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Interactive comment on “Deep plant-derived carbon storage in Amazonian podzols” by C. R. Montes et al.

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In general, I found this a very interesting study, clearly filling a large gap in carbon accounting. The manuscript is clearly written and does not suffer from major weaknesses. Yet, it is not really clear to me how the authors arrive at the extrapolation to the entire Amazone basin. It is clear that they have chosen a sufficient number of soil profiles in a relevant setting or toposequence, but as far as I can judge they merely average the rather variable concentrations and depths of the Bh horizons, where it can be argued that a kind of weighted average should have been selected, taking into account the proportions each of the different soil types take in such a toposequence. Perhaps, this is done, but at least it was not described. Page 7609 line 6: 'not in equilibrium with respect to soil chemistry' seems a rather vague statement? In a changing soil never

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anything is in equilibrium, so I don't catch the purpose here. Page 7613 line 14: should read figure 3 iso 2? Page 7614 line 21: see earlier remark on averages? Page 7616 line 8: yes, clearly due to the higher rainfall, but only at equal DOC concentrations? In reality, the difference is even bigger as DOC concentrations tend to be lower in boreal podzols? Page 7616 line 16 and further: specify that you refer to interflow or lateral flow? Page 7616 line 25 and further: indicate where you obtain the first order rates from and why you choose them?

In conclusion, I appreciate this paper a lot and it should be published, if possible enhanced by a more delicate way of extrapolating the results.

Interactive comment on Biogeosciences Discuss., 7, 7607, 2010.

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