Response to Anonymous Referee #2

We thank Referee #2 for his/her constructive comments, which are addressed as explained below. In our response below, referee comments are shown in italicized *blue*, our response in **black**.

General comments

In the present study, the authors aim to understand the mechanisms of the SOC decomposition in river systems. Their study is based on 2 hypotheses (e.g., (i) in the river water, SOC is exposed to an aquatic microbial community which may be able to metabolize SOC much more quickly than the soil microbial community, and (ii) SOC decomposition in rivers is facilitated due to the hydrodynamic disturbance of sediment) for which they will investigate their weight through an incubation experiment. The paper is interesting and the study is well designed. However, before acceptance, I would suggest addressing my comments.

My main concern is related to the discussion. I think the authors could improve the discussion with a deeper exploration of their results. Some parts look weak and not necessarily well supported by the literature (see my specific comments). Then, some other parts are the opposite. I would also suggest adding sub-sections to the Discussion to give a framework to the discussion.

Reply: We thank the reviewer for the constructive comments on the Discussion section, which will be addressed accordingly in the manuscript. We will seriously rework on the Discussion section by (i) further exploring the results and connecting them with previous studies, (ii) improve the structure and provide a better framework.

Specific comments

Line 65-103: I would suggest re-organizing these three sub-sections. For example, in the first sub-section, you present the site but you also include extra information in subsection 3. Then, in reading the sub-section 2, several questions came up in my mind. But I could find the answers only on sub-section 3. In the current form, it is a bit confusing and the readers need to go back and forth to gather all the information.

Reply: Thank you for this constructive comment, we will reorganize the subsections to better describe the approach in the revised version.

Figures 6 and 7: I would suggest changing the scale of the Y-axis and/or use colour. In particular, Figure 7 a and c or even d are not easily readable.

Reply: Agreed, we will improve the figures in the revised manuscript.

Line 172-174: Does this "increase" really mean something?

Reply: Yes, this increase was consistent in all replicates, we think that the increase of $\delta^{13}C_{POC}$ values during the first 24–48 hours suggests that during this period an isotopically lighter POC fraction was preferentially mineralised. This resulted in the POC in the aquatic environment becoming enriched in ^{13}C compared to the POC in the original soil sample. While this shift in $\delta^{13}C$ values is relatively small, we do feel it is significant given that it is consistent in both experiments, and larger than the analytical error, However, we are careful in our discussion and as to avoid any overinterpretations on this.

Line 205-207: This needs to be supported by the literature. Please refer to Ward et al., 2019, Wu et al., 2018, etc.

Reply: Agreed, this part will be improved following the suggestions made.

Line 244: What about the combined effect of AMO and rotation?

Reply: To identify the combined effect of AMO and rotation on the C decomposition rates, two-way ANOVA with the presence of AMO and disturbance conditions as the main factors was employed for the two soil types, separately. Results showed that the presence of AMO and rotation had no significant combined effect on the C decomposition rates for both soil types in our study

(*arable land: p=0.430; forest: p=0.683*). This is not surprising, as we proposed in the conceptual model, the mere immersion of soil particles in water might be sufficient to destroy most of the soil particles which were loess derived with the low structure stability. Therefore, further disturbance did not significantly increase the interactions between soil particles and microbial organisms. This combined effect might be more evident for SOC with strong physical protection.

Line 273: After 160h of incubation, can we expect a significant shift of the $\delta^{13}C_{POC}$? This needs to be discussed.

Reply: If this mineralization does not selectively affect specific fractions of the POC pool, the $\delta^{13}C_{POC}$ values can be expected to remain more or less constant throughout the incubation period. We will add discussion regarding to this aspect in the revised manuscript.

Line 278-290: You never discuss the combined effect of the occurrence of aquatic microbial organisms and physical disturbance. The discussion needs to be improved regarding this point.

Reply: The combined effect of the occurrence of aquatic microbial organisms and physical disturbance has been outlined above. We agreed that the combined effect of the occurrence of AMO and physical disturbance would be an interesting point to be further studied, and we will bring this into discussion in the revised manuscript.

Line 184-290: I have observed specific behaviour for each type of soils. I think this point needs to be highlighted and also discussed. How do you explain these variations?

Reply: We thank the referee for their constructive comment on comparison of the two soil types. We feel that the different SOC contents and the nature of the SOC (derived from agricultural crops versus forest litter) could offer a likely explanation for, the observed difference in decomposition behaviour. We will add a paragraph in the Discussion section to compare the decomposition behaviour, and possibly link this to the ^{14}C ages which are currently being determined.