

## ***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 #1**

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The manuscript entitled “Effects of land use and water quality on greenhouse gas emissions from an urban river system” provides data on GHG emissions from aquatic systems in a watershed located in Ecuador and investigates the link between water quality, adjacent land cover types and the magnitude of GHG emissions. The manuscript brings the importance of considering water quality on the estimates of the total GHG emissions from aquatic systems in addition to considering the total area only. This is a promising approach. However, there are many technical problems that need to be addressed. The estimation of gas transfer velocity ( $k$ ) is largely discussed in the literature, and  $k$  estimates from empirical models should be used with caution. One major technical problem is that the gas fluxes were estimated using  $k_{600}$  parameterized as a function of wind speed, which is valid for open water systems, such as reservoirs, lakes

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and oceans, but not recommended for rivers and streams. A flow-velocity- or water-depth-based model to estimate  $k_{600}$  should be considered as an alternative, and the associated uncertainties should be addressed. I suggest the authors to consider a recent paper published in the Biogeosciences by Li et al. 2019 and the cited papers to better estimate  $k$ . Additionally, the annual emissions were estimated using only data from 17/09/2018 to 21/09/2018 (5 days). This does not seem acceptable to me. There is no information about the number of samples per sampling site or any other information that justifies such extrapolation. In the manuscript, the authors mistakenly seem to use the Global Warming Potential (GWP) concept in the Results and Discussion section. Then, it is difficult to evaluate how authors estimated the emission of GHG in CO<sub>2</sub>-eq. In summary, the paper needs improvements on the method section, on the  $k$  estimates and the described data does not support most of the interpretations and conclusions. Personally, I support studies and papers that show results from tropical systems because of the relatively low available data and information. I encourage the authors to revise the paper with caution, but, unfortunately, in this form, the manuscript is not suitable for Biogeosciences.

Specific comments and suggestions are addressed below.

Ln 15 - Specify here that these indexes are water quality indexes.

Ln 23 - What authors mean with "nature sites"? Do you mean "sites close to forested areas"?

Ln 49-51 - The estimation of  $k_{600}$  is also a large challenge to estimate GHG emissions from aquatic systems. You should discuss  $k$  in the manuscript.

Ln 71 - The authors mentioned that the study area is 223 km<sup>2</sup> but this value is less than the sum of the area of the studied sub-basins added in Ln 79. Please, verify, clarify or specify the study area.

Ln 81 - All data in the manuscript were collected in five days (from 17/09/2018 to

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21/09/2018). Is it representative to discuss temporal variation? I agree that authors can evaluate temporal variation in a day scale approach. However, data from five consecutive days do not represent the annual variability and are not enough to estimate annual emissions of GHG.

Ln 83 - This sentence is not clear and there are many assumptions in the same sentence. Why do you assume that covering only daylight will ensure the investigation of temporal effects? Additionally, the connection between oxygen and GHG emission is not as simple as you stated in this sentence. Please, rephrase or remove this sentence.

Ln 90 - How many samples per sampling sites?? Were samples collected every day in each sampling site? Ln 95 - Please, change "Hack kit" to "Hach kit" in the Supplementary Material S1.

Ln 96 - Land use is one of the main subjects of the manuscript and it is also in the paper title. I suggest adding a subsection in the methods (as you did with the water quality) specifying what types of land use you considered, how they were determined and the characteristics of each land use (types of forests, agricultures, urban areas etc). Additionally, I suggest using the term "land use and land cover".

Ln 150 – Authors I suggest use a different symbol for partial pressure of the gas in the adjacent air.

Ln 157 - This sentence is not clear. The total watershed area should not be used for this calculation, but the water surface area should be used.

Ln 158-161 - There is a serious conceptual problem in this sentence. GWP is based on the capacity of a given gas to absorb heat compared to CO<sub>2</sub>. And here, the authors are assuming they are "determining" the GWP of the three gases. I assume that the authors are using GWP used on the Fifth Assessment Report to calculate emission in carbon dioxide equivalent (CO<sub>2</sub>-eq).

Ln 165-170 - The cited paper addressed lakes and pond, not streams and rivers. Addi-

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tionally, this sentence is displaced in the text and should be removed from the method section. The following sentences showing some results should not be here in the method section. Please, remove.

Ln 174 - Why calculate two different indexes? I suggest using only one index. Why are you using Prati and Oregon indexes? The author stated that the Oregon index was developed to express ambient water quality for general recreational use. Are the aquatic systems in the watershed for recreation purposes? If yes, I suggest the authors to describe the multiuse purpose in the Study Area section.

Ln 223 - "total emissions of the three gases per year from the whole river basin". This statement is not supported by your data because you analyzed only five consecutive days of the year. Authors should consider use "during the sampling period" instead of "per year".

Ln 247-256 - This entire paragraph does not add any useful information to the manuscript. I suggest removing this paragraph. Additionally, I suggest using only one index and focus on the relationship between the index and GHG emissions.

Figure 3 – Please, add the number of samples (n) that compose each box. And, I cannot identify which class does not have any value in the Oregon Index graphs. I suggest insert the class names in the x axis of each panel.

Ln 261-266 - This is an important information and the authors do not need both indexes to have the same conclusion. As I suggested before, use only one index.

Ln 268 - GWP should not be used here in this sentence. Please see the comments in the methods section.

Ln 271-274 – Holgerson and Raymond (2016) do not address emission from streams, as the authors stated in this sentence. They estimated emissions from non-running inland water. Please, verify.

Table 2 - What is the difference between "Urban" and "Industry"? Are urban areas

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residential areas? Please, specify each land use and land cover in the method section.

Figure 6 - This figure is in both the main text and Supplement Material. Please, remove from the SM.

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