We thank Ref#1 for the comments which helped to improve the manuscript significantly.

Anonymous Referee #1

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.

Reply (R): The ranges were added.

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.

R: We agree: ‘release’ was replaced with ‘exchange’.

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

R: We added the missing information about aerobic CH4 oxidation in river sediments. However, we do not see a need to refer to studies of lake ecosystems. Moreover, aerobic CH4 oxidation in the river water is already mentioned in the next sentence.

L 111: The number of references seems excessive to back a simple statement on the occurrence of black water rivers in SE Asia.

R: We agree. The number of references has been reduced to three: ‘Alkhatib et al., 2007; Martin et al., 2018; Moore et al., 2011’.

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

R: S***amples were collected at 1 m depth using a Niskin sampler. We added this information to the text.***

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).

R: We added the range of mole fractions of the used standard gas mixtures. These standards have been calibrated against certified NOAA gas standards in the laboratory at the MPI for Biogeochemistry in Jena, Germany. Unfortunately, the values of the primary gas standards are not known to us.

L 166: Please specify how was pH measured.

R: We added this information.

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.

R: The indophenol blue method used here works well with low concentration of Hg. (We only added a tiny amount of HgCl2 solution (2-3 drops) into each bottle.) The precision of our method was frequently better than +/- 3%.

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).

R: Thank you for pointing this out. We modified the text. We are now citing '(Borges et al., 2018)' instead of '(Barnes and Upstill-Goddard, 2011)'.

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?

R: On the one hand, tropical soils indeed can have pH <4 and thus net N2O production should be low as well when adapting our line of arguments for rivers. On the other hand it is well known that significant N2O (peat) soil production occurs (mainly via denitrification) when the water table is high/the WFPS (water filled pore space) is 100% (Pihlatie et al., 2004; Regina et al., 1996). This is not necessarily a contradiction since the microbial community in tropical soils is probably very different to the one found in the rivers. Moreover, N2O production by denitrification seems to be generally less sensitive against low pH (see Blum et al. 2018).

Blum et al., The pH dependency of N-converting enzymatic processes, pathways and microbes: effect on net N2O production, Environmental Microbiology, 20, 1623-1640, 2018.

Pihlatie et al., Contribution of nitrification and denitrification to N2O production in peat, clay and loamy sand soils under different soil moisture conditions, Nutrient Cycling in Agroecosystems, 70, 135-141, 2004.

Regina et al., Fluxes of nitrous oxide from boreal peatlands as affected by peatland type, water table level and nitrification capacity, Biogeochemistry, 35, 401-418, 1996.

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.

R: Thank you for pointing this out. We agree and thus replaced 'unlikely' with 'low' which indeed better reflects a potential of N2O production in river waters at low pH.

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.

R: We added a new Figure 3 which shows N2O vs pH. We added 'Figure 3 shows the N2O concentrations along the pH gradients. Obviously there are no trends except for an enhancement of the N2O concentrations in September 2017.'

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).

R: Thank you for pointing this out. We modified the text which now reads: 'This relationship can be explained by an interplay of various processes such as: (i) decrease of CH4 concentrations caused by a higher water flow (i.e. dilution under the assumption that the net CH4 production does not change significantly), (ii) higher flux across the river/atmosphere interface during periods of higher discharge (caused by an enlarged river surface area and/or a more turbulent water flow) (Alin et al., 2011; Borges and Abril, 2011) and (iii) the enhancement of CH4 oxidation […].'

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.

R: We did not state that the negative CH4/discharge relationship is a general rule. Indeed we wrote* '(i) the often observed inverse relationship […]' *which clearly implies that it is not a general rule. So we do not see a need to revise the text at this point.

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.

R: Indeed, Sawakuchi et al. is cited erroneously by the reviewer: Sawakuchi et al. report MOx rates and CH4 isotopic signatures from four stations in two black rivers (three stations were located in the Rio Negro and one was located in the Rio Preto, see e.g. Tables 3 and 4 in Sawakuchi et al.). MOx rates were measured in the Rio Negro during both high and low water season and MOx was measured during high water in the Rio Preto. Moreover, Sawakuchi et al. concluded that 'the relative amount of MOx was maximal during high water in black and white water rivers and minimal in clear water rivers during low water'. Therefore, we have good reasons to follow the line of arguments by Sawakuchi et al. . (We agree, however, with the reviewer that more studies on this issue are needed.)

L 363: I suggest replacing "results" by resulted

R: We agree: 'results' was replaced with '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).

R: We agree with the reviewer that a surface area-weighted estimate would give a more realistic ‘picture’ of the riverine emission estimates. However, since it was not possible to cover the entire salinity gradient during some of the sampling campaigns, an adequate surface area-weighted emission estimate is not possible for most of the rivers/estuaries sampled. Moreover, it seems reasonable to say that the uncertainty introduced by the poor seasonal/interannual coverage is much higher than the uncertainty introduced by the inadequate coverage of the salinity gradients (and thus the inadequate areal extrapolation).

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

R: The legends were added.