

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 #3

Received and published: 4 April 2013

General comments The subject of this paper is interesting and important. Nevertheless, the given conclusions are not 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. 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.

C782

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 check the continuity and the lateral homogeneity of the horizons? What was the distance 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 understood 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 analysis? If more samples were collected, it has to be explained and results must be given. 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. 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 a 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. 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? 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 hydromorphic, active podzol does not make sense.

Interactive comment on Biogeosciences Discuss., 10, 3341, 2013.

C783