

Dear editor,

We gratefully thank you for your comments and have revised the manuscript accordingly. In our response below, your comments are shown in italicized *blue*, our response in **black**.

*1. Title: First, “decomposition”, not “decomposition rate”, is rapid, so it would be more appropriate if the title is corrected to “Rapid soil organic carbon decomposition in river systems...” Second, “priming” might have played a role in the enhanced decomposition of SOC, but you have not introduced the concept at all, either in the abstract or the introduction. You can use a more relevant term you used, such as “aquatic microbial communities”.*

**Reply:** The title has been improved according to the comments above.

**The title now reads** “Rapid soil organic carbon decomposition in river systems: effect of the aquatic microbial community and hydrodynamical disturbance”.

*2. Line 8 (track-change version) “which is often old”: As the first reviewer commented (and you responded to the comment), you need to make clear that soil organic carbon is a mixture or continuum of materials varying in age. This specific sentence is confusing because it is not clear whether “which” refers to “a significant fraction” or “SOC”. As your I4C data showed, the significant fraction that rapidly decomposed might be a rather young, labile fraction. Therefore, you need to clarify the sentence.*

**Reply:** This statement has been revised in the manuscript.

**Line 6-8 now reads** “Mounting evidence indicates that a significant fraction of this SOC, *which displays a wide range of ages*, is rapidly decomposed after entering the river system”.

*3. Line 29 “storage time”: Do you mean “retention time”? I would place “among rivers” before “depending.....”.*

**Reply:** This sentence has been improved according to the comments above. See **Line 27-28, Page 1**.

**Line 27-28 now reads** “The mobilized SOC can display a very wide range of ages among rivers depending on the carbon sources and retention time”.

*4. Line 99 “4 ml unfiltered river water was added”: Please provide the ratio of inoculum to the incubated water sample.*

**Reply:** Agreed, this information has been added in the manuscript, see **Line 93-94, Page 3**.

**Line 93-94 now reads** “4 ml unfiltered river water (ratio of inoculum to the incubated water sample: 1:79) was added to serve as an inoculum for treatments with aquatic microbial organisms”.

5. Line 104 “POC concentration was controlled at 10–12 mg L<sup>-1</sup>”: here or somewhere later in the discussion, please provide the rationale for using this rather high (but often observed during intense rainfall events) range of POC concentrations, as you described in your response to the first reviewer’s comment. In addition, please provide the water sample volume to which 60 mg or 160 mg soil was added.

**Reply:** Thank you for your constructive comments. We now have added the rationale for using this high POC concentration. For more detailed information, see **line 98-101, Page 3**.

**Line 98-101 now reads** “In order to obtain a detectable rate of oxygen consumption, the POC concentration was controlled at 10–12 mg L<sup>-1</sup> by adding 160 mg arable soil and 60 mg forest soil in 320 ml river water (details in Table 3). While the sediment and POC concentrations we used in our experiments are relatively high, they are not unrealistic: during high flood we observed POC concentrations exceeding 10 mg L<sup>-1</sup> in ca. 5% of our samples”.

6. Line 203: As mentioned earlier, only “some fraction of SOC”, not all SOC components, can be mineralized quickly.

**Reply:** Agreed, this has been revised in the manuscript, see **Line 203-204, Page 6**.

**Line 203-204 now reads** “Our results show that a fraction of the terrestrial SOC can indeed be mineralized relatively quickly when introduced in an aquatic environment”.

7. Line 205 “83–139  $\mu\text{g C L}^{-1} \text{ d}^{-1}$ – $1\mu\text{g C}^{-1} \text{ d}^{-1}$ .”: This rate depends on how much POC you put in your water samples, so it would be more appropriate if you provide the values in the unit of  $\mu\text{g C/g soil C/d}$ .

**Reply:** Agreed, we have normalized decomposition rates relative to the amount of POC, and have added it in the revised manuscript. We would also keep the absolute rates in this section, since we compare them here to date reported in the papers cited there. See **Line 205, Page 6**.

8. Lines 182-187: Please indicate significant differences in the corresponding tables and figures. For instance, in Figs. 6 and 7 you can easily show this by putting asterisks on top of the corresponding values.

**Reply:** The significances discussed in the text has been indicated in **Table 4**.

As for Figure 6 and Figure 7, since we compared (i) different soil types, (ii) rotation vs. stationary and (iii) with or without the presence of AMO, we think that including significant differences in Figure 6 and Figure 7 may lead to confusions. Instead, we now have mentioned all significances explicitly in the text, and the ones central to our discussion are summarized in Table 4. For more detailed information, see **Line 171-173, Line 175-177, Page 5; Line 197, Page 6**.

9. Line 238 “priming”: Please define this concept at its first use and provide a relevant reference.

**Reply:** In this manuscript, with “priming” we mean the stimulation effect of the presence of the aquatic microbial community on SOC decomposition rates. In order to avoid leading misunderstanding on “priming effect”, we have replaced “priming” with specific descriptions. See **Line 1-2, Page 1; Line 236-237, Page 7**.

**Line 1-2 now reads** “Rapid soil organic carbon decomposition in river systems: effect of the aquatic microbial community and hydrodynamical disturbance”.

**Line 236-237 now reads** “Thus, our data do indicate that the relative role of physical disturbance vs. that of exposure to an aquatic microbial community may vary considerably between different ecosystems”.

*10. Line 252 “systematic”: Difficult to understand; please replace the word or elaborate what you meant.*

**Reply:** We have rephrased this sentence in the manuscript. See **Line 250-251, Page 7**.

**Line 250-251 now reads** “However, the effect of mechanical disturbance is small and statistically insignificant, also for the forest soil.”.

*11. Line 255 “which is consistent with its higher 14C age (Table 3)”: This is not always the case, because aged SOC tends to have lower C/N ratios given N accumulation during decomposition.*

**Reply:** Given that the age difference between the two soil types is very small (267 yr vs. 334 yr), we have removed this statement in the revised manuscript.

*12. Line 316 “have a weak structure”: Is this a confirmed fact? You can be more cautious in wording, like using “may”.*

**Reply:** This statement was made given that the soils used in this study were loess derived from a Belgian loess belt, known for their very low structure stability (Govers, 1991). We now have improved this part by adding the reference. See **Line 315-316, Page 9**.

#### **Reference**

Govers, G.: Rill Erosion on Arable Land in Central Belgium: Rates, Controls and Predictability, *Catena*, 18(2), 133-155, 10.1016/0341-8162(91)90013-n, 1991.

*13. Tables 3 & 4: “A”rable land, “F”orest*

**Reply:** These have been corrected in **Table 3 & 4**.

*14. Fig. 8: For “high”, please use consistently the uppercase initial letter (H).*

**Reply:** This has been corrected in **Figure 8**.