

## ***Interactive comment on “Influence urban infrastructure on water quality and greenhouse gas dynamics in streams” by Rose M. Smith et al.***

**Anonymous Referee #2**

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GENERAL COMMENTS The manuscript examines greenhouse gas (GHG) dynamics in urban streams, a relevant but relatively understudied topic in biogeochemistry, with potentially relevant implications for global GHG budgets. The manuscript presents interesting results on CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O concentrations and emissions in several streams with different types of urban infrastructures. It is found that potential drivers of GHG dynamics (e.g. carbon, nitrogen, oxygen concentrations) differed among infrastructure types and were related to CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O supersaturation in stream water. Moreover, N<sub>2</sub>O saturation ratios measured in these urban streams were among the highest ever reported for streams. In general, the manuscript is well written and potentially interesting for the readers of the journal Biogeosciences. However, there are some important caveats, which I briefly list here and develop more in specific comments below: - Some strange terms are used throughout the text that could be avoided

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(e.g. “watershed continuum”, anaerobic concentration) - The role of external (non-in-stream) and non-biological sources of GHG is not well considered in the manuscript. This may also make some calculations such as the index of aerobic and anaerobic respiration inaccurate. - Some parts of the methods need clarification (e.g. supersaturation, DOM sample preservation). In addition some parts of the methods seem unnecessary given the results that are presented - The dynamics of CO<sub>2</sub> are not considered in the discussion section - Reference to relevant recent studies on GHG dynamics in urban streams are missing (e.g. see Alshboul et al. 2016 Environmental Science & Technology 50: 5555-5563 DOI: 10.1021/acs.est.5b04923 and references therein).

SPECIFIC COMMENTS Title: I have the feeling that something is missing in the title. Maybe the word “of” before “urban”? P1, L17: Unclear what is meant by “watershed continuum”. I think it would be more correct to speak about river network. This study focuses on the river and not on the whole watershed. This should be clear throughout the manuscript. P1, L23: Not sure these r<sup>2</sup> values are helpful here. It is not clear which statistical test was used. P1, L26: Again, unclear use of r<sup>2</sup> value. P1, L29: This last sentence of the abstract does not seem appropriate. It refers to emissions, which are not the focus of the manuscript. I would rather include a more conclusive sentence here. P2, L4: Land use can alter GHG emissions from streams not only through changes in drivers of stream metabolism. Changes in external GHG sources (e.g. groundwater inputs, soil leaching, point sources) and some geochemical reactions may also be important. In general, only part of GHG emissions from streams come from in-stream metabolism. This relevant aspect is not made sufficiently clear in this manuscript. P3, L20-24: Yes, but how much do streams contribute to whole watershed GHG fluxes? P5, L1: Please specify what blanks are here. P5, L5: Unclear what is meant by “study reach”. It has not been defined. P5, L26: Not sure this equation and the associated text are necessary according to the results shown later. P5, L29: What about minor tributaries? Define better what you mean by major tributary. P6, L10-12: Specify how TDN and DOC were analyzed. P6, L16: 0.7 μm-filtered samples stored for 2 weeks seems inappropriate for a DOM composition analysis. 0.2 μm fil-

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tering is usually preferred. P6, L29: Why use a new name for this index if BIX is the name normally used? P7, L25 to P8, L11: This index seems controversial and needs clarifications. Not sure it can be really applied because apparently, it does not take into account external (non-in-stream) GHG sources and non-biological GHG sources. P7, L13: Remove “and” before “flux”? P7, L23-25: Unclear. Please explain better how Cesc was estimated from SF6 additions. P11, L1: This subtitle is repeated 3 times in this page. P11, L21: The term “anaerobic CO2 concentration” seems erroneous. It does not make much sense. The same applies for anaerobic N2O or CH4 concentrations. P15, L25-28: I suggest the authors try to include more results-based conclusions and implications at the end of the paper. It also seems confusing that the authors emphasize wastewater here, when the paper is about streams and GIs. Tables & Figures: For greater clarity, I suggest keeping the same order for the 3 solutes (CO2, CH4 and N2O) in all tables and figures as well as in the text. Table 1: I do not think so many decimals are necessary for most of these variables. Table 2: “0.000” = “<0.001” or “<0.0001”? Table 4: If some variables were log-transformed (e.g. logDOC:NO3), this should be indicated in the methods section.

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