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

### **Anonymous Referee #1**

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Review about the following manuscript: Journal: BG Title: Low vertical transfer rates of carbon inferred from radiocarbon analysis in an Amazon podzol Author(s): C.A. Sierra et al. MS No.: bg-2013-25 MS Type: Research Article

### 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 knowledge 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

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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.

### 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 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. 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 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.

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

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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.

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:

see equation as fig. 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

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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. 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  $F_a$  is the fraction of radiocarbon in atmospheric  $\text{CO}_2$  and not the fraction of radiocarbon in modern vegetation? (2)  $F$  cannot be the same at each depth, it has to be considered  $F_{ft}$ ,  $F_{st}$ ,  $F_{fs}$ ,  $F_{ss}$ .

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.

Other imprecisions, errors or inconsistencies in the manuscript. The 1.5 106 km<sup>2</sup> 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 1.4 105 km<sup>2</sup> only, this values matching the one given in Quesada et al., 2011. The value of 1,554,105 km<sup>2</sup> given in Montes et al. (2011) is certainly a typeset error and is certainly 155,410 km<sup>2</sup>, this

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value matching the calculated carbon stock. Line 212-213: what are decomposition rates that transfer carbon along the depth profile? Single transfer rate, mineralization rate, the sum of mineralization and transfer? Explain. Lines 218 to 235: the  $k_1$  to  $k_4$  decay constants were not defined in the text. How was defined  $I$ , the carbon inputs to the topsoil? 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, median, 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. Lines 275 to 285 and Fig. 2b: the heterotrophic respiration unit ( $\text{mg C gdw}^{-1} \text{ day}^{-1}$ ) is not explained, is it  $\text{mgC}$  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  $\text{mgC gC}^{-1} \text{ day}^{-1}$  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.

#### Technical corrections

Fig. 2 can be improved and greatly reduced. Define what is  $\text{gdw}$  in the legend. Fig. 3 can be reduced.

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

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