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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.

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We thank the reviewer for considering the work interesting and important, and for providing critical comments to the manuscript. We provide here answer to all comments, with the original reviewer’s comment on italics.

General comments

The subject of this paper is interesting and important. Nevertheless, the given conclusions are no sufficiently sustained by the given experimental data. Two soil profiles only were studied, and there is a need for more information about the studied soils.

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The authors referred to previous works for a detailed characterization of the studied soils, but data as soil morphology, horizon thickness, granulometry and mineralogy are not given in the cited works, when such data are necessary to interpret the carbon dynamics within the soil. Moreover, the conclusions are supported by results obtained from only 2 profiles, but profiles where topsoil and deeper horizons are disconnected. That is why, although the work described here is potentially very interesting, I think it can hardly be published in Biogeosciences because of the lack of data to support the conclusions.

Response: In the revised version of the manuscript we provide new supporting data to interpret the results of our radiocarbon analysis. We included profile descriptions by depth that includes: texture, bulk density, element concentrations (including Al contents) as well as pH (see new Table 2).

Our analysis of vertical C transfers is supported only on information from two profiles, each from the two different plots. We had logistic and budget constraints for sampling more profiles, especially for radiocarbon analyses. However, we believe the data and the analysis are interesting enough to deserve publication. First, to our knowledge, this is the first time that radiocarbon measurements are reported for a profile in Amazon podzols. Only one single radiocarbon measurement has been reported before for the Bh horizon of an Amazon podzol (Horbe et al. 2004), but this radiocarbon age is difficult to interpret given that it lacks an appropriate modeling framework for interpretation. Second, we believe that our finding of no modern radiocarbon in the Bh horizon of the podzol is important. Third, the lack of replication only limits the scope of inference of the results but not their validity. With one single profile we cannot make inferences about all podzols in the amazon or in the western part of the basin, which is not our objective. Our scope of inference is limited to the profiles we studied, but we believe that this is interesting enough given previous generalizations of C transfers for all podzols in the Amazon. With our data we can say that at least, the large vertical C transfers reported

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previously for all podzols in the basin do not apply to the profile we studied.

Specific comments

1. *The authors studied carbon vertical transfers but sampled the upper and lower horizons in separate profiles. It is well known that, even in small areas, horizon thickness and characteristics can be highly variable, which would alter the results. Did the authors checked the continuity and the lateral homogeneity of the horizons? What was the distances between the samples upper and lower horizons? How is the landscape morphology in the studied areas? Why the topsoil horizons were not sampled in the same place than the deeper horizons, i.e. outside the permanent plots? From the description given in the "Study sites and sample collection" section it is unclear how many samples were collected. From the text it can be understand that, for each soil type, 5 samples were collected from the topsoil and 1 sample for each of the three deep horizons. In such a case, how was possible a statistical analyse? If more samples were collected, it has to be explained and results must be given.*

Response: There seems to be confusion about our sample design and the assessment of different hypotheses in our study. First, we did not use the topsoil samples collected from the 5 random points inside the plots to estimate vertical C transfers. We only used the profile data for informing the model and calculate vertical C transfers. More specifically, we only used the data inside the plots to assess our hypothesis 1, and used the profile data separately to assess hypothesis 2. For this reason we did not checked the lateral homogeneity of the horizons as pointed out by the reviewer.

Second, statistical analyses were performed only on the random samples from the topsoil to assess hypothesis 1. We did not performed statistical analyses with

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the profile samples. We only used the information from the profile samples to inform a simulation model. Error bars presented in Figure 3 for the radiocarbon values are only analytical error, i.e. the error of the radiocarbon determination for each sample.

Third, we sampled the profiles outside the permanent plots to minimize disturbances to the ongoing vegetation monitoring of decade-scale C dynamics. However, the profiles were located just a few meters from the plot edge and they should represent well the soil characteristics inside the vegetation plots.

We recognized that in the previous version of the manuscript the description of sample sizes was ambiguous. In the revised version we provide a better description of sample collection, size, and data analysis to clarify issues pointed out in the review processes.

2. *The Table 1 gives granulometrical data for both profiles, but a single average value is given for each profile. What is the meaning of such a value? It is necessary to give the values for each horizon.*

Response: The soil description data in Table 1 was obtained from Quesada et al. (2011), where only information on the topsoil was available. We obtained detailed soil description data from C.A. Quesada, who sampled in the same sites. In the new Table 2 we provide detailed granulometric data by horizon as requested by the reviewer.

3. *One of the authors' conclusions is that the carbon transfer rate from the topsoil to depth in the podzol is 8-fold lower than in previous studies. At the same time, the authors question the fact that the soil they studied is a true podzol. Moreover, the fact that the studied podzol is seasonally water-logged implies um behavior different with regard to a soil that is water-logged all along the year. To support their conclusion, the authors would need results from other types of podzol profile, particularly permanently water-logged podzol.*

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Response: We are not trying to make any extrapolations with our data to other podzols. We simply are trying to say that our estimate, based on a very different method, gives vertical C transfers 8 times lower than what has been reported previously for permanently waterlogged podzols in the entire Amazon basin. We do not think that, within the scope of this local study, we need to sample other types of podzol in the Amazon. We are only interested in reporting our observations for this specific site and suggest that previous estimations in other podzols may not be comparable at other sites.

We do not have continuous measurements of the water-table level in the podzol we studied, and that makes it difficult to compare our results from those from Montes et al. (2011). However, we believe that the soil we sampled is also waterlogged for the entire year. Our own personal observations suggest that the site gets seasonally flooded (water level of 5-10 cm above ground level). At the time of sampling, during the dry season, the water table was above the Bh horizon. Although this observations are not conclusive, it is possible that the podzol is permanently flooded and therefore comparable with the podzols described by Montes et al. (2011).

4. *The authors assert that the respiration carbon at 70 cm at depth in the podzol is higher than the respiration carbon at 55 cm at depth in the alisol. Is this result supported by any statistics?*

Response: No, this result is not based on any statistical analysis. We did not have replicates of the profile samples, and this observation of larger respiration values for the Bh samples cannot be confirmed with a statistical test.

5. *The fact that no post-bomb carbon were incorporated in the podzol Bh indicates that this Bh is no more active. In such a case, the comparison with real hydro-morphic, active podzol does not make sense.*

Response: This is the main message we are trying to convey; that this soil, previ-

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ously classified as podzol, has very little incorporation of post-bomb radiocarbon. For this reason, we hypothesize that the soil is actually not a true podzol or that it is in a late stage of development where podzolization has already stopped.

It is possible that the comparison with other podzols is unfair. However, Montes et al. (2011) report vertical C transfer rates for all podzols in the Amazon, which may include soils like the ones we studied. For this reason, we would like to report with our study that vertical C transfer may differ considerably with other soils that are thought to be podzols as well.

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