

Interactive comment on “A dual isotope approach to isolate carbon pools of different turnover times” by M. S. Torn et al.

Anonymous Referee #1

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Numerous models are available to simulate storage and turnover of soil organic carbon. A major problem has been to relate conceptual pools (model) to something that can be measured in a soil sample (experiment). Density fractionation is one way to meaningfully do this by equating the labile/intermediate and the stable carbon pools with the light and heavy soil fraction, respectively. The present paper proposes a refinement to this approach by separating each of two density fractions into a slow and a fast pool. This is achieved by means of a dual isotope approach (^{13}C , ^{14}C), the applicability of which seems to be limited to fossil CO_2 enrichment experiments.

The paper is clearly written, provides some new insights to organic carbon cycling in annual grasslands in California and is a pleasure to read. Also, it leaves me with a question that I would like the authors to discuss more in depth in a revised version.

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How much do the results of the experimental part of the study depend on the assumption that soil organic carbon pools are in a steady state (Equation 4 and related text)? Concentrations of CO₂ were almost doubled in the labelled plots (to > 700 ppm). Even a much smaller enhancement of CO₂ concentrations (to 475 ppm) can already substantially affect the size of carbon pools and respiration (Ross et al., Soil Biol. Biochem. 58, 265-274, 2013). I am wondering, to what extent does the better performance of the two-pool model in simulating similar turnover-times in control and elevated CO₂ treatments result from its ability to better compensate a possible discrepancy between the steady state assumption in the model and a non-steady state behaviour in the field? Could there be a circularity between assumption and result? Please discuss.

Interactive comment on Biogeosciences Discuss., 10, 10189, 2013.

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