Biogeosciences Discuss., 10, C1592–C1602, 2013
www.biogeosciences-discuss.net/10/C1592/2013/

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.

C. A. Sierra et al.

csierra@bgc-jena.mpg.de

Received and published: 6 May 2013

Offline, we received additional comments by an anonymous reviewer. Below we show his/her comments in italics and answers in normal font.

General

The authors' initial assumption indicates that they are not well-versed in podzol lore. Mean radiocarbon ages (better: Mean Residence Times) of B horizons in tropical hydromorphic podzols (see e.g., Buurman & Jongmans, Geoderma 125 (2005):71-83) are in excess 10,000 years. This excludes the possibility of significant short-term re-

placement. Radiocarbon 'ages', however, refer to precipitated carbon. As long as the soil remains porous, there is no reason why DOC should not pass through the B horizon – or over the B-horizon without being retained. The fact that much DOC is NOT retained by hydromorphic podzols in Amazonia is illustrated by the high DOC content in the black rivers. Obviously, huge amounts of DOC pass through Amazonian podzols without being retained and without replacing previously precipitated SOM. The authors' misconceptions regarding these aspects invalidate their paper.

Response: There are two important points that require clarification: 1) mean residence times (MRTs) do not correspond to Δ^{14}C values, conventional radiocarbon ages, or calibrated ages. 2) We worked on a hydromorphic podzol that remains continually waterlogged. We will elaborate on these two points below.

The reviewer assumes that conventional radiocarbon ages, as reported by Buurman & Jongmans (2005), are analogous to MRTs. First, it has been shown that radiocarbon concentrations in the atmosphere have changed considerably in the past and that conventional radiocarbon ages need to be calibrated against a standard calibration curve (Reimer et al. 2009). A conventional radiocarbon age can be far from the more accurate calibrated age. Second, as opposed to closed systems such as tree-rings or bones, soil organic matter is an open system that continually incorporates and losses ^{14}C. To estimate MRTs in soil organic matter it is necessary to use a model to account for these inputs and losses as well as internal transfers within the system (Torn et al. 2009). Third, even for systems in which the rates of losses are much slower than the rates of inputs (e.g. boreal peatlands), it is possible to measure the incorporation of modern radiocarbon produced by nuclear-weapon-testing during the 1950s (Trumbore 2000, 2009, Dioumaeva et al. 2002, O'Donnell et al. 2011). Our assumption for this study was that if the podzols are accumulating significant amounts of C in the Bh horizon we would be able to observe modern radiocarbon from the bomb-spike. We believe our assumption is more rigorous than simply assuming that a conventional radiocarbon

BGD
10, C1592–C1602, 2013

age provides the MRTs of the soil organic matter in the Bh horizon.

Concerning point 2 above, we would like to emphasize that we worked on a podzol that is continuously waterlogged due to the impermeable Bh horizon. For this reason DOC may not be incorporated in the Bh and therefore it may be transported laterally. This is consistent with the idea that black-water rivers in the Amazon receive most of the DOC from landscapes dominated by podzols. Therefore, there is no contradiction with what is suggested by the reviewer.

Specific comments

- *There is insufficient information on the soil investigated by the authors. Details of morphology, classification and general properties (C content, pyrophosphate-extractable Fe and Al, pH) should be reported.*
  We introduced a new table including information by horizon on texture, Al, pH, among other variables.
- *The authors systematically write elluviaton instead of eluviations.*
  Thanks. We made changes accordingly.
- *Line 9: Sesquioxides of Fe tend to be absent in hydromorphic podzols.*
  Not necessarily. do Nascimento et al. (2004) found increased amounts of Fe oxides in the Bh horizons compared to upper horizons in Amazon podzols, although Al oxides were much larger.
- *Line 12: The authors do not provide evidence or a large production of fine roots. Actually, B horizons of hydromorphic podzols are not generally rooted. It is possible that there are large quantities of fine roots in the A horizon but suc information is not presented.*
  Fine root production in the first 20 cm is twice as large in the podzol than in the

BGD
10, C1592–C1602, 2013

alisol (Table 1). We present now a more detailed description of the profiles. Fine root biomass was large in the A horizon of the podzol, but absent in the E and Bh horizons.

- *Line 13: The amount of metal ions in the B horizons of hydromorphic podzols tends to be very low, and these metals are already saturated with SOM. It is unlikely that there is any excess capacity to bind SOM. The authors' statement is therefore an incorrect assumption.*
  According to Montes et al. (2011) podzols store large quantities of C in their Bh horizon and are characterized by significant vertical C transfers. Our initial assumption is that these podzols may act as significant C sinks given these preliminary studies. In fact, metal ions in the Bh may be already saturated as indicated by the reviewer, but it is also possible that translocation and removal of metals from upper horizons contribute to the capacity of these soils to stabilize OM in the Bh (do Nascimento et al. 2004).

- *Line 20. This statement can only be valid for the upper 5 cm of the soil.*
  Yes, it is only valid for the first 5 cm and we present it as such.

- *Line 31. The authors apply a simplification that is incorrect. In the subsoil of the podzol, most carbon will arrive as DOC. As this DOC is not retained, the value of 1.7% transfer is incorrect. In the Alisol, on the other hand, most carbon in the subsoil is not provided by mobile DOC but by local decomposition of roots. Unless the authors have measured DOC transport the Alisol, their assumption is unlikely.*
  In the previous version of our model we ignored fine-root inputs in the alisol and incorrectly assumed that the only source of C transfers was vertical DOC movement. This is only true for the podzol where fine roots are only present in the A horizon. We made changes in our model to account for fine-root inputs in the subsoil of the alisol. With this change we obtained different values for the calculated

BGD
10, C1592–C1602, 2013

vertical transfers in the alisol, instead of 90% transfers from the decomposed C in the fast pool we obtained 30%. Results for the podzol did not changed.

- *Line 53: Nutrient-depleted and organic matter-rich are not mutually exclusive.*
  We changed the dichotomy to: nutrient-depleted/nutrient-rich.

- *Line 54-57: This sentence is unclear.*
  Here we refer to the idea that soils may control how much C plants invest in above- or below-ground structures. This is an old idea in ecology (e.g., Tilman 1985, Chapin et al. 1990) and we believe it does not require further explanation in this context.

- *Line 69: Much of the transport is lateral rather than vertical. This is usually made clear by the profile morphology, but the authors do not present that information.*
  We present now a better profile description (see section 2.1), however with the information we have it is not possible to calculate how much transport is lateral.

- *Line 71. There is newer literature available, and I do not think hat there is as much doubt as the authors suggest.*
  We would appreciate the reviewer to point us to more recent literature. On the topic of Amazon podzols and their genesis there is very little new, except do Nasciment et al. (2004, 2008), and the more recent papers by Montes et al. (2011) and Lucas et al. (2012).

- *Line 86. These appear to be data from elsewhere. Is there sufficient correspondence between the profiles to warrant extrapolation to the present system? Anyway, a plinthosol is very different from an Alisol so that the data cannot be use to compared the systems under study.*
  Here we are just giving an example between values of NPP of the podzol we studied and a forest growing on a plinthosol 50 km away. The purpose of presenting this information is just to set up our hypotheses.

BGD
10, C1592–C1602, 2013

- *Line 95-98. This suggests that either DOC was decomposed in the E horizon (which is not likely), or that significant DOC transport occurred laterally over the B horizon. Which applies to the present system?*
  The reviewer seem to be confused between the data we present in the manuscript and other studies that we cite to pose our hypotheses. In lines 95-98 we were presenting results obtained by Montes et al. (2011) on their estimation of vertical C transfers. These are not our data.

- *Line 101. I do not see how conclusions can be drawn about NPP.*
  This is a reference to one paragraph above this line, where we report that despite differences of 53% in aboveground NPP between a tall-stature and low-stature forest growing on a podzol, the difference in total NPP is only 16%.

- *Line 106. Presence of Fe is unlikely.*
  Please give a reference to support this statement. According to do Nascimento et al. (2004, 2008) the presence of Fe is likely.

- *Line 107. In hydromorphic podzols, belowground-NPP should be restricted to the A horizon (no roots in E and B horizons).*
  Yes and that was what we observed. We made this clear in the text and added that the high levels of belowground NPP are only for the topsoil (A horizon).

- *Line 108. Later instead of latter. Anyway, both the authors' results and MRTs from literature indicate that this does not happen.*
  Yes, but here we are only setting up our hypotheses. Our results helped us to reject the idea that vertical C transfers are being stabilized in the subsoil of the podzol.

- *Line 126. The hypothesis of higher fine root production does not appear to be supported by data.*
  Please see Table 1.

BGD
10, C1592–C1602, 2013

- *Line 137. This statement is not valid because the MRT of the SOM in the B horizon is very large.*
  In this type of systems there is not a direct relationship between MRTs and the Δ^{14}C value of carbon. If the OM in the Bh decomposes very slowly (high MRTs) the accumulation of new OM should contain the Δ^{14}C signature of the bomb-spike and should be detectable by AMS (Trumbore 2009). For example, in boreal peatlands where organic C has very high MRTs, it is possible to detect modern Δ^{14}C values (C fixed after 1950, see Dioumaeva et al. 2002, O'Donnell et al. 2011). Therefore, high MRTs in our Bh horizon does not imply that modern radiocarbon values cannot be detected.

- *Line 144. Ortstein Podzol is not a classification term in Soil Taxonomy or WRB. What is the classification according to these systems?*
  It should be Ortsteinic Podzol.

- *Line 154 Ortsteinc*
  It should be Ortsteinic. We changed it in the new version.

- *Line 275. To eliminate the influence of carbon content, respiration should always be expressed in C per C and NOT per dry (sample) weight. Significance of differences should be tested statistically.*
  Units are now presented in C per C per day. Significant differences were not tested statistically due to the lack of replication.

- *Line 281: see remark line 275.*
  Idem.

- *Line 299; bound, not bounded.*
  Bounded according to the New Oxford American Dictionary.

- *Line 297. It is likely that the respiration (in the Alisol sample) was largely due to recently dear roots. It need mot be fixed carbon.*

BGD
10, C1592–C1602, 2013

We sieved the soil to 2 mm and removed all big roots. In soils, the carbon released as CO_2 in incubations is always much younger than the carbon in the bulk soil (Trumbore 2000).

- *Line 312. It is more likely hat the SM was not due to transport, but to local death of roots.*
  The reviewer is right, and we made changes in our model to account for root inputs in the subsoil of the alisol.

- *Line 314-317. There is no reason to expect transfer of slow-pool C.*
  Although there is no reason to expect transfer of slow-pool C we included this parameter in the model to account for all possible sources of C transferred vertically. Indeed, the model results show that this transfer is insignificant.

- *Line 333. Again the value of root production, which is not substantiated in this paper.*
  The value of root production is presented in Table 1 and its source is Jimenez et al. (2009).

- *Line 355. Here, finally appears the possibility of lateral transfers (over the B horizon) in the podzol. Additionally, there is the possibility of transfer through the B horizon without retention.*
  In section 4.2 (paragraph 4) of the new version we expanded our discussion about lateral transfers.

- *Line 370. In the light of the present and other research, it is likely that the transport to the B horizon mentioned by Montes also refers to DOC hat is not retained in the B horizon.*
  In this regard, Montes et al. (2011) are not explicit on whether the vertically transferred DOC is retained in the Bh horizon. However, this is the most obvious interpretation of their study.

BGD
10, C1592–C1602, 2013

- *Line 375. As stated before, I do not think there is significant vertical transfer in the Alisol. The carbon in the mineral soil is most likely due to decomposing roots and NOT to DOC.*
  This was corrected in the new version of the model.

- *Line 379. Eluviation of clay is not a common process in podzolisation, although it may precede the process.*
  Changed to eluviation of metals.

- *Line 386. Klinge's conception is now outdated.*
  It is outdated, but worth mentioning since it could be a potential explanation for our data.

- *Line 390. If the authors have reason to believe this, they should be explicit. Again, a good description of the investigated profile is necessary.*
  We mention this just as a possible explanation, but have no evidence for any of the different possibilities we present. A better description of the profile is now presented in Table 3.

- *Line 398. Considering that accumulation of SOM in podzol B horizons is a gradual process, real 'ages' of such horizons should be at least twice as large. To compare this 14C age with those of the investigated profiles, morphology of the profiles should be compared. A less hydromorphic profile is expected to have lower MRTs.*
  Conventional radiocarbon ages of soil organic matter cannot be directly translated into MRTs or even real ages. For this reason we don't attempt to compare these values with our results. In the subsequent lines we provide the argument for why these ages are of little use, but we mention them in the manuscript because they are the only radiocarbon measurements reported in the literature for Amazon podzols.

BGD
10, C1592–C1602, 2013

- *Line 409-412. This is not supported by the data.*
  In these lines we state: "*Our modeling approach incorporated all these processes and more confidently can attribute the observed* $\Delta^{14}C$ *values to differences in the rates of organic matter decomposition and transfers along the profile*". Why this statement should be supported by the data? We are simply saying that to interpret our C14 data we used a model that incorporates radiocarbon dynamics and not only report conventional ages, which are known to provide misleading results.

- *Line 432, Incorporate*
  Changed.

- *Line 435. This statement should be reconsidered.*
  Our estimates of vertical C transfers are still 8 times lower than those reported by Montes et al. (2011) for all podzols in the Amazon basin. We do not see a reason to reconsider this statement.

References

P. Buurman and A. Jongmans. Podzolisation and soil organic matter dynamics. *Geoderma*, 125(1–2):71 – 83, 2005.

F. S. Chapin, E. Schulze, and H. A. Mooney. The ecology and economics of storage in plants. *Annual Review of Ecology and Systematics*, 21(1):423–447, 1990.

I. Dioumaeva, S. Trumbore, E. A. G. Schuur, M. L. Goulden, M. Litvak, and A. I. Hirsch. Decomposition of peat from upland boreal forest: Temperature dependence and sources of respired carbon. *J. Geophys. Res.*, 107(D3):8222, 2002.

E. M. Jimenez, F. H. Moreno, M. C. Penuela, S. Patino, and J. Lloyd. Fine root dynamics for forests on contrasting soils in the colombian amazon. *Biogeosciences*, 6(12):2809–2827, 2009. BG.

Y. Lucas, C. R. Montes, S. Mounier, M. Loustau Cazalet, D. Ishida, R. Achard, C. Garnier,

B. Coulomb, and A. J. Melfi. Biogeochemistry of an amazonian podzol-ferralsol soil system with white kaolin. *Biogeosciences*, 9(9):3705–3720, 2012.
C. R. Montes, Y. Lucas, O. J. R. Pereira, R. Achard, M. Grimaldi, and A. J. Melfi. Deep plant-derived carbon storage in amazonian podzols. *Biogeosciences*, 8(1):113–120, 2011.
N. Do Nascimento, G. Bueno, E. Fritsch, A. Herbillon, T. Allard, A. Melfi, R. Astolfo, H. Boucher, and Y. Li. Podzolization as a deferralitization process: a study of an acrisol–podzol sequence derived from palaeozoic sandstones in the northern upper amazon basin. *European Journal of Soil Science*, 55(3):523–538, 2004.
N. R. Do Nascimento, E. Fritsch, G. T. Bueno, M. Bardy, C. Grimaldi, and A. J. Melfi. Podzolization as a deferralitization process: dynamics and chemistry of ground and surface waters in an acrisol – podzol sequence of the upper amazon basin. *European Journal of Soil Science*, 59(5):911–924, 2008.
J. A. O'DONNELL, J. W. HARDEN, A. D. McGUIRE, M. Z. KANEVSKIY, M. T. JORGENSON, and X. XU. The effect of fire and permafrost interactions on soil carbon accumulation in an upland black spruce ecosystem of interior Alaska: implications for post-thaw carbon loss. *Global Change Biology*, 17(3):1461–1474, 2011.
P. Reimer, M. Baillie, E. Bard, A. Bayliss, J. Beck, P. Blackwell, C. Ramsey, C. Buck, G. Burr, R. Edwards, et al. IntCal09 and Marine09 radiocarbon age calibration curves, 0–50,000 years cal bp. *Radiocarbon*, 51(4):1111–1150, 2009.
D. Tilman. The resource-ratio hypothesis of plant succession. *The American Naturalist*, 125(6):827–852, 1985.
M. S. Torn, C. W. Swanston, C. Castanha, and S. E. Trumbore. Storage and turnover of organic matter in soil. In N. Senesi, B. Xing, and P. M. Huang, editors, *Biophysico-Chemical Processes Involving Natural Nonliving Organic Matter in Environmental Systems*, pages 219–272. John Wiley & Sons, Inc., 2009.
S. Trumbore. Age of soil organic matter and soil respiration: radiocarbon constraints on below-ground c dynamics. *Ecological Applications*, 10(2):399–411, 2000.
S. Trumbore. Radiocarbon and soil carbon dynamics. *Annual Review of Earth and Planetary Sciences*, 37(1):47–66, 2009.

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