Reply to reviewer 3

Comments to the manuscript by Koschorreck et al, “Diurnal versus spatial variability of greenhouse gas emissions from an anthropogenic modified German lowland river”

Overview:

In this study the authors investigate the temporal and spatial variability in greenhouse gas emissions along a German lowland river that has been heavily impacted by anthropogenic activities. Specific focus is on how different greenhouse gases (CO2 and CH4) differ in their respective variability between different locations along a 1 km river reach and over a diurnal temporal scale. The study is based on a three to four days (dependent on variable) long measurement campaign where different locations (middle and side of river channel, different habitats at the terrestrial-aquatic interface) within the river are monitored. The authors conclude that the variability in aquatic emission is gas-specific and that CH4 are more variable on a spatial scale than CO2, whereas CO2 is more variable on the diurnal scale than CH4. They further show that the non-aquatic parts of the river (i.e. parts that are temporally flooded but that were not flooded during the measurement campaign) contributed 10% of the total GHG flux during the campaign.

The manuscript focus on an important topic that is very suitable for publication in Biogeosciences. Although I do not fully agree with the authors that large rivers are much more heterogeneous in their spatial variability of GHG emissions than small streams, I agree that large rivers are somewhat less studied. I also agree that, due to the relative lack of data, GHG emissions from large rivers are often estimated without any validation. Although small streams (< stream order 4) dominate the total global stream and river length, the water surface area that larger rivers (> stream order 4) are representing is a large share of the total surface area of running water. Hence, understanding the temporal and spatial variability and what controls these variabilities are essential for representative GHG emission estimates.

General comments:

With this background the manuscript is an important contribution to the research field. I appreciate the detailed and small scale focus of the study which highlight fundamental differences in how CO2 and CH4 are sourced from a central Europe (and likely also elsewhere?) common type of river environment. The manuscript is in general well written and presented but I have some points that needs to be clarified/added prior to a publication of the study. 1) I have some problems to follow the method section and the structure of it. There are also some unclear parts of the methods which at least to me is confusing and which makes it a bit hard to interpret the results.

We will revise the method section

2) The study is based on a variety of different measurement approaches and setups for capturing GHG fluxes at different habitats/scales. It is currently hard to assess the uncertainty of each approach which makes it hard to understand their absolute or relative difference when measurements are compared.

We agree. We will add a table to the methods showing what was measured with which method including method uncertainties.

3) The conclusion section of the study could be stronger given the high detail of the measurements. Currently the text is rather vaguely written and not fully representing, or capitalizing on, the outcome of the study.

The reviewers are a bit contradictory here. On the one hand we are asked to be careful with generalizing from this case-study. But we are also asked to be stronger with our conclusions. We tend to follow the advice to be careful with generalization. However, we will critically assess our conclusions and will formulate them a bit more explicit.

Detailed comments:

Ln 15-16, Although not clear but I interpret this sentence starting with "Here quantification…" that small streams (which are mentioned in the sentence before) are not displaying spatial and temporal variability in their GHG emissions. If this is what the authors mean I strongly disagree. I would claim that the spatial and temporal variability in GHG emissions could be even more pronounced in small streams. I however certainly agree with the authors that small scale assessments of temporal and spatial variabilities are rarely made in larger rivers and even less simultaneously. I suggest that the authors rephrase this sentence.

It is actually an interesting question how to compare spatial variability in systems of different size. It was not our intention to address this question and we will rephrase as suggested.

Ln 17, It later on says in the text that the campaign lasted for four days. I suggest that the authors keep it consistent although I think I understand that the difference stems from that different variables/habitats were measured during different number of days.

This point was also raised by another reviewer. We will check the manuscript for consistency.

Ln 24-25, yes the data confirms the hypothesis, but I think it needs to be transparently stated that this was just true for this river section and during the three or four days long campaign. Whether this is a more universal pattern requires measurements covering more extensive temporal and spatial scales.

True – point will be added to the discussion and conclusion.

Ln 29-31, I think the authors can update the CH4 referencing, here and elsewhere suitable, with the very relevant and recent global stream and river studies (Rocher-Ros et al. 2023; Stanley et al. 2023).

We will definetly refer to Rocher-Ros et al. and will check were it makes sense to cite Stanley et al 2023.

Ln 53-54, I think this motivation statement (comparison with small streams) to why it is necessary to study small scale variability in large rivers is not really true and also not really needed. I have measured very high small scale variability along stream channels in both concentrations and emissions of different GHG´s (especially for CH4). In comparison, I have also measured low spatial variability in GHG´s across larger rivers. To conclude, whether concentrations and emissions are spatially variable are highly site specific and not necessary

related to the size of the water body. I agree though that little is known about spatial (and temporal) variability in larger rivers, a good motivation of the study as such. I suggest a rephrasing of this statement.

Thanks for this suggestion. This refocusing of the objectives will improve the story.

Ln 80, in abstract “three days’ campaign”. I suggest to be consistent.

We will check the manuscript for consistency.

Figure 2, what is meant with “mean low discharge”? Unclear to me.

It is the mean summer discharge during the last 15 years. Will be specified in the revision.

Ln 107, section header. This section contains more than just flow velocity and depth measurements, I suggest to give a more suitable header.

Header will be changed to “Hydrodynamics and basic physicochemical measurements”

Ln 111-113, These velocity measurements are more suitable for the result section to me, or why are they place in the methods?

True. We will move the results of the velocity measurements to the results section.

Ln 117-119, “The water supply for both sensors was the moon pool of the Albis”. This comes without any introduction to the reader, what is moon pool and what is Albis?? Please clarify!

Will be rephrased and explained.

Ln 122-147, This section is a bit unclear to me, and at the same time the core of many of the measurements included. I suggest that the authors go through it and make a more logical structure of the text. For example:

- Are the same spatial measurements described starting in the lines 123 and 139? If so, why are they separated in the text? If not, what is the difference between them?

Floating chamber measurements as well as CO_2 concentration measurements were completely independently done from CH_4 measurements using the degasser. We will add a table to make more clear what was measured with which method and when and were.

- The dissolved gas mapping described in ln 130, why was it just done for CH4 and not also for CO2? I though the LGR instrument handled both gases?

Yes – the LGR can handle both gases. The setup in combination with the gas-equilibrator however, was never been tested for CO_2. Thus, we prefered not to use those CO2 data but used discreet water samples analysed by GC for CO_2. We will re-assess our raw data and check it the CO_2 data of the LGR instrument can be trusted and used for the manuscript.

- The conversion of ppm values of CH4 to concentrations determined from the mapping was made by a regression equation I assume? I suggest to show the data for this conversion in the SI

We will add to the methods section: “The range of concentrations from the water samples used for calibration was rather narrow (178 – 258 nmol/L), thus we used a conversion factor (water sample conc. / ppm from GGA) which was 88.7 ± 23 nM / ppm).”

- Ln 144, again, what is the moon pool of Albis? Maybe obvious for a “ship-based” researcher but not for me.

We will replace “moon pool” by "ship's duct with direct water supply".

- Ln 145-147, how was the CH4 ppm values from the Contros converted to absolute concentrations, similar to above I suggest to show this conversion in some way. As the use of CH4 sensors are in the forefront for this kind of research it would be highly useful for other researcher to show how this was done.

We will add to the methods section: “The relative values of the Contros sensor were converted to concentrations by relating them to water samples measured with a GC, similar to the values from the GGA (LosGatos). The conversion factor here was 0.06 µmol/ppm.”

Ln 148, this section contains more than “terrestrial measurements”. The first part of the section describes for example atmospheric measurements. I suggest the authors give a more suitable section header.

We will change the header to “Terrestrial and atmospheric measurements”.

Ln 185, what probe measurements? The ones conducted at the ship? Please clarify!

We are refering here to the measurments shown in figure 3, performed with the LosGatos on the inflatable boat. We will change the "probe measurements" to "measurements with the transportable GGA (LosGatos)"

Ln 186, why was a fixed value of k600 (5.5 m d-1) used? I don’t see 5.5 in Table 1 as referred to. Also, what habitat do these fluxes represent?

We are sorry – that was caused by wrong number in table 1 (see our reply to reviewer 1). The actual k600 for the side habitat (were the probe was installed) was 5.2 m d^{-1}. As explained in the answer to the other reviewer, hydrodynamics and wind did not change much during our study. Data in Figure 5b will be corrected accordingly.

Ln 213, I believe it should be “at the sides” instead of “at the sites”? or?

Yes.

Figure 3, why was not CO2 measured at the same time?

See our reply above.

Ln 243, “were” instead of “are”.

Yes.

Figure 5, I assume the time point 12.00 on the x-axis refer to mid-day, I suggest to clarify this.

We think it is clear since night is indicated by the grey shaded area.

Table 2 and related text. Here comes one of my larger concerns. It is currently hard to assess how the different method approaches correspond to each other. Although a range is given for all emissions it is hard to understand with what certainty each individual emission determination (done with a different measurement approach) is made with. Some kind of uncertainty estimate would certainly help the reader with this.

We completely agree. In the revision we will estimate the uncertainties of the different approaches and add this information as a table in the methods section.

Figure 6, similar to above, the CV values are good and illustrate well the variability associated with each gas, variability and habitat. However, to what extent these differences in CV values are dependent on variable certainties in the different methods involved in measuring these emissions is currently unknown for the reader.

Same reply as above

Ln 320, The sentence that starts with “The floating chamber….” Used for what? Do you mean GHG emission measurements? What about the eddy covariance method? There are a few examples of the application of the EC method on rivers e.g. Huotari et al. 2013, Guseva et al. 2021. I suggest to rephrase this sentence.

This point was also raised by another reviewer. We will rephrase it and add a sentence about EC measurements and the problem of their footprint in rather small systems.

Ln 390, here refered to as “stream”, in other places “river”, I suggest to be consistent and use river. Here and elsewhere in this section specifically, but also throughout the ms.

We agree and will change this.

Ln 399, “logyrhymtic scale”??

Will be corrected

Ln 405 and onwards, I appreciate this exercise of simulating different monitoring approaches and what you might miss/capture with a certain approach. This is good information for the reader and it also put some more perspectives on the detailed data basis that this study provides.

Thanks

Ln 426, I suggest the authors rewrite the conclusions to make them stronger and better reflecting the outcome of the study. I appreciate the starting sentence that the study “just” represent a snapshot of one river reach, still I think the conclusions could be more concise, focused and with a stronger message. The study deserves that.

See our reply above. We will critically assess our conclusions and will formulate them a bit more explicit.

References

Guseva, S., Aurela, M., Cortés, A., Kivi, R., Lotsari, E., MacIntyre, S., et al. (2021). Variable Physical Drivers of Near-Surface Turbulence in a Regulated River. *Water Resources Research*, *57*(11), e2020WR027939.

Huotari, J., Haapanala, S., Pumpanen, J., Vesala, T., & Ojala, A. (2013). Efficient gas exchange between a boreal river and the atmosphere.Geophysical Research Letters, *40*(21), 2013GL057705.

Rocher-Ros, G., Stanley, E.H., Loken, L.C. et al. Global methane emissions from rivers and streams. Nature 621, 530–535 (2023). https://doi.org/10.1038/s41586-023-06344-6

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