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Interactive comment on "Soil organic carbon decomposition rates in river systems: effect of experimental conditions" *by* Man Zhao et al.

Anonymous Referee #1

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General Comments:

This study examines microbial and hydrodynamic mechanisms for soil organic carbon (SOC) in rivers based on a series of incubation experiments. The topic is both timely and important. The authors found that amending incubations with aquatic microbes drove a significant increase in SOC decomposition, whereas shaking the incubations did not have a significant impact on respiration rates.

In general the manuscript is well-written. The discussion section can be improved with a bit more referencing in parts, and also a bit deeper discussion of factors that may lead to the observed results. The introduction could also be improved by adding some nuance to the discussion about SOC. The authors overly simplify that SOC mobilized into rivers is 1) generally old and 2) assume that all soil-derived OC enters rivers in the

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particulate phase. In terms of data, it would be useful to describe the starting conditions of the incubations. Specifically, the DO and nutrient levels are not mentioned, both of which could significantly influence interpretations of the results. Specific comments are given below.

Specific Comments:

Line 29: In general, the statement that much of the SOC transported into rivers is old should be made with more nuance. For example, the Mayorga reference shows that DOM in the Amazon and DIC are both predominantly modern in age. The point that tropical, temperate, and high latitude rivers behave differently is important to make.

Likewise, when reading the intro I get the impression that the authors refer to SOC as being in the river in the particulate phase only. Soil organic carbon is also leached into rivers in the dissolved phase, and most studies indicate that DOM is the main substrate fueling respiration.

Line 90: Please provide more detail. Were soil cores collected or surface soils? How deep were the soils collected from? Do the authors suspect that the SOC used was old or modern per my comment on Line 29?

Line 93: Why was this concentration chosen? That is a rather high POC concentration. Was the intention to mimic conditions you might find in the rivers being studied?

Line 105: What was the DO measurement frequency?

Line 180: Is there any nutrient data available for the experiments and/or the river water that was used? For example, more N was presumably added for the arable soils since the C:N ratio is lower than the forest soils. Nutrient limitation could be one important factor, but I am unable to evaluate this.

Line 193: Use consistent units

Line 212: Perhaps you could expand on this discussion a bit more. The Ward 2018 experiment was fundamentally different for many reasons, so it's not surprising that the results did not show the same thing. The biggest factor is that they used raw (unfiltered) water, which means the abundance and composition of POC, DOC, microbes was the same as ambient conditions, whereas this present study used an inoculum and manipulated soil additions. Another difference to mention is that the Ward experiment took place in a tropical river known for its high respiration rates as opposed to this study taking place in a temperate environment. This present study also added ∼2-3 times more SOC than is present in the turbid Amazon River and also added beads to the incubation. Do POC concentrations in the Dilje River ever get that high, i.e. were the manipulations realistic? How full of beads were the containers? This particle surface, could allow microbes to be active throughout the entire bottle even when stationary, e.g. the hypothesis by Ward 2018 was that "The relationship between rotational velocity and respiration rates exists because of the importance of interactions between suspended particles, dissolved constituents, and free-living and particle-bound microbes in driving aquatic metabolism." In contrast, this present study hypothesizes that the physical breakdown of SOC particles by disturbance is what should cause higher respiration rates.

Another important point is context about nutrient conditions. How do we know that nutrients weren't limiting?

One finding in the Ward et al 2019 paper that was cited was that respiration rates varied in response to the proportion of turbid vs clearwater river water added to incubations. There was an optimal mixture that resulted in the highest rates, and in those experiments there wasn't always a significant difference between stationary and spinning chambers. That could perhaps be something to bring up here, e.g. perhaps you would have seen different results with lower POC concentrations more similar to what you'd observe in situ. And likewise, removing the beads could have made the rotation treatment more important.

Line 298: How do you know the SOC in this experiment was old?

Lines 225-280: This section could use more references and literature comparisons.

Table 3: The caption is a little confusing. By "weight" do you mean the mass of C added?

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