

Interactive comment on “Seasonal variations of belowground carbon transfer assessed by in situ $^{13}\text{CO}_2$ pulse labelling of trees” by D. Epron et al.

Anonymous Referee #1

Received and published: 17 March 2011

This manuscript presents an extended tracer study of carbon isotope ($^{13}\text{C}/^{12}\text{C}$) transfer between plant canopies and the soils for three different tree species growing in France. The tracer technique was applied at different periods of the year to explore whether species allocate carbon belowground differently over the growing season. The experimental tracer technique coupled with fast response isotope measurements has facilitated the collection of an impressive dataset. This technique has been published now by the authors in two previously published manuscripts (Plain et al., 2009; Dannoura et al., 2011). Plain et al only presented trunk and soil CO_2 efflux and isotope data for one sampling campaign, whereas this paper presents multiple campaigns building a seasonal picture. This paper also presents data for different species, also presented in the Dannoura et al, 2011 paper, but instead this manuscript focuses on the soil and microbial respiration fluxes and isotope signals. On the whole the article is well written

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and the results interesting. The calculation of residence times is however unclear (or misleading) and should be clarified as explained below. After Eq 8 they write : “The kinetics of the label recovered in soil CO_2 efflux were described using a four-pool model that was fitted on the observed CLRFS values (Fig.1).” This is very misleading because never are the different residence time (or time constants) for the 4 different pools given. Instead, only k_{CB1} was estimated from their data which means that they have used a two-pool model comprised of a canopy pool, QC and a bulk soil+atmosphere pool, QA+B’ that includes (QA + QB1 + QB2). I think this whole section needs to be rewritten and the correct equations they are using should be clearly presented before the manuscript is accepted for publication. The parameters in the Tables should then have the same notations/names as in the equations.

Minor comments Section 1 pg5 line 16 This first hypothesis actually comes a little out of the blue. You do not set-up the background as to why you would expect differences in transport time on the basis of anatomy and or growth rhythm. Section 4.1 pg17 line 17 replace end of sentence to “compared to lower frequency isotope ratio measurements that do not gauge the peak precisely.” Section 4.1 pg 17 line 18 The authors state that “no other labeling experiments on tall trees were reported so far in natural conditions in the field” I disagree with this statement they should rewrite and cite the other studies such as Hogberg et al., 2008; Subke et al., 2009 that pioneered these experiments. Section 4.1 pg 17 lines 21-25 I think it would also be appropriate to point out at the end of this paragraph that “our time lags are however, consistent with those of Wingate et al., 2010 for Maritime pine that also varied over the growing season” Section 4.1 pg 18 lines 25 rewrite to “time lags related to transport in the soil air space are negligible” Section 4.1 pg 19 line 1 The authors should be a little more critical of their drought exclusion set-up. The evidence from the water potential measurements on the trees indicate that they did not impose any substantial drought on the study trees.