

## ***Interactive comment on* “Predicting decadal trends and transient responses of radiocarbon storage and fluxes in a temperate forest soil” by C. A. Sierra et al.**

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### **1 Response to general comments**

We appreciate the reviewer’s recognition of the difficulties of using this type of data for testing long-term effects of environmental change on soil carbon stocks. Despite the large variability we observed in the  $\Delta^{14}\text{C}$  values of soil  $\text{CO}_2$  efflux and heterotrophic respiration, the manipulation experiments showed significant differences in the age of carbon respired two months after the start of the experiment. We take this result as a good indication of important effects of N addition and warming on the short-term

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carbon balance of these soils. We believe it is important to test our model and different hypotheses with these data. Although the large variability we observed poses some limitations, we believe our results are valuable and add interesting insights into the response of soil carbon to environmental manipulations.

## 2 Response to specific comments

- Throughout the manuscript we use terminology standard in earth system science and reservoir theory. We define *flux* as the amount of matter transferred per unit time, and *rate* as the instantaneous fractional transfer in units of  $\text{time}^{-1}$ , also known as the inverse of the residence time (Eriksson, 1971; Olson, 1963; Jacobson et al., 2000). We recognize that the term rate is used differently in chemistry, but we believe that the usage in our manuscript should follow the terminology used in earth system science. Also, this terminology is commonly used in the literature of terrestrial decomposition of organic matter.
- Page 2199, line 18-21. We agree that changes in land-use also challenge the assumption of steady-state. We included this point in this paragraph as suggested by the referee.
- Page 2202, line 6. Yes, they are alternative hypotheses. We consider them because they have been widely discussed in the literature and we are interested in our analysis to find if there is evidence in our data to reject any of them.
- Page 2202, line 6-25. The hypotheses are difficult to present without a more formal description of the model, which we do in subsequent sections of the manuscript but would be awkward to introduce at this point. To help the reader, we introduced a sentence in which we point the reader to a more detailed (graphical) description of the hypotheses in a subsequent section. We also reworded

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- some sentences to address the referee's comments. In particular, we changed the word 'treatment' in hypothesis 1 to 'N and temperature manipulations'. In hypothesis 2, we changed Arrhenius 'kinetics' to Arrhenius 'equation'.
- Page 2204, line 16. The CO<sub>2</sub> is actually trapped on a molecular sieve inside a stainless steel trap. We corrected this sentence in the manuscript for clarity.
  - Page 2204, line 26. As noted by another reviewer also (Baisden), it is important to use the local radiocarbon signature of atmospheric CO<sub>2</sub> for running the model and for comparison with measurements. We use an atmospheric history derived from tree rings and our own measures of atmospheric  $\Delta^{14}\text{CO}_2$  made during the growing season. These have been published in Gaudinski et al. 2010, which is also referenced in the text). We have made this more explicit in the revised text (see also response to Baisden review)
  - Page 2206, line 12. The distinction should be total CO<sub>2</sub> efflux and heterotrophic respiration. We made this correction in the text.
  - Page 2209, line 15. You are right, we are not describing horizons here. We changed the term 'horizon' to 'fraction'. We made also this change in the caption of Figure 3. We also changed the verb 'fit' to 'agree'.
  - Page 2210, line 15. The amounts of C in this figure correspond to those presented in Figure 1 in the original model proposed by Gaudinski et al. (2000).
  - Section 3.4. We acknowledge that the variability in the observed data is large so model validation and hypothesis testing is challenging. However, despite the large variability of the observations we still found large and statistically significant differences as a consequence of the manipulations. We believe this result is important and worth an analysis. It also agrees with other studies that found unexpected results in <sup>14</sup>C of heterotrophically respired CO<sub>2</sub> after experimental

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- manipulation (we added a new reference to this study, which is now in press). The use of the model helped us in our case to reject the idea that this response can be caused by increases in decomposition rates alone.
- Page 2210, line 28. We chose the value of 1.5 because that was the increase in respiration rates observed in this manipulation experiment and reported in Conosta et al. (2011). This is clearly explained in this paragraph.
  - Page 2211. Figure 9 was originally cited in the discussion, because it was an idea presented a posteriori. Referee 1 suggested that we formally present this idea as one of the hypothesis, and Referee 3 has suggested using the GLUE methodology to address this issue. So, the original Fig. 9 was replaced in the revised manuscript with two new figures (9 and 10) that show the results of the GLUE analysis. The order for citing figures is now corrected.
  - Page 2213, line 6. Although Fig 3 shows a moderate fit of the data with model predictions, it shows that the model can predict the general trend of radiocarbon content in bulk soil. Fig 2 shows a better fit of the data with observations of radiocarbon in heterotrophic respiration. It is important to keep in mind that the model was not developed with any optimization procedure between data and observations. For this reason, we believe this empirically derived model provides a good predictive power.
  - Page 2215, lines 8-25. We have a different point of view on this point. It is true that the variability of the data is very large compared to the predictions of the different hypotheses. However, the idea of any hypothesis test is to find evidence to reject a hypothesis, and in this analysis we can confidently reject H3. This is an important result because it tell us that the labile pool cannot be the only one that respond to the manipulations as suggested by Melillo et al. (2002). In addition, none of the hypotheses provide strong support to the observed data, which also suggests that the observed pulse in decade-old radiocarbon was caused by a

- mechanism different than just an increase in decomposition rates alone. So, in this sense we can say we learn something from our hypothesis test.
- Page 2215-2216. We acknowledge that the idea of changing pool sizes is somewhat arbitrary and not based on rigorous physical and/or biological mechanisms. However, this is an intriguing idea that has been reported in the literature previously (see citations in the text). In our case, this is the only way we can explain our observations in the manipulation experiment and we think it is valuable to present this idea here. It is not possible at the moment to provide an explanation for this possible change in pool sizes, but by reporting the observation in this manuscript we hope it can be addressed and resolved in further studies.
  - Page 2217, line 9. The model was able to predict: 1) the decadal trend of  $^{14}\text{C}$  in soil  $\text{CO}_2$  efflux (Fig 2), 2) the decadal change of radiocarbon in organic and mineral fractions (Fig 3), and 3) the effects of warming and N additions on the radiocarbon of heterotrophically respired carbon in the organic fractions (Fig 8a). The model was not able to predict the effects of the manipulation in the mineral fraction only 2 months after the start of the experiment. For these reasons, we say the model was able to predict most of the observations.

### 3 References

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