Dear Editor,

Thank you for obtaining helpful reviews of our manuscript. Please find our point-by-point responses below. We are grateful for the feedback and the opportunity to respond to it, which we feel has helped improve the manuscript. Our responses are in **bold** font while reviewer comments are in regular font.

Sincerely,

James Stegen (on behalf of all co-authors)

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Reviewer 1:

General

As papers on river ecosystem functioning are booming, the exact meaning and the interrelation of different "ecosystem processes" are becoming a hot issue. The paper by Stegen et al. makes an interesting contribution to the field. They measured cotton-strip decomposition at 48 sites across the Yakima River Basin (YRB) and compared the results to a paper (Garayburu-Caruso et al., 2025), performed at the same sites, that measured whole-ecosystem and water-column respiration, and inferred sediment respiration. Stegen et al. found that cotton-strip decomposition is more tightly linked to total (whole-ecosystem) than to sediment respiration, an unexpected outcome. They also found, unsurprisingly, a very high variability in cotton decomposition across the YRB.

Overall, the paper makes a nice contribution to the field. The methods are sound, results interesting and the discussion mostly accurate. There are, however, some small issues that should be considered before publication.

Thank you for the encouraging remarks and for the suggested improvements. Below are our point-by-point revisions based on your suggestions.

Specific comments

The title is a bit misleading. The paper shows point-scale organic matter decomposition to be more tightly linked to total than to sediment respiration but does not clearly address (or not in sufficient detail) the basin-scale connection between both processes. The authors should find a title that better reflects their findings.

The title has been edited to address this suggestion, it now reads: "Point scale organic matter decomposition in streambeds is weakly associated with reach-scale respiration."

L15-16. Is "microbial-catalyzed" a correct expression? Better rewrite as "microbially-catalyzed" or as "microbe-catalyzed".

The referenced text has been edited to "microbe-catalyzed."

L24-26. It is hard to imagine a mechanism for this relationship between cotton decomposition and whole-ecosystem (but not sediment) respiration.

We too were surprised by the result. We added further details to our interpretation within the associated Results and Discussion section, as opposed to adding more interpretation to the Abstract. The additional text in the Results and Discussion is in the subsection "Respiration in sediments explains little variation in decomposition" and reads as follows: "To interpret these results, we note that cotton strips were deployed a few centimeters into the riverbed sediments, beneath a brick set flush with the streambed surface. We conceptualized this deployment as the shallow hyporheic zone, which was a reason we hypothesized that decay rates would be most strongly connected to ER_{sed}. However, the results indicate that point-scale decomposition of particulate organic matter within the shallow hyporheic zone is linked to respiratory processes occurring in both the sediment and water column. Therefore, our deployment depth might have reflected sediment-water interface processes in addition to shallow hyporheic zone processes, leading to stronger associations of cotton strip decomposition with ER_{tot}. We also note that ER_{sed} estimates may not exclusively reflect biological respiration, as non-respiratory O_2-consuming processes can contribute to these estimates (see Methods). We propose that if our deployment configuration was complemented with a simultaneous deployment that enabled growth of benthic algal biofilms on the cotton strips, the combined decomposition from both deployments could, in some systems, capture more of the processes that contribute to ER_{sed}. This would provide a more complete view of sediment-associated biogeochemical function, potentially leading to a stronger correlation between decomposition rates and ER_{sed}."

L51. The accumulation or release of carbon is not a measure of heterotrophicity. A heterotrophic ecosystem is one that produces more organic carbon than it mineralizes, which is not exactly the same thing. Please, rephrase.

We revised the original sentence from: "Streams are commonly net heterotrophic whereby they release more carbon than they accumulate (Battin et al., 2023; Bernhardt et al., 2022)" to: "Streams are commonly net heterotrophic, meaning they mineralize more organic-matter–associated carbon than they fix by in-stream primary production (Battin et al., 2023; Bernhardt et al., 2022)."

L88-96. Please, give more details about the Garayburu-Caruso paper. Explain the estimated length of the reaches for which whole-ecosystem respiration was calculated and whether this length could create autocorrelation issues between neighboring sites.

We added text to the Methods section addressing these aspects of the data. The added text reads: "In brief, DO timeseries were analyzed via StreamMetabolizer (Appling et al., 2018) to estimate ER_{tot}. The length of the reach that a given DO sensor integrates varies across streams and has been estimated to be roughly three times the turnover length of O_2 (Hall & Hotchkiss, 2017). As such, integrated reach lengths likely varied across sites due to differences in reaeration and discharge. Field sites were located in separate, non-overlapping reaches, helping to minimize potential spatial autocorrelation between neighboring sites in the estimated metabolic rates."

L100-101. ERsed was calculated from the difference between ERtot and ERwc. This will, thus, include respiration in the hyporheos, on the sediment surface and by submerged plants, but also any non-respiratory process that consumes oxygen, such as oxydation of metals in upwelling groundwaters. The authors should be more cautious when linking oxygen consumption and respiration and specifically state that there can be multiple non-respiratory processes consuming oxygen across a basin. It would also be nice to see this issue in the discussion.

We have added text to the Methods section that explicitly acknowledges the points raised by the reviewer. The revised text in the Methods section now reads:

"We note, however, that ER estimates derived from open diel O_2 methods capture all oxygen-consuming processes, not solely aerobic respiration. These estimates could also include abiotic oxidation of dissolved Fe(II) or the oxidation of other end products from anaerobic metabolism (Demars et al., 2015; Piatka et al., 2021), other oxidation processes such as nitrification and photooxidation of organic matter (Demars et al., 2015; Estapa and Mayer, 2010), or O_2 inputs from groundwater (Hall and Tank, 2005). Because ER_{sed} was calculated by the difference between ER_{tot} and ER_{wc}, it may be sensitive to these non-respiratory O_2 sinks. However, biological metabolism is generally considered the dominant O_2-consuming pathway in streams and we therefore interpret ER_{sed} primarily as sediment-associated biological O_2 consumption while acknowledging that other contributions may be captured."

We have also added corresponding text to the Results and Discussion addressing this issue. This text reads: "We also note that ER_{sed} estimates may not exclusively reflect biological respiration, as non-respiratory O_2-consuming processes can contribute to these estimates (see Methods)."

L106. Strips were deployed on the surface of the riverbed and covered with a brick. This makes them much more likely to correlate with processes occurring at the water column than if they had been deployed deeper into the sediments. Authors should at least discuss the effects of their methodological choice.

To more fully bring that methodological choice into the interpretations, we added some additional discussion on this point in the relevant Results and Discussion subsection. The new text reads:

"To interpret these results, we note that cotton strips were deployed a few centimeters into the riverbed sediments, beneath a brick set flush with the streambed surface. We conceptualized this deployment as the shallow hyporheic zone, which was a reason we hypothesized that decay rates would be most strongly connected to $ER_{sed.}$ However, the results indicate that point-scale decomposition of particulate organic matter within the shallow hyporheic zone is linked to respiratory processes occurring in both the sediment and water column. Therefore, our deployment depth might have reflected sediment-water interface processes in addition to shallow hyporheic zone processes, leading to stronger associations of cotton strip decomposition with ER_{tot}. We also note that ER_{sed} estimates may

not exclusively reflect biological respiration, as non-respiratory O_2-consuming processes can contribute to these estimates (see Methods). We propose that if our deployment configuration was complemented with a simultaneous deployment that enabled growth of benthic algal biofilms on the cotton strips, the combined decomposition from both deployments could, in some systems, capture more of the processes that contribute to ER_{sed}. This would provide a more complete view of sediment-associated biogeochemical function, potentially leading to a stronger correlation between decomposition rates and ER_{sed}."

L113. 7th-order rivers can have very deep sections. The authors seem to have deployed all their strips on wadeable parts of the river. It would be good to at least give some caveats about this reflecting "true" decomposition across the river, and better to discuss how this could have affected results.

We included the following text in the last subsection of the Results and Discussion. "We note that our decomposition estimates represent point-scale conditions at wadeable locations and should be interpreted as such rather than as reach-integrated measures of decomposition. Capturing within-reach spatial variability in decomposition across habitats and channel depths would complement the among-reach approach used here, and may lead to stronger relationships between decomposition rates and reach-scale ER."

We also added the following text to the methods section: "This design prioritized among-reach coverage across the basin (48 sites) and provided point-scale estimates of decomposition potential at a single location within each reach.

The cotton strips were deployed in the shallow hyporheic zone for 35 continuous days at 48 sites across the YRB (Fig. 1). In larger river systems (e.g., 7th-order reaches), deployments were limited to wadeable areas near channel margins."

L116. The four replicated strips were deployed next to each other, which is very convenient to deploy and recover them, but not at all when we want to capture sediment decomposition rate of a reach that for whole-ecosystem respiration can be hundreds to thousends metres long. The literature has shown breakdown rates to differ markedly from consecutive riffles, even more so between riffles and pools, and even more between substrata deployed on the sediment, as is the case here, and in the sediment. This should be mentioned as a caveat on the methodology and discussed later on.

We agree and note that it is closely related to the comment at L113. We addressed both points together in the comment above.

L139-146. It would be interesting to compute respiration in degree days also.

We agree that could be interesting and would parallel the degree day based estimation of decomposition. We are not aware of existing literature that normalizes reach-scale respiration by degree days, instead of representing the rates per day. In the estimation of respiration there are parameters in the model that are temperature dependent and we are not entirely sure if it is appropriate to simply divide by degree

days. We do feel it would be a useful technical exercise to deeply evaluate how to best represent reach scale respiration per degree day. We also feel that deserves a focused effort that is beyond the scope of the current paper. As such, we prefer to leave such calculations to future efforts, as opposed to introducing a new method that's not fully developed in our current paper.

L154. "....rates, a..." replace point by comma.

This edit has been addressed.

L169. The variation in decomposition rate is surprising given that data come from a single basin? Not so much. Benthic processes are extremely patchy. But this also draws the question whether 4 strips are enough to characterize the decomposition of a reach whose metabolism has been characterized by the open-channel method, which typically has a spatial extent of hundreds to thousands of metres.

This comment is closely related to comments on L113 and L116; please see our responses related to L113.

L173-174. Delete. Climatically diverse basins are interesting, but any other diversity factor (geologic, topographic, human...) can result in interesting information.

We edited the original sentence from: "This emphasizes the value of climatically diverse watersheds like the YRB as useful testbeds to study variation in decomposition rates within single hydrologically connected basins" to: "This emphasizes the value of environmentally diverse watersheds like the YRB as useful testbeds to study variation in decomposition rates within single hydrologically connected basins."

L178-181. High decomposition rates in summer have been often reported. Nevertheless, it is unclear whether these derive from high temperatures, as even when correcting by degree days the decomposition tends to be higher in summer. It is also unclear these high rates are a consequence of slow flows, as slow flows can also reduce the exchange of nutrients etc. between water and OM, thus slowing decomposition. In fact, comparison between mesohabitats systematically show decomposition to be faster on riffles than on runs or pools. Therefore, the high summer decomposition rate more probably is a consequence of the longer time of stability since the last disrupting floods, which allows developing the biological communities, or the less diluted waters, which enhance nutrient concentration. Please, discuss in greater detail.

To elaborate on the potential effects of a long time-since-disturbance that could allow for robust biological community development, we revised this sentence from: "This shift towards faster rates and the wide range in rates may be because YRB rates were estimated in later summer, a time of year when decay processes are likely maximized due to relatively high temperatures and slow flows (Collier et al., 2013b; Mancuso et al., 2023)"

To "This shift towards faster rates and the wide range in rates are likely due to several factors linked to rates being estimated in later summer with high temperatures, and established biological communities due to several months since high-flow disturbances (Collier et al., 2013b; Grimm and Fisher, 1989; Mancuso et al., 2023)."

L182-185. Not so surprising. This simply shows that in the YRB other factors (nutrients? sediment biota? fine sediment accumulation? toxics?) override the effects of temperature, what also has been reported in many papers.

We revised the sentence from: "These results are surprising given previously reported influences of temperature on particulate organic matter decomposition (Benbi et al., 2014; Griffiths and Tiegs, 2016)" to: "These results contrast previously reported influences of temperature on particulate organic-matter decomposition (Benbi et al., 2014; Griffiths and Tiegs, 2016), and likely reflect dominance of other influential factors such as variation in nutrient concentrations (Rosemond et al. 2015)."

L215-217. I cannot agree. From the description it seems the strips were located on top of the sediments, and below one brick. This is not exactly hyporheic. Other authors have measured decomposition within the sediments and found it to greatly differ from that on the surface.

The result the sentence refers to is "…decay rates were most strongly connected to ER_{tot}, less so with ER_{sed}, and not at all with ERwc (Fig. 5)."

The deployed brick was at the same level as the streambed such that cotton strips (below the brick) were a few cm into the streambed. Where exactly the hyporheic zone starts is ambiguous, which is why we write "…the shallow hyporheic zone (i.e., within the riverbed sediments)."

To avoid calling the result 'surprising' we revised the original sentence from "These results are surprising, in part, because the cotton strips were deployed within the shallow hyporheic zone (i.e., within the riverbed sediments)" to: "To interpret these results, we note that cotton strips were deployed a few centimeters into the riverbed sediments, beneath a brick set flush with the streambed surface. We conceptualized this deployment as the shallow hyporheic zone, which was a reason we hypothesized that decay rates would be most strongly connected to ER_{sed}."

L220-228. Very speculative and will depend ultimately on the contribution of epilithon to the total sediment respiration, which can be highly variable depending on the extent and activity of the hyporheic zone.

We edited the original sentence from: "We propose that if our deployment configuration was complemented with a simultaneous deployment that enabled growth of benthic algal biofilms on the cotton strips, the combined decomposition from both deployments would capture substantially more of the processes that contribute to ER_{sed}."

To: "We propose that if our deployment configuration was complemented with a simultaneous deployment that enabled growth of benthic algal biofilms on the cotton strips, the combined decomposition from both deployments could, in some systems, capture more of the processes that contribute to ER_{sed}."

L256-259. That would affect Rsed, but not K values, as the surface for microbial attachment of cotton strips would be independent of the sediment. What about other factors? Aridity usually results in higher temporal variability in flows, lower nutrient concentration during baseflow, less fungal inoculum, higher deposition of fine sediments, which cloggs intestices…

We edited the original sentence from: "If the relationship between decomposition and D50 is causal, it could be mediated by the total surface area available for microbial attachment. Coarser sediments have much less surface area, potentially limiting overall microbial activity."

To: "If the relationship between decomposition and D50 is causal, it could be mediated by the total surface area available for microbial attachment. This would, however, influence decomposition only to the extent that microbial biomass in adjacent sediments impacts microbial activity on the cotton strips."

L273-275. Not convincing. These authors show that a large fraction of respiration comes from the degradation of labile carbon produced by photosynthetic organisms, but this should have no effect on cotton decomposition, unless you are talking about the priming effect?

We edited the original sentences from: "Though not measured here, less light could suppress autotrophic production which will limit heterotrophic respiration (Bernhardt et al., 2022; Mulholland et al., 2001; Young and Huryn, 1999) and, in turn, indirectly lead to slower decomposition relative to high light leading to priming organic matter degradation via labile carbon derived from phototrophs."

To: "Though not measured here, less light could suppress autotrophic production which may limit heterotrophic respiration (Bernhardt et al., 2022; Mulholland et al., 2001; Young and Huryn, 1999). Lower autotrophic production could therefore slow decomposition relative to high-light conditions by reducing the supply of labile carbon from phototrophs that can prime organic-matter degradation (Danger et al. 2013; Howard-Parker et al. 2020)."

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Reviewer 2:

This MS describes the results of a comparison of cotton strip decomposition rates within the riverbed with whole ecosystem and water column respiration rates measured at the same set of sites across the Yakima River Basin. The authors found that cotton strip decomposition rates were more closely related with whole ecosystem respiration than they were with an estimate of sediment-associated respiration calculated as the difference between measured whole ecosystem and water column respiration rates. The MS is generally well-written and will make a useful contribution to the literature on the relative contribution of different elements to overall river ecosystem respiration.

Thank you for the encouraging remarks. We are pleased that Reviewer 2 feels our study will make a nice contribution to the literature. Below are the point-by-point responses and edits to the suggestions.

My main concern with the MS relates to the use of the term sediment-associated respiration (ER_{sed}). My initial perception was that this parameter represents respiration within the sediment beneath the riverbed, when in fact it represents respiration both within and on the riverbed. I think this needs to be emphasized early in the MS so readers are clear about what this parameter really represents. Importantly, a large component of ER_{sed} may be attributed to autotrophic respiration from periphyton and aquatic plants growing on the riverbed and not associated with heterotrophic carbon decomposition. Therefore, there is less expectation that there should be a strong link between ER_{sed} and cotton strip decomposition rates within the bed.

In the third paragraph of the Introduction we define ERs_{ed} and state that "ER_{sed} comprises reach scale sediment-associated respiration from benthic/streambed sediments, rooted/submerged plants, and hyporheic zones that are hydrologically connected to the active channel…" We feel this definition is placed in the right location (early in the paper and adjacent to definitions of ER_{tot} and ER_{wc}) and is explicit about the combination of system components that contribute to ER_{sed}. We thought about ways to improve this, but prefer to retain the existing text.

It would also be useful for the authors to emphasize what other elements might be included in the ER_{sed} parameter, since it was not measured directly. For example, has the potential impact of low DO groundwater inputs on whole ecosystem respiration been accounted for? Could chemical oxygen demand contribute to some of the ER_{sed}?

We added text to the Methods section that explicitly acknowledges the points raised by the reviewer. The revised text in the Methods section now reads:

"We note, however, that ER estimates derived from open diel O_2 methods capture all oxygen-consuming processes, not solely aerobic respiration. These estimates could also include abiotic oxidation of dissolved Fe(II) or the oxidation of other end products from anaerobic metabolism (Demars et al., 2015; Piatka et al., 2021), other oxidation processes such as nitrification and photooxidation of organic matter (Demars et al., 2015; Estapa and Mayer, 2010), or O_2 inputs from groundwater (Hall and Tank, 2005). Because ER_{sed} was calculated by the difference between ER_{tot} and ER_{wc}, it may be sensitive to these non-respiratory O_2 sinks. However, biological metabolism is generally considered the dominant O_2-consuming pathway in streams and we therefore interpret ER_{sed} primarily as sediment-associated biological O_2 consumption while acknowledging that other contributions may be captured."

We also added corresponding text to the Discussion addressing this issue. This text reads: "We also note that ER_{sed} estimates may not exclusively reflect biological respiration, as non-respiratory O_2-consuming processes can contribute to these estimates (see Methods)."

I'm also concerned about whether the overall weak relationships between point-scale cotton strip decomposition and reach scale measures of respiration primarily reflect differences in the scale of measurement rather than poor linkage between different components of the river ecosystem. I'd like to see some evidence in the MS that 4 replicate measures of cotton strip decomposition are sufficient to represent small-scale variability in decomposition at a reach scale. How did within-reach variability compare with between-reach variability in cotton strip decomposition? At the very least, this issue deserves some attention in the discussion.

To address this, we made several revisions to the Methods section to be transparent about our design choices and their implications.

First, we added text clarifying the design tradeoff: "This design prioritized among-reach coverage across the basin (48 sites) and provided point-scale estimates of decomposition potential at a single location within each reach."

Second, we added text noting how deployment locations were chosen within each reach: "At each site, deployment locations were selected to be representative of the reach in terms of sediment size, flow velocity, and substrate composition."

Third, we acknowledged the constraint in larger rivers: "In larger river systems (e.g., 7th-order reaches), deployments were limited to wadeable areas near channel margins."

In the final subsection of the Results and Discussion, we also added text acknowledging that our decomposition estimates represent point-scale conditions and that capturing within-reach spatial variability in decomposition across mesohabitats, substrate types, and channel depths would complement the among-reach approach used here. The new text reads: "We note that our decomposition estimates represent point-scale conditions at wadeable locations and should be interpreted as such rather than as reach-integrated measures of decomposition. Capturing within-reach spatial variability in decomposition across habitats and channel depths would complement the among-reach approach used here, and may lead to stronger relationships between decomposition rates and reach-scale ER."

I note that cotton strip decay rates were weakly correlated with both ER_{tot} (R^2 = 0.29) and ER_{sed} (R^2 = 0.22). Does the slightly stronger correlation with ER_{tot} really justify the key conclusion of the MS that 'decomposition is the result of integrated processes occurring across the sediment-water continuum'? Presumably there is a strong correlation between ER_{tot} and ER_{sed} – is this correlation presented in the MS?

We agree that the difference in R^2 between ER_{tot} (0.29) and ER_{sed} (0.22) is modest. We find the ER_{tot} result noteworthy primarily because it was counter to our stated hypothesis, not because the difference in explained variance is large. We revised the manuscript in several places to reflect this. In the "Respiration in sediments explains little variation in decomposition" subsection, we added: "We note, however, that the difference in explained variance between ER_{tot} (R^2 = 0.29 for K_{cd}; R^2 = 0.16 for K_{dd}) and ER_{sed} (R^2 = 0.22 for K_{cd}; R^2 = 0.13 for K_{dd}) was modest, and the stronger association with ER_{tot} should be interpreted cautiously."

We also tempered language throughout the manuscript to better reflect the strength of the observed relationships. For example, in the Abstract we revised "with little connection to ER_{sed} or ER_{wc}" to "less so with ER_{sed}, and not at all with ER_{wc}. This suggests that point-scale particulate organic-matter-decomposition potential within stream/river sediments is more closely associated with integrated system respiration rather than with processes confined to sediments or the water column alone though these relationships were weak overall."

Regarding the correlation between ER_{tot} and ER_{sed}, while a direct correlation is not presented in our manuscript, Garayburu-Caruso et al. (2025) showed that ER_{sed} contributions to ER_{tot} spanned 0-100% across the YRB, with 88% of locations showing ER_{sed} contributions exceeding 50% of ER_{tot}. This strong correspondence means the two metrics are inherently linked, which further explains why R^2 values for decomposition versus ER_{tot} or ERsed are similar and reinforces the need for cautious interpretation, which we now emphasize throughout the revised manuscript.

Specific comments:

Lines 220-228: I agree with the comment made by Arturo Elosegi that this part of the MS is very speculative. A measure of algal biofilms on the cotton strips may or may not be related to the autotrophic respiration component of ER_{sed}.

We changed the original sentence from : "We propose that if our deployment configuration was complemented with a simultaneous deployment that enabled growth of benthic algal biofilms on the cotton strips, the combined decomposition from both deployments would capture substantially more of the processes that contribute to ER_{sed}."

To : "We propose that if our deployment configuration was complemented with a simultaneous deployment that enabled growth of benthic algal biofilms on the cotton strips, the combined decomposition from both deployments could, in some systems, capture more of the processes that contribute to ER_{sed}."

Line 402: remove '++' from within the title of this reference

We made this change.

Line 570: ...shown WITH solid black lines....

We made this change.

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Reviewer 3:

Stegen et al investigate the relationships between organic matter decomposition and reach-scale respiration. The authors address a relevant topic in freshwater science and the manuscript is well-written. However, I find the study not scientifically sound given strong methodological limitations and data interpretation. The authors hypothesised that organic matter decomposition rates in sediments, calculated from deploying cotton strips in the shallow hyporheic zone, positively correlate with reach-scale sediment respiration (ER_{sed}). Not surprisingly, the authors found a weak correlation between both variables. This is likely due to the deployment method of the the cotton strips (cages), which results in experimental conditions that do not mimic the complexity of the shallow hyporheic zone. Although the authors recognised this limitation in lines 220-225, they wrote the manuscript as if they actually measured organic matter decomposition rates in sediments, which they did not. I recognise the sampling effort but I believe the authors need to reframe the manuscript.

We appreciate this feedback. As noted, the relationships between our measured values of organic-matter decomposition and stream respiration were not strong (R^2 values in the 0.20s), which is often the case with ecological field studies. Despite not being strong, they were significant and showed a pattern opposite to our hypothesis, which we found noteworthy. We also found other notable results including an extremely broad range of decay rates within a small geographic area, impacts stemming from human activities, and a novel and useful approach for assessing microbe-driven decomposition in streambeds using cotton strips in widely available metal cages. We agree that our experimental conditions do not fully mimic the complexity of the shallow hyporheic zone, but our methods do provide a useful proxy for microbially-driven decomposition of particulate organic matter.

We also agree with the comment "they wrote the manuscript as if they actually measured organic matter decomposition rates in sediments, which they did not." It is important to contextualize this. Most decomposition studies (and there have been thousands) measure organic-matter decomposition *potential*, that is, the capacity of the ecosystem to decompose organic matter (e.g., using litter bags). These studies, like ours, do not take into account full system complexity or how much organic matter is actually being decomposed in the ecosystem. This is a widespread semantic problem in the literature and Reviewer 3's point is valid. To address this, we included language throughout the manuscript to clarify that our approach (like thousands of studies before us) quantifies organic-matter decomposition *potential.*

Specific examples of revised text include:

In the Introduction, we added a framing sentence: " Like other standardized substrates used in decomposition studies (e.g., litter bags), cotton-strips quantify

organic-matter-decomposition potential, that is, the capacity of the ecosystem to decompose organic matter, rather than the actual rate of native organic-matter decomposition *in situ*."

In the Abstract, we now state: "This suggests that point-scale particulate organic-matter-decomposition potential within stream/river sediments is more closely associated with integrated system respiration rather than with processes confined to sediments or the water column alone though these relationships were weak overall."

In the final Discussion subsection, the edited text reads: " The implication of our analyses is that point-scale organic-matter decomposition potential was more closely associated with integrated system respiration than with individual respiration components."

Major comments

lines 193-208. The relationships between K_{cd} or K_{dd} and drainage area are rather weak, yet significant. Did the author consider using land use/land cover instead?

Please see Table S1, which reports results from the LASSO analysis. That analysis included land cover variables, which were not selected in the LASSO modeling as being explanatory.

lines 133-135. Please indicate the number of observations where the tensile strength was assumed as 0.05. The authors should provide an strong case for using this approach. There are robust statistical methods to deal with non-detects (in this case, values equal or lower than the lowest tensile strength).

We now include the number of non-detects in the revised manuscript, which was 5.4% of cotton strips. We expanded the rationale in the Methods section for using half the LOD. The revise text reads: "The tensiometer pulled each cotton strip at a rate of 2 cm/min. Some of the cotton strips were completely degraded such that there was no material to measure, while other cages only contained fragments that were too small to measure tensile strength. These non-detect strips accounted for 5.4% of all deployed strips. In both cases, a limit of detection was assigned as the lowest tensile strength calculated in Tiegs et al. (2019) divided by 2, resulting in a final value of 0.05. This was done to avoid statistical artifacts that can arise when simply introducing a value of 0 (i.e., the natural log transformation in Equation 1 would be undefined). Removing these strips entirely would bias the analyses away from conditions with very fast decomposition."

In the Results and Discussion, we incorporated the non-detect information into the existing description of the range of observed values. The revised sentence now reads: "This is evidenced by some cotton strips being completely consumed prior to retrieval (5.4% of all strips, K_{cd} and K_{dd} maximized), while others were largely intact (K_{cd} and K_{dd} ≈ 0)."

lines 262-265. These two studies are not comparable due to methodological differences.

We agree that there are methodological differences, and as noted in the manuscript, we believe that these explain the discrepancy in research outcomes. The cited study allowed access to the organic matter by macroinvertebrates and ours did not. The manuscript directly acknowledges this and uses it to help explain differences in outcomes across the two studies. We prefer to retain this text.

Minor comments

line 19. The sentence is unclear

We revised the sentence for clarity. It now reads: " It is also unclear whether organic-matter-decomposition potential is more closely associated with ER_{sed}, whole-ecosystem respiration (ER_{tot}), or water-column respiration (ER_{wc})."

line 122. Revise the sentence "... and clean 70% ethanol was..."

This sentence was revised and it now reads: "Tubes were capped and rolled approximately 10 times before ethanol was removed. After completing this step, clean 70% ethanol was added to the 50 mL tube to minimize further microbial-based decomposition."