. Please check the caption to confirm if the "Eq. (3)" was used? It does not seem right. Also, d13C values of DOC need to be included e.g. as volume-weighted averages (see Major et al. 2010).

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.

Interactive comment on Biogeosciences Discuss., 11, 1, 2014.