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Biogeosciences
Discussions

Interactive comment on "Fluvial sedimentary deposits as carbon sinks: organic carbon pools and stabilization mechanisms across a Mediterranean catchment" *by* María Martínez-Mena et al.

Park (Editor)

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Dear Authors,

Thank you for submitting your manuscript to Biogeosciences.

To move forward the review process delayed by an assigned reviewer who does not appear to submit his report in a foreseeable time, I provide you with my own comments as detailed below. Please consider my comments in preparing your Author Responses.

Sincerely,

Ji-Hyung Park Associate Editor, Biogeosciences

<Major comments> 1. Terminology You cited one reference for "microbial degradation index", but I could not understand how the ratio described in the provided equation (Page 7, Line 10) may indicate the degree of microbial degradation. Furthermore, your interpretation of this ratio as an index of old C is also very confusing. As commented below, I would suggest you to articulate your rationale, supported by some data available from your or other studies. Another term "protected OC" would also require some theoretical or empirical back-ups.

2. Interpretation of results on OC in deposited sediments One of your main conclusions is stabilization of OC in deposited sediments. However, your measurements of basal respiration may indicate a higher lability of OC in your sediment samples compared with the source soils. Except some indirect indices (protected OC and microbial degradation index), you don't have any other C quality data that can support your arguments for more stable and older C in sediments. That's why you first need to provide robust backgrounds for the two ratios as well as a more in-depth discussion of the conflicting results on BR and OC compared between the source soils and sediments. In addition, your incubation settings did not consider different in situ conditions of the three set-ups (source soils, stream sediments, and deposited sediments). For example, aerobic conditions can accelerate degradation of sediment OM otherwise limited by O2? You did not provide any detail on this and other important environmental conditions such as initial moisture levels. Please discuss how the arbitrary lab conditions might deviate from the actual "filed conditions", affecting the measured rates of BR.

3 Editorial improvement Although the manuscript is generally well organized and written, I found numerous typos and scattered short paragraphs that can be more coherently organized, as some specific examples are indicated below. Please pay attention to details and revise the manuscript thoroughly.

<Specific comments> - L 24: Do you mean “representing suspended load and bedload in the main channel”? - L 30: Please define (or elaborate the meaning of) “physical stabilization” and “chemical protection”. - P 2, L 1-2: Please be more specific in providing your major conclusion about the relative importance of “temporary and permanent deposits”. Do you mean that both sources are equally important? - P 2, L 8: Please remove “but” and begin the following sentence with “However,”. - P 3 L 10-28: Please combine these into one paragraph. - P 3 L 24: Please fix this and other “numerous” super- and subscript typos throughout the manuscript . - P 3 L 26-28: Given the importance of aggregate structure for POC stabilization during transport and deposition, you need to provide a more detailed review of the previous works on this topic. I would suggest you to expand the short introduction misplaced at the end of the first paragraph (- P 2 L 20-26) with these (Six et al„) and more recent citations in a separate paragraph. - P 4 L 11: “more detailed” description? - P 4 L 27-28: Please provide more details on soil sampling: depth, sampling method, etc. - P 5 L 1: “after a flooding event”? As you know, the bulk of suspended load is transported “during” rainfall events, so sampling timing is a critical information. Please specify when and how long suspended sediment was collected. - P 5 L 26: What is “min”? - P 6 L6: 40oC or 50Oc? Please provide reason in case you used different temperatures. - P 6 L 25: “protected OC” is a misnomer, because this is actually a ratio of “protected OC to MPOM”. - P 7 L 10: Why don’t you use simply “OC-M” as denominator? In addition, it is assumed here that OC in free microaggregates and mineral fractions is older than OC in macroaggregates. Do you have any data supporting this assumption? If not, you need to reformulate relevant sentences throughout the manuscript. - P 7 L 17: 30 g soil “on a dry mass basis”? - P 7 L 14: There must have been significant reductions in soil moisture given the high incubation temperature and 32 days of incubation. Please clarify this. - P 8 L 1: Please describe why you opted for the nonparametric test. You might need to mention any prior test for normal distribution. - Sections 3.2-3.3: These two short sections can be better combined into one section, in a more coherent way to compare OC fractions among the three watershed components. - Sections 3.4 & 3.5: Please also consider

to integrate these sections in the preceding one or in a separate section on OC quality. - P 11 L 14: Please rephrase “macroaggregates are the nucleus for microaggregate formation”. How can larger macroaggregates function as the nucleus? - P 11 L 8-11: I would provide an overview of major findings on different erosion sources in this beginning paragraph. - P 12 L 20-25: This type of 1 to 1 comparison between sediment and source soils does not make sense, because three sources have different source capacity. Please take into consideration estimates of source capacity in evaluating C enrichment or depletion during OC transport. - P 13 L 4: Typo at the end of the sentence. - P 13 L 6: “sediments”? - P 13 L 12: “soil forming” or “aggregate forming”? - P 13 L 17-21: Please rewrite (better split) this long and vague sentence. This and the following sentences are logically conflicting, because you are arguing that OC in the deposited sediments is more stabilized than the source soils, even though more labile materials, as evidenced by higher BR rates, exist in the same sediments. Please clarify this. - P 13 L 21: typo “Thw" - P 13 L 22: Please specify what you meant by “microbial induced processes”. - P 14 L 10: You did not measure “microbial activity”. - P 14 L 16-18: Again, this short paragraph can find a better place in the preceding one. - P 15 L 5: Again, you need to clarify how more stabilized OC in deposited sediments exhibited higher rates of BR compared with those measured for the source soils. - Fig 3: Please clarify in the figure legend whether significant differences indicated by different letters are among the compared fractions or soil/sediment samples.

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