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Interactive comment on "Evaluating the simulated mean soil carbon transit times by Earth system models using observations" *by* Jing Wang et al.

Anonymous Referee #2

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In this manuscript, the authors present observation-based estimates of transit times of carbon in soils, and compare these estimates with model predictions. This is an important topic because transit times are a very good constraint for evaluating model performance. There has been a lot of recent research on this topic, motivated in part by the work of Carvalhais et al. (2014), who used an *stock-over-flux* approach to compute residence times from models and observations. Recent publications have shown that this approach has problems to compute transit times for systems of multiple pools and out of equilibrium (Lu et al., 2018; Sierra et al., 2017), and better methods for estimating transit times for systems out of equilibrium have been developed (e.g. Rasmussen et al., 2016).

Despite these recent developments, this manuscript uses observations from incubation

experiments and δ^{13}C measurements from C_3/C_4 vegetation replacement experiments, in which the rate of soil carbon loss is estimated assuming one single pool in equilibrium. This is evidenced by equations (1) to (3) in Text S1 of the supplementary material. The implication of this assumption is that the observations are treated as a homogeneous system, without differentiating between the age of the stored carbon and the age of the carbon in the output flux. In the introductory paragraphs, the manuscript gives the impression that it provides an advance by providing observation estimates of transit times, but in reality these estimates suffer the same problems of previous approaches.

I recommend the authors to use the data they compiled to fit multiple-pool models to better estimate age and transit times from these observations. You probably would still need to keep the steady-state assumption for this type of observations, but at least you can remove the one-single homogeneous pool assumption. For a fair comparison with the model output, I recommend you compute their transit time at the spin-up state, which better represent the equilibrium state of the model. In the current version, you compute model-derived transit times from a multi-year average, but this corresponds to a transient state where transit times are not unique.

Another aspect that requires clarification is the computation of the transit time distributions in Figure 1b. How were these distributions obtained from the data? Did you assume a specific distribution function and fitted its parameter values using the data? This seems to be the case for the δ^{13} and the stock/flux data, but not for the incubations. Please clarify.

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