

Interactive comment on “How to link soil C pools with CO₂ fluxes?” by Y. Kuzyakov

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The manuscript by Kuzyakov addresses a critical issue in the advancement of our knowledge of soil organic matter (SOM) dynamics. For several decades now, there has been recognition that SOM is a highly heterogeneous substance, and one response to this has been the development of multi-compartmental models to simulate SOM dynamics. More recent interest has been to associate the conceptual model pools to measurable constituents. In the current manuscript, Kuzyakov describes how various experimental designs can be used to deconvolute CO₂ fluxes from various SOM sources. This is a critical advance that will help build better models and permit more predictive assessments of SOM responses to change.

The structure of the argument and manuscript are clear, but in some places I found citations lacking. While the intent may have been to lay out a conceptual framework rather

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than present an exhaustive review of past and current literature, I would recommend adding a few key citations to support some of the statements made. For instance, the discussion of LTBF could be initiated by citing the work of Ruhlmann (1999, Plant & Soil), who proposed using C in LTBF as the stable pool. The citation to Barre (2010) should also be updated to Biogeosciences, 7, 3839–3850, 2010. Section 4.1.2 on the use of multi-exponential equations to model respiration in incubations would be greatly enhanced with more references to the long history of work on this topic with citation to some of the papers by EA Paul. Papers by S Crow could enhance the last paragraph of this section on incubating physically isolated fractions, which illustrate challenges with this approach. Also, the wheat-corn succession experiment conducted at the “Closeaux” field trial at Versailles came to mind when reading the passage on gradual shifts. In this experiment, the C3-C4 shift was performed on one set of plots per year for 10 years. The experiment was used to study lignin dynamics in Dignac et al. (Geoderma, 2005, 128:3-17), and is likely reported elsewhere. And lastly, a new review was recently published by Trumbore on radiocarbon (Annu Rev Earth Planet Sci 2009), which should be cited in Section 4.2.3. There are many other opportunities to provide the reader with further references, and I recommend that the author consider these, without necessarily providing an exhaustive review.

I would also have liked to see more concrete examples for each of the experimental designs discussed when possible. The current manuscript provides only Table 3 as an example using data to illustrate the conceptual. The impact of the manuscript might be greatly enhanced with more such examples covering a wider range of the scenarios described, and I know that the author has previously published work that could be used as examples.

Lastly, in the “Challenges” section, the author presents the case for the partitioning of fluxes into more than two pools. While multicompartmental models typically have more than two pools, why would the separation of two pools be fundamentally insufficient? While I agree with the concept, I think an explicit rationale should be included. How-

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ever, there is a danger in demanding more pools. More pools generate better model fits, but how do we determine the “correct” number of pools? One extreme would be to go with a continuous model (e.g., q-model of Bosatta and Agren; Bruun et al. *Biogeosciences*, 7, 27-41, 2010), but anywhere in between two pools and infinite pools can seem somewhat arbitrary. Perhaps the authors could comment on this.

Specific & Technical comments

While the title is appropriate and informative, I wonder if something more specific than “link” could be used. Clearly, the goal is to provide a quantitative assessment of the contributions of various pools to the total soil CO₂ flux rather than a conceptual linkage. Perhaps a rephrasing of the title to avoid the term “link” and include the term “partitioning” or would be appropriate.

p.1958 ln1: Revise to “efflux in most”.

p.1959 ln4: Revise to “exhaustion of one of”.

p.1960 ln21: Revise to “partition total soil C for at least two pools”. Throughout the manuscript, numbers below 11 should be spelled out.

p.1960 ln25: Revise to “Only three”.

p.1962 ln11: Revise to “How can we use these”.

p.1962 ln17: Revise to “by the following”.

p.1963 ln23: Cite the figure at the end of the sentence rather than “modeling on Fig 3”. More importantly, there is no Figure 3 included with the manuscript. I believe the author is referring to Figure 2.

p.1964 ln3: “only one pool” seems out of place, please revise.

p.1965 ln10: Omit “e.g.”.

p.1967 ln1: Revise to “Besides” or “Aside from”.

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Table 1: I did not find this table particularly useful. Perhaps it could be omitted.

Shading in Fig 2 needs to be improved. It is also a rather “busy” figure. Perhaps it could be broken up into its constituent parts (abrupt permanent, gradual permanent and abrupt temporary) either as separate panels (Fig 2a,b,c) or separate figures (Fig 2, Fig 3, Fig 4).

Interactive comment on *Biogeosciences Discuss.*, 8, 1947, 2011.

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