EGUsphere, referee comment RC2
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Comment on egusphere-2022-373

Anonymous Referee #2

Referee comment on "High-resolution vertical biogeochemical profiles in the hyporheic zone reveal insights into microbial methane cycling" by Tamara Michaelis et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-373-RC2, 2022

general comments

The manuscripts describes the results from 5 porewater peepers deployed in a small stream at different times and dates. From porewater profiles of different solutes the authors extract information about methane producing and consuming processes in the stream sediment. Results are supported by a molecular biological analysis of one of the five sites. The topic is interesting, innovative, and suitable for the journal. The paper is well written, methods seem to be carried out with great care (although I cannot judge the molecular methods). Major problems with the paper are related to the methods which make linking results and interpretation sometimes problematic or not possible:

1. Peepers integrate over long periods. I am not fully convinced that using them in a highly dynamic habitat is the best choice. Also the interpretation of the profiles relies on steady state assumptions. In order to judge this it is necessary to have more information about temporal dynamics in that stream. I suggest to improve Figure 1 by showing discharge data with high temporal resolution (at least daily means) and to indicate peeper deployment periods in the figure. That enables judgement whether e.g. a flood occurred shortly before peeper retreaval. I think only stable conditions during peeper deployment allow the presented interpretation of the profiles.

2. The spatial resolution of the profiles is often not sufficient to allow the resolution of different biogeochemical zones

3. The study mixes spatial and temporal variability because peepers were deployed at different sides not simultaneously. As a result the study gives very limited information regarding both spatial and temporal differences between peepers. The most striking result from the study is probably that all peepers were unique. We cannot tell which part spatial and temporal factors play but I think this has consequences for other studies: General

conclusions about stream functioning and upscaling from such single spot data is simply not possible. One could guess that having 5 more peepers would have resulted in 5 more very unique datasets. I recommend to discuss the issue of variability more.

4. They applied a 2D model (PROFILE) to a 3D scenario. This means any deviation from what was expected could be attributed to horizontal heterogeneity, transport inhomogeneity etc.. That makes interpretation of the profiles with respect to vertical reaction profiles highly subjective. Who decides in which case a feature of the profile is due to vertical biogeochemical processes or rather an artefact caused e.g. by transport inhomogeneity?

detailed comments

L.27: The abstract should end with a summarising/concluding sentence.

L.36: Which % of natural sources are streams?

L.47: One reference would be enough

L.46-52: It is not really clear why this is relevant for the study

Introduction: There is lots of introduction about microbes but it is not really clear why. There are also lots of microbial references. I suggest to tailor the introduction more towards the aims and questions.

L.86: What were the findings of that study?

L.98: campaigns

L.112: Does that mean faster flow with macrophytes?

L.118: That information cannot be seen in Figure 1. Give discharge data with high temporal resolution.

L.120: How wide was the stream? Water depth?

Figure 2: What are the two objects at the water surface? What are the 2 vertical lines separating the figure?

L.148: What was the orientation of the peepers relative to flow direction? Wan´t there sediment erosion near to the peepers after deployment, because the peepers generate turbulence in flowing water.

L.151-152: Be more specific. Why were 2 weeks not enough? How do you know?

L.161: Give type ans size of vials. That means there was no water (except the 10µl NaOH) in the vials? There must be some small loss of sample gas using the described method. Did you check artefacts e.g. by preparing samples with known CH4 content?

L.211: I do not understand the boundary conditions chosen for CH4. Zero flux at top or bottom? Why can you assume that? Why not using concentration at the top and bottom as boundary conditions?

Results and discussion: I am not sure whether joining results and discussion are the best choice here. Jumping permanently between results and discussion is difficult for the reader. If a large revision is done I recommend to separate results and discussion. Use always past tense for results (e.g. L 252: depended).

L.256: Figure 3a and c.

L.275: What is the detection limit of the O2 measurements and are <10 significantly different from zero?

L.314: Information on sediment composition would help a lot. Don´t you have e.g. LOI data for table A3?

L.324: "production" or rather "concentration"?

L.328: Why “seem”. It should be possible to calculate CH4 partial pressure and compare with hydrostatic pressure.

L.333: “by”

L.334: Can you show the correlation between CH4 and NH3, e.g. in the supplement?

L.380: So what? How is this sentence related to your study?

L391: delete “measured”

L.396: ad a reference for this statement.

L.408-409: Difficult to understand

L.412: Why can you conclude that CH4 oxidation was not relevant at site D?

L.423: There is a problem of logic: Diffusion is a transport process and cannot reduce a concentration in the profile. If CH4 dissapears you need a CH4 consumption process.

L.429: Unknown? Is there really no literature about CH4 ebullition in streams?

L.438-439: Of course because that is what the PROFILE software is doing: Interpreting changes in slope as production/consumption processes.

L.452: This is a dangerous argument. The model is a quantitative one and give concrete numbers. How can you judge which numbers you trust and which not? This argumentation may question the entire quantitative interpretation of your profiles.

L.462: These O2 fluxes look extremely low. I would guess that the spatial resolution of the profiles was either not sufficient to model proper O2 fluxes or that assumption about transport coefficients were not met.

Fig.6: Is it possible to compare different groups quantitatively? It is striking that there were more methanotrophs than methanogens and that there were much more SRB. This brings also up the idea whether it makes sense to compare sulfate reduction and methane production rates from the PROFILE analysis to get information about the contribution of the different processes to total organic matter mineralisation.

L.542: The molecular analysis also integrates over a larger timescale. Without having information about short term dynamics of e.g. redox conditions it is difficult to interpret the findings.

L554: Delete “can”

equation C3: Explain symbols

refs: 112 references are a lot. I suggest to critically check the necessity of all reference. There is potential for shortening esp. in the introduction. On the other hand I wonder if at least some discussion of temporal dynamics (e.g. the work of https://www.ufz.de/index.php?en=38353) might be helpful for interpretation of the data.