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Interactive comment on "Nitrous oxide (N_2O) and methane (CH_4) in rivers and estuaries of northwestern Borneo" *by* Hermann W. Bange et al.

Anonymous Referee #1

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The authors report a very valuable data-set of dissolved CH4 and N2O concentrations obtained in several estuaries in Borneo. I have a few minor suggestions for improvement/clarification listed below.

L 39 : Please provide ranges of pH, O2 and DOC. "very high/low" is vague.

L 77 : I suggest replacing "release" by "exchange", since the direction of the flux is not necessarily always to the atmosphere, as shown here by frequent N2O depletion in some rivers.

L 96 : CH4 is also oxidized aerobically in freshwater sediments, in rivers (Kelley et al. 1995) and lakes (Frenzel et al. 1990).

L 111: The number of references seems excessive to back a simple statement on the

occurrence of black water rivers in SE Asia.

L 142: Please specify how was the water collected for the CH4/N2O samples? Niskin bottle?

L152: Please provide the values of standards for N2O/CH4. Authors state that their standards were calibrated against NOAA standards, but NOAA standards have usually very low CH4/N2O values (close to atmospheric equilibrium), but given the reported concentrations, the measured pCH4 and pN2O should have strongly deviated from atmospheric equilibrium, unless the gas samples were diluted (in which case this needs to be specified).

L 166: Please specify how was pH measured.

L 171: Did you check if there was an interference of HgCl2 on NH4+ samples ? Based on personal experience HgCl2 strongly modifies NH4+ samples for colorimetric measurements.

L 247: Over-saturation of N2O of 12,480% was reported in an agriculture impacted small stream of the Meuse Basin (Borges et al. 2018).

L 256-273: The authors develop the idea that N2O production did not occur in black water rivers due to low pH values because of the protonation of NH3 and the pH-dependent reduction of nitrification and denitrification. Consequently, the authors conclude N2O production occurred in soils, and that N2O was subsequently transferred to the river. However, peat soils themselves are also very acid, so the same reasoning of inhibition of N2O production should also apply to soils. So, why should low pH inhibit N2O production in river water but not in soils?

L 256-273: The experiments of Le et al. (2019) showed that nitrification was strongly inhibited but still occurred until pH 5.3, and was totally inhibited at pH 5.0. Since N2O is produced as a by-product of nitrification, it is possible that the N2O yield increases with decreasing pH (the same way that N2O yield from nitrification increases with decreasing O2)? Even if this is not the case, the fact that nitrification is inhibited by pH but still occurs down to pH 5.3 still allows the possibility of N2O production occurring in river water in the sampled sites. So there could still be a case for N2O being produced in black-water rivers.

L 256-273: While the lowest values of pH in the ranges reported in Table 1 are clearly lower than 5.0 (the value at which nitrification was undetectable in the experiments of Le et al. (2019)), I cannot figure out how many observations of high N2O coincided with pH<5.0. It might be useful for the discussion to plot N2O versus pH to show the reader how many data point of high N2O occur at pH > and < 5.0.

L 315: Higher discharge/rain also leads to enhanced gas transfer velocities and loss of CH4 to the atmosphere. Higher discharge/rain also leads to decreased residence time of water (flushing of water), which will decrease the accumulation of CH4 in the water (even if sources such as sediment flux remain the same). Higher rain (surface runoff) also leads to simple dilution of all solutes (including CH4).

L 316: A negative relation between CH4 and discharge is not necessarily a general rule. Teodoru et al. (2015) reported higher CH4 in the Zambezi River during high-waters and lower CH4 during low-waters due to variable connectivity with floodplains. At a fixed station in the upper Congo, Borges et al. (2019) showed that the CH4 seasonal evolution roughly follows the one of discharge. So in both studies a positive relation between CH4 and discharge was reported.

L 318: Most of the low- vs high-water comparisons of MOX and CH4 given by Sawakuchi et al. are for white water and clear water rivers, and only for one black river at a single station (Negro). I'm not sure this is sufficient to derive a general rule on methane oxidation in black water rivers.

Further, methane oxidation is a first order process, so should be lower when CH4 concentrations are lower, so, it's unlikely that CH4 oxidation is higher when CH4 concentrations are lower, as stated by the authors.

L 363: I suggest replacing "results" by resulted

Please explain how were the "average" flux calculated. It's unclear how the "average" flux intensities and integrated fluxes were derived to take into account the estuarine geometry. Estuaries are generally wider at the mouth than upstream ("funnel shaped"). So even if high salinity regions show lower flux intensities, their relative contribution to total flux will have more weight (relative larger surface area). To put it in other words a simple average of all of the data points will lead to an over-estimation of the flux intensities because the average will be biased towards low salinity values that in reality correspond to a lower surface of estuary. So, each data point needs to be weighted by a corresponding surface area (section of the estuary), and the average should be surface weighted. This requires a little bit of GIS but is feasible (even with Google Earth).

Figures 2 & 3: it could be useful to add in plots a legend of the symbols.

References

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