Biogeosciences Discuss.,
doi:10.5194/bg-2016-86-RC2, 2016

Biogeosciences
Discussions
Open Access
EGU

Interactive comment on "Fate of rice shoot and root residues, rhizodeposits, and microbe-assimilated carbon in paddy soil: I. Decomposition and priming effect" *by* Zhenke Zhu et al.

Anonymous Referee #2

Received and published: 22 May 2016

Zhu et al. present an interesting and cleverly designed experiment examining the fate of different 13C-labeled tissues in rice paddy soil. However, in my opinion there are several important deficits in the presentation of the manuscript, one potentially significant error in analysis, and critical caveats of interpretation that should be considered.

First, the authors use an isotope mixing model based on delta notation (line 216) to partition CO2 from native SOM vs. 13C-labeled tissues. This is likely to yield biased results, because delta notation becomes highly nonlinear with respect to 13C atom percent away from zero per mil, and thus delta notation should not be used in the context of isotope labeling experiments. This flaw can be readily fixed by using atom percent 13C values, as opposed to delta values, in the mixing model. This should affect the magnitude of calculated C fluxes, but not the direction of the results.

Second, there appears to be confusion and a misstatement with respect to the total amount of C added in each treatment. If I understand correctly based on the methods, the total C in each treatment is as follows: Shoot C and Root C treatments should have 100 g of bulk soil (1.8 % C) plus 0.6 g tissue with C content of 41 and 29 %, respectively. This yields total mass-weighted C content of 2.04 % and 1.97 % based on the above data, which is in fact substantially greater than the other treatments (1.9%), in contrast to what is claimed without support in the abstract (where it is claimed that the Rhizo C and micro C had greater C).

This difference in C inputs among treatments is important to consider in the context of priming, on of the main foci of the study. If one assumes that there is a limited and finite capacity for stabilization of fresh C inputs to soil, regardless of source, one might postulate that the priming response to addition of C varies with the amount of C added. Thus, one could potentially observe differences in priming among treatments simply due to C quantity, in addition to the likely impact of biochemical differences among C substrates. This is especially important given that the treatment which exhibited the greatest priming also had the greatest C addition (2.04% for shoot C). I don't think this is necessarily a fatal flaw, but rather an important limitation of interpretation that needs to at least be acknowledged and discussed. It seems odd to me that the experiment was not designed to add a uniform amount of organic matter among treatments.

Third, estimates of variability around means are typically not presented. These are critically needed to interpret differences (or lack thereof) among treatments. There is also confusion and contradiction in the manuscript about which differences are significant or not, particularly with respect to priming in the root addition treatment (once it is stated that there was a positive priming effect, elsewhere it is stated that this was not significant). These will likely need to be re-evaluated with the new mixing model results from atom percent data, as discussed above.

Fourth, the hypothesis that was posed at the end of the introduction was ambiguous and was not further addressed in the discussion. The hypothesis needs to be justified in the Introduction, and evaluated in the context of the data in the Discussion.

Fifth, because there was a different mass of 13C added to each treatment, I cannot see how Figure 1a,b are useful, and these should likely be removedâĂŤFigures 1c,d show the normalized data and are much more useful.

Sixth, Tables 1 and 2 are confusing and possibly contain errors, as discussed below. Finally, although I agree with the authors' overall interpretation of the data, there are several sentences that are logically inconsistent throughout the manuscripts, where the statement at the beginning of the sentences does not support what follows. There is also substantial speculation and extraneous text that should be revised or removed. These are detailed below.

Detailed comments: 32-36: This statement is not logically consistent. An increase in 13C emissions does not imply lower soil organic C decomposition, nor that the rhizo-C and micro-C soils decrease mineralization of native soil C. 52: Heterotrophic microbes are typically much more abundant in terms of biomass than autorophs, and would be expected to be a more important C input to SOM. This distinction is not important here. 58-59: But you just mentioned the importance of microbes... green manure and manure are also often used in paddy systems. 75-77: This may be statistically significant but autotrophic microbial C fixation is equivalent to a rounding error in the total C budget of these systems... 81-83: This is a false dichotomy, as plant residues decompose to yield low molecular weight substances. 88: Contradicts the above statement, where you asserted that straw leads to priming. 97: How do you define complexity here? It is unclear whether fresh plant tissue or microbial biomass would be more complex than the other in terms of biochemical composition. This hypothesis needs to be introduced and justified in the context of the literature. 127: I disagree with this statementâĂŤmicrobes were definitely exposed to the 13CO2 label given that root respiration would have been enriched in 13C. Even heterotrophic microbes assimilate CO2 via anapleurotic fixation. This does not matter in the context of your treatments, and this text could be removed. 215: Delta notation should not be used for 13C-enriched samples because it is highly nonlinear away from 0 permil. The mixing analysis should be repeated using the atom percent data. 235: This contradicts what was stated in the abstract with respect to trends in 13C in the rhizo C and Micro C treatments. 232-237: Because there was a different mass of 13C label added to each treatment I think that Figure 1 a,b is misleading. Figure 1 c,d normalize the 13CO2 fluxes to the amount of label added, thus the treatments can be readily compared. I recommend removing Fig. 1a,b and the associated text in the Results. 250-255: Standard errors associated with these percentages are needed. 259-260: Standard errors needed 273-274: Isn't it trivial that the cumulative 13CO2 respired increased over the experiment? Discussing rates of change would be more informative. 278: Do you mean "no" effect? 304-310: This claim cannot be supported by the present data, and should be couched as speculation or removed. 319-321: Unsupported speculation 322-323: But you saw PE decrease over time, right? 328: But in natural systems, Rhizo C and Micro C typically accompany root and shoot CâĂŤthey are not present on their own, unless roots and shoots are manually removed. One implication of your results might be that soil C would disproportionately benefit from shoot removal by farmersâĂŤis this correct? 330-332: Better support for this claim would come from the isotope mixing model. 333: I assume you mean 13C of CO2? Need to specify here and elsewhere. 333: Unnecessary to include "rice-growing season" given that this is not a field study. 333-336: This conclusion does not follow from the premise. This sentence is confusing and not logically consistent. 336-340: That is one hypothesis; another would be that these tissues are selectively stabilized due to interactions with minerals or aggregate formation. This uncertainty should be acknowledged. 346: You stated before that the PE for root treated soils was insignificant. Need to be consistent in the textâĂŤis it significant or not? If not, PE is not positive. 350: Should mention as a caveat that different amounts of C were added in each treatment, and it is uncertain whether this contributed to differences in the results. Table 1: The third row is unclearâĂŤwhy does bulk soil have 0 mg total 13C, when it is 1.08 atom percent 13C? You need to clarify or account for natural abundance 13C. Table 2: “Size” of the pools is unclear hereâĂŤis this the proportion of 13C that was respired over the experiment?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-86, 2016.