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Biogeosciences Discussions

Interactive comment on "Low vertical transfer rates of carbon inferred from radiocarbon analysis in an Amazon podzol" *by* C. A. Sierra et al.

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We thank the reviewer for his/her constructive comments. Here we provide detailed answers to all of them. Reviewer's comments are presented in italics and our response in normal font.

General comments

The manuscript deals with the cycle of organic matter (OM) in the Amazon, where specific OM primary production is one of the largest in the world and where soils, specifically podzols, can store large amounts of carbon, raising the need for a better knowl-

edge of the carbon cycle in such areas. The results are based on a very interesting scientific approach using bomb radiocarbon to constrain OM dynamic modeling. Such type of work deserves to be published. In this manuscript, however, presentation of data, model parameterization and scientific discussion require so many improvements that most of the paper needs to be rewritten. The main points are that presentation of the study site and soil description are insufficient, number of samples and samples location are questionable, the model has to be better described with regard to removal of dissolved organic matter by lateral water flow in the podzol, the representativeness of the studied podzol with regard to other hydromorphic podzols and its stage of evolution has to be discussed more consistently. As I think that such type of studies are of great interest, I strongly recommend the authors to address all the following comments and consequently to entirely rework the manuscript.

Response: We did significant changes to the manuscript based on the reviewer's comments. The newest version is not a complete rewrite, but it incorporates new data, a reformulation of the model, and provides more detailed descriptions about the sampling strategy, statistical, and modeling analyses.

Despite the major changes made on the manuscript the main conclusions remain the same. However, we now provide better data describing the profiles that allows better comparisons with other soils and interpretation of the results.

Specific comments

- *The soil description is actually insufficient. The ZAR-01 podzol was roughly described in Quesada et al., 2011 and ZAR-04 Alisol was not described in any of the cited references. Table 1 gives soils characteristics, but the single value given for each character is not related to any horizon, when all those characters are likely*

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to change with depth! Moreover, there is a lack of consistency with previously published data: the ZAR-01 soil is given as the same as described in Quesada et al., 2011, where the Bh begins at 90 cm in depth with a C content around 20% Here the Bh was sampled at 70 cm in depth with a C content around 17% This indicates soil spatial variability and has to be discussed.

Response: The reviewer is right in that we gave insufficient description of the soil profiles. In Table 1 we presented soil descriptions as in Quesada et al. (2011). However, the description in that paper only pertains to the topsoil and not the entire profile. To address this issue we incorporated a detailed profile description by horizon provided by Carlos A. Quesada, who is now coauthor of the mansucript. In the new Table 2, we provide detailed profile data by horizon, including texture, element concentrations, and pH.

In fact there is spatial variability in terms of the carbon contents. However, we believe the 3% difference in C content between our measurements and the measurements presented in Quesada et al. (2011) are not very large given the large differences that can be observed in soils in terms of C content. For example, Schrumpf et al. (2011) report coefficients of variation between 21 and 49% in forest soils. The difference in C contents in our samples compared to those reported in Quesada et al. (2001) is only 15%, smaller than reported differences for other sites.

- *The position of the studied soil within the landscape and its hydric regime throughout the year has also to be explained: in hydromorphic podzols as those described by Montes et al. (2011) the Bh is during the whole year beneath a perched water-table, under reductive conditions proper to OM (organic matter) conservation, and these authors hypothesized that a fluctuating water table would induce oxygenation of the Bh during part of the year and a subsequent mineralization of the Bh more labile OM. Such conditions are more easily found*

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in shallow podzols, as the one studied here, and when the podzol is situated near a landscape incision that favors water-table lowering. All these points have to be considered and discussed.

Response: This is a very good point and we acknowledge that information about the temporal behavior of the water table would greatly improve our analysis. Unfortunately, we do not have information about the annual dynamics of the hydrological regime in the soils we studied.

According to our own personal observations, the podzol we studied floods seasonally (water levels 5-10 cm aboveground). We do not have measurement on the water table during the rest of the year. However, at the time of sampling, during the dry season, the profile was watterlogged, which suggests that the Bh horizon may be under anoxic conditions for most, if not all, of the year.

It is important to point out that this site is located in a very remote area, 5 hours away from the closest road where access of research equipment and regular monitoring is highly restricted by logistics. Our lack of measurements of the seasonal dynamics of the hydrologic conditions is therefore restricted by logistics and appropriate budget for such measurements.

- *The number of samples and the samples location are questionable. The topsoil was sampled using 5 random points located inside permanent plots, when deeper horizons at 10, 40 and 70 or 55 cm in depth were sampled outside the plots, so that there were no direct genetic relationships between the sampled topsoil horizons and the sampled deeper horizons: vertical transfers cannot be studied between horizons that are not situated on the same vertical! This would not be a problem if the number of samples would permit to statistically validate the extrapolation of the out-plot data to the in-plot soil, or vice-versa. Unfortunately, the number of samples outside the plots is not given in the manuscript: reading lines 167 to 175, it can be understood that a single point was sampled,* *but statistical data given in the Results section let suppose that at least 5 points (a minimum to define quartiles!) were sampled at each depth outside the plots. No details, however, are given about distances between repetitions or distance between the in-plot samples and the out-plot samples.*

Response: There is a confusion here about the number of samples and our estimations of vertical carbon transfers. We sampled at 5 random points within the plots only to address Hypothesis 1, which is concerned about C cycling in the topsoil. In the assessment of Hypothesis 2, where we estimate vertical C transfers, we only used information from the profiles established outside the plots. So, we did not combine information from the two types of sampling to estimate the vertical C transfers.

However, we only used one single profile per forest type to assess Hypothesis 2 and calculate vertical C transfers. The reason for only sampling one profile per forest type was due to cost limitations of the radiocarbon analyses. In addition, we also had logistical limitations in the field, where we had large difficulties reaching the Bh horizon due the continuous water saturation of our profile.

To address these issues in the manuscript, we added a better description of sample sizes and the information used to inform the model and calculate vertical C transfers.

- *Some fundamentals of the model are questionable. The model supposes vertical transfers in each soil type. In equatorial podzols, however, a significant part of the DOC produced in the topsoil is transferred to the rivers by lateral flow of the water-table perched over the Bh (see for example Chauvel et al., 1987, Experientia 43: 234-241 or Lucas, 2001, Ann. Rev. Earth Planet. Sci. 29: 135-163). According to Montes et al. (2011), in the high Rio Negro area 70% of the water percolating through the topsoil is transferred to the rivers before reaching the Bh. Taking in account such process would need a sink term in the (1) and (2) sets of equations.*

Moreover, it is difficult to understand the model because the k_1 to k_4 constants are not described in the text. If they are decay constants by respiration, I do not understand why the DOC transferred in depth is not removed in equations describing C dynamics in the topsoil. For example, taking in account the DOC transferred to the Bh and the DOC removes laterally, the first equation of the set (1) would be:

$$\frac{dC_{ft}}{dt} = \gamma I - (k_1 + \alpha_{3,1} + \alpha_{5,1}) \quad (1)$$

Where the $\alpha_{5,1}$ transfer coefficient represents the proportion of fast decomposing carbon that moves outside the system by lateral water flow. If so, the results given by the model are highly questionable. If the k_1 to k_4 constants are not decay constants by respiration but represent the sum of respiration removal and removal by transfer, the conclusion given in line 313 makes no sense: considering a steady state, it would mean that only 10% of the fast carbon input in the topsoil horizons is removed by respiration and 90% is transferred in depth! As described here, the model also assumes that the carbon input by fine roots is negligible in the deep horizon. Is such an assumption true, particularly for the alisol at 55 cm in depth? This point must also be addressed.

Response: We acknowledge our model was poorly described in the previous version of the manuscript, leading the reviewer to some confusion about the meaning of the different terms. We included a new figure graphically representing the conceptual model. We also added a better description in the text.

The main point is that the decomposition of organic matter from the pools, mathematically described as the product between the decay rate k and the amount of carbon in the pool C, does not necessarily represent respiration losses. This product is conceptually conceived as the amount of organic matter consumed by microorganisms and leaving the pool either as respiration, DOC horizontally

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transferred, or getting transferred vertically to the pools in the subsoil. The coefficients $\alpha_{j,i}$ only represent the proportion of the total amount of decomposed C that is transferred vertically. The proportion of C that is respired as well as the amount of C that is transported horizontally is therefore equal to $1 - \alpha_{i,j}$. We do not attempt here to separate the amount of carbon that is respired from the amount of C that is transported horizontally because we do not have enough data to constraint additional parameters for the model.

So, the assumption we made in the model classify under the second alternative suggested by the reviewer: '*[k_1 to k_4] . . . represent the sum of respiration removal and removal by transfer*'. According to our previous version of the model, this would imply that 90% of the decomposition from the fast pool of the topsoil in the Alisol is transferred vertically, which may seem unreasonable as suggested by the reviewer. We agree that this proportion is relatively high and acknowledge that we are ignoring root inputs along the vertical profile.

To address this issue we reformulated our model and included vertical inputs of carbon from fine roots, but only for the simulation in the alisol. In the podzol, fine roots are concentrated in the topsoil and do not penetrate deep into the profile due to the perched water table. With this reformulation of the model we find a smaller proportion of vertical C transfers in the alisol (30%), and found that vertical inputs from roots contribute an important portion of the radiocarbon inputs along the soil profile.

- *A last point: I lacked time to study the radiocarbon model, but I was questioned after a look at the equations by the following: (1) why Fa is the fraction of radiocarbon in atmospheric CO2 and not the fraction of radiocarbon in modern vegetation? (2) F cannot be the same at each depth, it has to be considered Fft , Fst , Ffs , Fss.*

Response: Conceptually, the radiocarbon model is identical to the soil organic

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matter model, except that radioactive decay is accounted for. (1) F_a is the fraction of atmospheric CO_2 because this is the quantity that we know thanks to long-term monitoring of atmospheric radiocarbon concentrations that are reported in standard agreed-upon datasets. We make the assumption, as in most previously published analysis, that the inputs of radiocarbon from litter inputs have the same radiocarbon signature as the atmosphere. In other words, we assume there is a negligible time-lag that radiocarbon spends in vegetation before it enters the soil. We believe this assumption is valid for our purposes because we are interested in decade- and century-scale differences in C cycling. Differences in radiocarbon signatures of a few years make little difference when we try to follow bomb radiocarbon in a large pool such as the Bh horizon that contains very low radiocarbon values.

As for point (2), the reviewer is correct in that the fractions are not the same for the different depths and pools. This was a mistake of notation and was corrected in the text.

- *Consistency of the discussion. In the eventuality of new modeling, the discussion will obviously need to be rewritten. A main point, anyway, must be pointed out as important to be discussed. The authors guess that the podzol they studied is not actually a true podzol (line 378) and that its genesis differs considerably from previously studied podzols in the central Amazon basin (lines 390-393). It is difficult to evaluate the validity of these assumptions without a proper description of the podzol, of its moisture regime and of the associated landscape. It must be considered that the assumption made in Montes et al. (2011) are only valid for hydromorphic podzol where the Bh horizon is never under oxic condition, which does not seem to be the case of the podzol here studied.*

Response: We did change some of the model assumptions and obtained some results that lead us to a somewhat different discussion. However, the general results did not drastically change and we still predict low vertical transfer rates for the podzol soil. Our field evidence indicates that this podzol is also continuously water-logged during the year (or at least for most of it), similarly to what is assumed by Montes et al. (2011).

The point that we want to emphasize is that our radiocarbon approach, combined with a simulation model, lead us to very different transfer rates than those obtained by Montes et al. (2011) who based their calculations on DOC concentrations and soil water flow. Even without the use of a model, the lack of modern carbon in the Bh horizon of the podzol is a strong indicator of very low vertical C transfers in this podzol.

Comparing the podzol we studied and those reported in Montes et al. (2011), there seems to be important similarities with the bad-drained podzols of that study and the profile we describe. They both are characterized by a shallow A horizon followed by a bleached E horizon with very low C contents. The underlying Bh horizon is hardened an impedes the vertical flow of water. Its C content is high and contains more clay than the horizons above. One main difference is that the podzol we studied is shallower than those described by Montes et al. (2011). We found the Bh horizon at 75-80 cm depth (Quesada et al. (2011) found it at 90 cm depth), but Montes et al. (2011) found the Bh below 135-165 cm depth. Despite this difference, there seems to be important similarities between these soils.

- *The 1.5 10^6 km^2 given for the poorly-drained podzol area in the Amazon (line 64) is much overestimated. Lucas et al. (2012) estimated from RadamBrasil data that 18% of the Amazon are covered by podzol-ferralsol systems, which doesn't mean poorly-drained podzols, and Montes et al. (2011) estimated (after Bernoux et al., 2002 and Batjes and Dijkshoorn, 1999) the poorly-drained podzol surface in Amazonia to be more than 1.4 10^5 km^2 only, this values matching the one given in Quesada et al., 2011. The value of 1,554,10^5 km^2 given in Montes et al. (2011) is certainly a typeset error and is certainly 155,410 km^2, this value matching the* *calculated carbon stock.*

Response: We took this estimate directly from Montes et al. (2011), which on page 118 reports 1,554,105 km^2, or 1.5×10^6 km^2 as we originally reported. The reviewer is correct in that the estimate is an order of magnitude larger than what has been reported somewhere else. We agree that there might be a typo in the Montes et al. (2011) paper and we do not want to propagate this error, so we report now 1.5×10^5 km^2.

- *Line 212-213: what are decomposition rates that transfer carbon along the depth profile? Single transfert rate, mineralization rate, the sum of mineralization and transfer? Explain.*

  This was not an accurate description of the model and the decomposition rates. We rewrote the model description part to make it more clear.

- *Lines 218 to 235: the k1 to k4 decay constants were not defined in the text. How was defined I, the carbon inputs to the topsoil?*

  Idem

- *Lines 245-250: no difference between ZAR-01 O and ZAR-04 0 but difference between ZAR-04 O and ZAR-04 M is hard to believe. As 5 samples were taken, all values are given by min, 1st quartile, mediane, 3rd quartile and max in Fig. 1. These values estimated from Fig. 1 show statistical difference between ZAR-01 O and ZAR-04 0 and no statistical difference between ZAR-04 O and ZAR-04 M, the opposite to what is argued in the text.*

Response: We tested for significant differences using a linear model of the form $y_i = \beta_0 + \beta_1 Plot + \beta_2 Horizon + \beta_3 Plot * Horizon + \epsilon_i$. The result of the analysis

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of variance was:

```
Analysis of Variance Table

Response: C
             Df  Sum Sq Mean Sq F value    Pr(>F)
Plot          1  205.58  205.58  7.7944   0.01753 *
Horizon       1 1243.52 1243.52 47.1483 2.705e-05 ***
Plot:Horizon  1   45.56   45.56  1.7273   0.21550
Residuals    11  290.12   26.37
---
Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

The post-hoc Tukey HSD test gives the following results:

```
  Tukey multiple comparisons of means
    95% family-wise confidence level

Fit: aov(formula = C ~ Plot * Horizon, data = Plot)

$Plot
                   diff       lwr       upr     p adj
ZAR-04-ZAR-01 -7.420571 -13.27066 -1.570481 0.0175285

$Horizon
        diff      lwr      upr    p adj
O-M 18.20403 12.35394 24.05412 2.77e-05

$`Plot:Horizon`
                       diff        lwr       upr     p adj
ZAR-04:M-ZAR-01:M -2.330833 -14.135493  9.473826 0.9317294
```


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```
ZAR-01:O-ZAR-01:M 22.080167  10.275507 33.884826 0.0007516
ZAR-04:O-ZAR-01:M 12.724167   0.919507 24.528826 0.0338112
ZAR-01:O-ZAR-04:M 24.411000  13.482009 35.339991 0.0001638
ZAR-04:O-ZAR-04:M 15.055000   4.126009 25.983991 0.0075470
ZAR-04:O-ZAR-01:O -9.356000 -20.284991  1.572991 0.1016870
```

These statistical analyses show that there is a small (74.2 mg g^{-1}), but significant difference between the plots at $\alpha = 0.05$ in terms of their carbon content. However, this difference among the plots is mostly driven by different across horizons. The Tukey HSD test shows that comparing only the horizons, there are no statistical differences between the two mineral horizons (p.adj = 0.93) or between the two organic horizons (p.adj = 0.10). In other words, there were only significant differences between the O and M horizons of ZAR-01 as well as between the O and M horizons of ZAR-04.

We included in the text additional information about the statistical analysis. The R code we provide in the supplementary material can also be used to confirm these results.

- *Lines 275 to 285 and Fig. 2b: the heterotrophic respiration unit (mg C gdw-1 day-1) is not explained, is it mgC by gram soil dry weight by day? It can be inferred from lines 275 and 280 that the values given in Fig. 2 are the total amount of carbon respired during the incubation period. Giving respiration in mgC gC-1 day-1 would be more relevant and would show that the respiration rate of the Bh carbon is much lower than these of the deep horizons of the alisol.*

  We decided to remove Figure 2 because the information is better portrayed in tabular form. We changed the units of Rh to mg C mg C^{-1} day^{-1}.

- *Fig. 2 can be improved and greatly reduced. Define what is gdw in the legend. Fig. 3 can be reduced.*

We removed Figure 2 and present now the information in tabular form. We kept Figure 3 as previously presented because we think it gives important information about the radiocarbon analysis.

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