

## ***Interactive comment on “Seasonal variations of belowground carbon transfer assessed by in situ <sup>13</sup>CO<sub>2</sub> pulse labelling of trees” by D. Epron et al.***

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Received and published: 4 April 2011

We are grateful to the deep critical lecture of our manuscript you did. As many other colleagues worldwide, we acknowledge the nice work of Peter Högberg group using the labelling approach in the field, even if pioneer work were in fact initiated by Mariah Carbone and Suzan Trumbore using <sup>14</sup>CO<sub>2</sub> as tracer instead of <sup>13</sup>CO<sub>2</sub>. The papers of both teams are cited in several places in the manuscript. The two main differences between these two pioneer works is (i) we labelled single trees using a crown chamber instead of several trees in a large chamber that includes the soil leading to some artefacts in the initial recovery of label in soil respiration (see the nice paper of Subke et al. 2009) (ii) the tree we labelled were 10m tall.

The main point you have addressed concerns the fact that we didn't discuss enough  
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the output of the multi pool model we fitted on the data. We agree that we should have cited the work of Hans Schnyder's groups. This is done now, both in the method section when we describe the model, and in the discussion. But it should be stated that their model was applied to grass growing in quartz sand in a growth cabinet, while we are studying a complex root-soil system of tall trees growing outside where climate fluctuates. We are aware that the model is based on an oversimplified description of the tree-soil complex system with only two pools, and for this reason, the model is clearly empirical. The rate constant we derived by fitting the model on the soil respiration data should be analysed with caution.

We made all the minor corrections suggested: 1. we replace “objectives” by “objectif” (and “were” by “was”) 2. we added the amount of <sup>13</sup>C recovered in soil CO<sub>2</sub> efflux in the abstract 3. there were already details about seasonal patterns in the abstract 4. we replace “telluric” by “soil” 5. I agree that rhizospheric is better than autotrophic. I used this word in a paper in 2001 but I felt a little bit alone using it since that time. We changed. 6. We agree that we cannot separate rhizosphere and mycorrhizosphere and that mycorrhizosphere is part of rhizosphere so we removed ‘mycorrhizosphere’ as suggested. 7. We didn't understand where is the problem with the sentence 8. We removed the “. . .” 9. We agree that the Högberg-2001 paper is a nice paper and we added it in the list. However, between you and me, except the fact that it was published in Nature, it didn't add much more compared to what was gained a decade before using root exclusion techniques like trenching. 10. We added the two suggested references (Mencuccini and Hollta 2010, Kuzyakov and Gavrichkova, 2010) 11. We agree that “at different times in the season” was vague and even unclear so we rewrote the end of this sentence (“at different seasons”). 12. We didn't cite Högberg et al 2008 paper in this sentence because this sentence is about the use of laser diode spectrometry, which was not used in the Högberg paper. But Högberg paper is cited elsewhere in the text 13. We prefer using tuneable (UK English) rather than tunable (US English) maybe because we are European. 14. “Transfer time” is used by Mencuccini and Hollta 2010 and in several other papers 15. We agree that label is recovered in leaves and

trunk before reaching the soil but we are addressing belowground compartment here 16. Yes, we did labelling in winter in Pine 17. We don't have any idea of the amount of root that was severed by trenching but the area delimited by the trenches was always higher than the mean area per tree (inverse of tree density). 18. We replace "permits to exclude" by "excludes" 19. We fully agree that saprophytic fungi will cross the mesh bag. But we agree that diffusion of soluble organic carbon or motile bacteria can do it so we changed the text accordingly 20. The experiment was setup in three sites that were already instrumented. This explains why different sensors were used for measuring soil water content in each site. 21. About section 2.2: Plain paper is now cited earlier in the section. We added information about polyane but we don't want to expand too much this section because it has been already described in detail in Plain et al. 2009, as mentioned by the referee. So, as suggested by the referee, we now refer more often to the Plain et al. 2009 paper. 22. The selection of the VPDB value is not trivial and this has been nicely discussed in Griffis et al. 2004 paper. We used the VPDB value reported by Coplen et al. 2002 (we now give this reference). 23. We correct 'roots' to 'root' 24. A 4 day delay before processing microbial biomass samples is not problematic if the samples are stored at 4°C. 25. We now define DM at first use in the text. 26. We fully agree that TDLAS is no more new and we removed the first sentence of the conclusion section. 27. The readability of the figures is OK on printed documents. We are not convinced that adding colour will improved but we will follow editor's advice provide there is no additional cost.

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Interactive comment on Biogeosciences Discuss., 8, 885, 2011.