



EGUsphere, referee comment RC1
<https://doi.org/10.5194/egusphere-2022-613-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on egusphere-2022-613

Frederick Colwell (Referee)

Referee comment on "Hyporheic Zone Respiration is Jointly Constrained by Organic Carbon Concentration and Molecular Richness" by James C. Stegen et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-613-RC1>, 2022

Scientific significance:

The research concept is very clearly and simply stated in the Introduction. Hypotheses such as that framed and tested by the authors are challenging to undertake due to the difficulty in obtaining enough samples from a broadly distributed set of sites, using the same methods, in a timely fashion. Often these types of studies accrue data from multiple projects that were never intended to be considered collectively and so they may suffer from bias introduced because disparate research teams never coordinated and may have used different methods. This research is an exception to that norm. This team has accomplished their research by using the WHONDRS program to contemporaneously collect a set of samples, using a common plan, to address their question. The WHONDRS program sets a standard for how to carefully collect samples and corresponding reference data from a team of broadly distributed, motivated, self-selected collaborators and then to follow it up with detailed, systematic sample analysis.

Scientific Quality:

The use of the WHONDRS program's new and extensive database is notable and an exciting consideration of the data collected by the broad community of scientists who are in support of WHONDRS. I appreciate the use of standardized samples (Field vs. Incubation samples) as a way to account for possible heterogeneity in the samples and control for skewed results that might occur due to such heterogeneity. Does this approach mean that their conclusions are contingent on only acquiring samples from settings that are homogeneous? In other words, do they only have confidence that their findings hold whenever samples are strictly homogeneous? Is it possible that using this technique they've removed samples that are naturally heterogeneous and worth including despite

this challenging feature? This might be a consideration in the interpretation of the results.

I also wonder about the variation in extraction efficiency that the authors note in the Methods (pg 4). What is the basis for the assumption that extraction efficiency is not systematically linked to respiration rate? Is it possible that some compounds that are not easily extracted might also not be easily respired, i.e., that the "extractiveness" of a sample corresponds to its biological accessibility (in terms of respiration)? The authors state that this assumption seems to be acceptable because it is extremely unlikely that extraction would be linked to respiration, but is there some evidence for this assumption? Might it be possible that compounds exist that are both especially challenging to extract and challenging for microbes to respire? Perhaps more background to support their assumption would come from organic biogeochemistry studies that have considered the nature of recalcitrant compounds.

This is a relatively high-level view of the processes associated with the respiration of organic carbon in the hyporheic zone. Regarding a more detailed inspection of the data, a couple things come to mind and these might be helpful to point out in the discussion. Recognizing that the study was focused on trends that might be evident at the continental scale and accordingly required a collection of samples from a geographically vast area, it seems that there are some sample types that were not considered in the broad sampling effort. Presumably, this is because collaborators could not be recruited from these areas to collect samples. This might mean that certain watershed types as defined by regional climatic conditions, vegetation type, edaphic quality, regolith, underlying geology, stream gradient, etc. would have been under-represented in the dataset. From the map, examples of missing areas seem to be rivers located in the upper Plains, the Basin and Range Province, and on the Pacific coast. I cannot say that any such missing watersheds or river systems are critical to their story, but omission of these regions in this study suggest that they should be included in future studies.

A tangential question: Are there systems other than marine and river corridors (as referenced at the bottom of pg 6) for which OM diversity and microbial respiration may have been considered? Could soils and marine sediments be added to their list and considered in this regard and if so, then could they also be interesting reference points, or distinctive contrasts for this study performed on samples from hyporheic zones? Conceivably, because of the way the WHONDRS work is conducted, this paper may be something of a landmark in having studied such a broad sweep of sample locations and might be used for future comparisons of microbially dominated ecosystems.

The Introduction includes reference to the importance of studying respiration in hyporheic zones. I think the Discussion could be improved by returning to this point and considering the how the findings may impact critical processes occurring in these riverine settings (i.e., where development or survival of larval/juvenile stages of fish species or aquatic invertebrates is fostered, where contaminant degradation occurs, where cold water refugia become important as rivers warm). The authors consider this at the end (lines 240-247) ; however, I think something more about the implications of the paper findings would be helpful to include here and would underline the importance of the work to readers.

Presentation Quality:

This paper is a concise, straightforward, and articulate test of a well-stated hypothesis using a large and unique dataset. As a high-level view of the observed relationships between microbial respiration and OM diversity or OC concentration the paper succeeds in presenting the information. The figures are all appropriate for explaining their observations.

I suggest that the authors acknowledge the time and care taken by numerous scientists who sampled rivers and then contributed the hundreds of samples that were subsequently analyzed by the WHONDRS program. I'm certain that the original WHONDRS paper does so; however, it seems appropriate to have such a statement in all of the papers using data from this program.