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Interactive comment on "Effects of land use and water quality on greenhouse gas emissions from an urban river system" *by* Long Ho et al.

Anonymous Referee #2

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General comment: Ho and colleagues present a study on greenhouse gas emissions from a small urban river system in Ecuador. They evaluated the effects of water quality and land use types on the magnitude of GHG emission rates. Two WQIs were used to determine the water quality status and finally the random forest model was applied to identify the primary drivers for each of the three GHGs. Although land use and wastewater discharge have been widely recognized to impact GHG concentration and emissions worldwide, it has been seldom examined within a river system which encompass both human disturbances. Pollution of inland waters has become a global issue and how it has affected GHG emissions remains largely unknown. This study provides a timely investigation into this research question.

Major comments: 1) Sampling was conducted only once in late September, soon after

the start of the rainy season. I am not implying that this sampling strategy is wrong, but it is risky to use this 5-d sampling results to estimate the annual GHG emission flux. Considering the seasonality in hydrology, the calculated annual fluxes might have high uncertainties and don't really reflect the actual annual fluxes (and seasonal variations). Because of the focus of this study is to examine the impacts of land use types and water quality categories on GHG emissions, I would recommend the authors remove the calculation of the annual GHG emission fluxes and related discussion from the text. Please also refer to my other comments on sampling in the specific comments below.

2) Because estimation of the GHG flux involves dissolved GHG concentration and the gas transfer velocity across the water-air interface. For flowing rivers, the gas transfer velocity is more affected by flow velocity rather than by wind speed. The authors need to think about the suitability of using wind as a proxy for estimating the turbulence. Also, compared with emission flux, personally I think the dissolved GHG concentrations would be more appropriate as an indicator to examine the effects of land use and water quality.

Specific comments (with line number, L): L35: The rivers themselves are not the major sources of GHGs. Instead, a large proportion of the GHGs is derived from terrestrial ecosystems. L37: This flux has been updated. See Drake et al., 2018. Limnology and Oceanography Letter, 3, 132-142. L51/52: Given the spatial heterogeneity of hydrology, geomorphology, climate, etc, it is natural to see a strong spatial variation of GHG emissions. This has been widely observed worldwide. L62: There are a number of WQIs available for water quality assessment (see Zotou et al., 2019. Environ Monit Assess, 505), I am not clear how the authors chose these two indices for this comparison. It seems the Canadian Council of Ministers of Environment (CCME) index is more appropriate and common in describing water quality. L74/75: Over what period are these mean climate characteristics calculated? Please specify. L78: Sum of the fiver tributaries is not equal to the total drainage area of the basin (223 km2). L83: I don't know how the sampling during the daytime can ensure the investigation of temporal effects. First, the sampling was only conducted during the daytime, thus the temporal effect of diel cycle cannot be detected. Second, the sampling was performed once only (soon after the start of the second rainy season of a year). Thus, the temporal effect of seasonal variation cannot be detected either. L101: how wide are the rivers? L120: Using the headspace equilibrium method to measure pCO2 with no consideration of alkalinity/DIC concentration is prone to errors and may lead to gross biases in the finally calculated pCO2 results. The authors can take a look on this recent open discussion (https://bg.copernicus.org/preprints/bg-2020-307/) and, if necessary, correct your calculations after headspace equilibration. L157: To calculate the total emissions of each river tributary, shouldn't it be based on the total stream surface area? I don't know how the total emission flux was calculated on the basis of total watershed area. If it is the total stream surface area used in the calculations, the authors need to elaborate on the details on the stream surface area estimation. L165: Holgerson and Raymond (2016) only looked at lakes (reservoirs), not at rivers and streams. Thus the results here are not directly comparable to Holgerson and Raymond (2016). L171: the authors need to justify why these two WQIs were used in this study. A brief justification will suffice. L222-225: how were these relative contributions calculated? Because the five tributary catchments have different catchment sizes, it is reasonable to observe a large contribution from the Tomebamba tributary because of its large catchment size. I don't think such comparison makes sense as it is not normalized by catchment area. In need, because almost all the sampling sites are located downstream near the catchment outlet, the calculated GHG emission fluxes are site-specific and don't really reflect the spatial variability of GHG emissions across the five tributaries or across the whole study area. L225-227: following my comment above, how was the spatial variation determined? By comparing the sites within each tributary catchment? This spatial variation is reasonably clear as the sites are located within different land use landscapes or with/without wastewater inputs. L230: The skewness was clearly caused the extremely high values. I am not sure if you should remove these outliers for this plotting. If the outliers are removed, the arithmetic means will be much lower. L263: this is a very good obser-

vation. Does the dissolved GHG concentration also show a similar variation by water quality level? Also, for the DO, does it also show a similar variation? Because GHG emission reflects the combined effect of dissolved GHG concentration and gas transfer velocity, it will be more meaningful to show the change of dissolved GHG centrations which are perhaps better indicative of the water quality status. L274: again, the cited values from Holgerson and Raymond, 2016 are for lakes only, not for rivers. L275: To estimate the global average GWP, perhaps you need to know the relative abundance of streams/rivers/lakes/reservoirs/wetlands. L291-303: Move these descriptions to the section 'study area'? L308: for the effect of land-use types on GHG emissions, I agree with the authors that land use will have diverse impacts on GHGH concentration and emissions. However, most of the sampling sites (Fig 1) are nested within the catchment. This suggests that the observed GHG emissions at these sites are not necessarily affected or controlled by only one single land use type. For example, the sites in the downstream may have been simultaneously affected by urban, agriculture and nature. If without an accurate quantification of their relative contributions, it is problematic to compare the impacts of different land use types on GHG emissions as shown in Fig 4. This is also related to the results in Table 2. Are these GWPs solely governed by one land use type? L368-370: how have thee factors affected the variation of the GHG emissions? Any evidence? L390: again, for these emission rates expressed as Gg CO2 yr-1, I don't think it is reliable as expected. The sampling was not spatially and temporal resolved enough for an annual-scale estimation. L428: what does the 'later' refer to? I believe it is 'nature'. Fig 1. How did the authors differentiate rivers from streams? By Strahler order?

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