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***Interactive comment on* “The role of vegetation in the CO₂ flux from a tropical urban neighbourhood” by E. Velasco et al.**

E. Velasco et al.

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The authors thank the comments made by this reviewer, who raised valid concerns about the application of the eddy covariance method and representativeness of the CO₂ fluxes reported here. All his concerns are covered in the manuscript. If they are not discussed in the main text, they are discussed in the supplementary material. The specific comments are discussed below.

1) Eddy covariance methodology, as described, leads to many questions about the deployment and computations.

A number of articles in the peer-reviewed literature have demonstrated the capability of the eddy covariance method to measure fluxes of trace gases and aerosols over urban

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surfaces (e.g. Velasco and Roth, 2010).

2) A continuous record is cited but no mention made of gap filling, sensor orientation to North (based on instruments used, there will be gaps), and no mention of low Ustar criteria- tropical EC measurements are typically plagued with lots of data loss due to low Ustar and condensation on the sonic and open path IRGA windows.

Depending on the study purposes, gap filling may not be necessary. If diurnal averages are only considered for long monitoring periods (e.g. months, seasons, years), as in this study, no gap filling is needed (see Velasco and Roth, 2010).

As indicated in the manuscript, the sonic anemometer and CO₂ sensor at the top of the mast were mounted facing to the prevailing wind. They were shifted twice per year following the monsoons wind direction.

Friction velocity (u^*) in some cases is used to filter flux periods of weak turbulence. In cities, the urban fabric keeps usually intense turbulence (i.e. unstable atmospheric conditions), and therefore u^* is not a recommended parameter to remove periods with spurious data. As indicated in the manuscript, CO₂ flux periods not meeting the stationary criteria following Aubinet et al. (2000) methodology were not considered for further analysis. However, as part of the flux post-processing we evaluated u^* , and tested its usefulness as data flux filter, finding no improvements in the data quality analysis.

3) No mention is made of storage flux or influence of the point of maximum influence in the footprint since the instrument ht. is so low. It calls into question the representativeness of the flux measurement to the entire section of the city.

As in many other cities, the energy fluxes measured simultaneously with the CO₂ flux showed that the urban fabric tends to maintain unstable atmospheric conditions during the night through the release of the heat stored during the day. In a coming publication the characteristics of the energy balance will be presented.

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The observed footprint was widely discussed in the main text and supplementary material. The flux system (sensors and mast) were designed to measure fluxes from a specific fetch. The article states clearly that results are only valid for the monitored neighborhood and not for a larger section of the city.

4) Also, since the tower is so low (just 13m above a solid surface) some evaluation of the deployment is needed beyond that given in the manuscript. In particular, the authors mention a tower height of 20 m and a average building ht. of about 10m but as they question, the instruments are in a questionable zone - in the roughness sublayer or not? At what point is the displacement height concept (developed over a porous surface, such as vegetation) no longer valid- thereby becoming the effective height? So, some turbulent statistics are needed to show whether Similarity Theory holds. Otherwise, how do we know if eddy covariance represents the surface flux under investigation?

(Co)spectra analysis results included in the supplementary material, demonstrate the capability of our monitoring system to measure representative fluxes from the underlying surface.

As indicated in the manuscript, sensors were mounted at a sufficient height to be within the inertial sublayer. The displacement height was estimated using the methodology suggested by Grimmond and Oke (1999) and used to estimate the observed footprint.

5) Further, given that traffic is the main source of CO₂, given the potential for CO₂ vegetative sinks within the city, there should be considerable surface heterogeneity despite that authors claims for homogeneity (pg 23, lns 1-2). Unless winds are very steady, stationarity would be hard to maintain. So, the statistics must show more than whether Similarity holds for momentum due to roughness elements but must also hold for CO₂. Perhaps using integral stats., scaling the standard dev. of CO₂ (similar to sigma T/T star in Busch 1972 Workshop on Micrometeorology) vs. a range of stabilities would clear this up. Values should be near 2-3 in magnitude, given previous work and

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T and Q (water vapor) should have similar values (except for T which goes to infinite near neutral stability).

Urban surfaces are not as homogeneous as many natural surfaces and therefore eddy covariance flux measurements cannot be conducted in any urban neighborhood. The assumption of homogeneity in terms of land-use, buildings morphology and emission sources and sinks is discussed in the manuscript. The stationary and wind direction flux data analyses (mentioned briefly in the main text, but in detail in the supplementary material), as well as the (co)spectra analysis demonstrate the representativeness of the CO₂ fluxes reported here.

6) Traffic speed is used as an index of fossil fuel emission from vehicles. Since traffic is the largest emission for the area, a much clearer explanation as to how just this one statistic can be used to arrive at emissions is needed. What would seem to be the best index would be the number of vehicle kilometers traveled in the footprint, with data on type as well, per unit time.

Traffic emissions were estimated using the USA-EPA MOVE2010 model. Its predictions were based on traffic counts (automatic and manual), traveling speed, driving style and fleet characteristics. A complete description of the modeled emissions is presented in the supplementary material.

7) I also did not follow the calculation for human respiration. The derivation was not clear to me- in particular- that does 7ml/kg mean in this regard? 7ml of what?

The article provides references in which the human respiration calculation is explained in detail. In average, a person breathes 7 ml of air per kg of corporal weight. For example, if a person weights 50 kg, will breathe 350 ml of air.

8) There is also a bit of confusion in handling R_v and P_v . In eqn 1, P_v is called net photosynthesis, R_v as above ground respiration. Later its called dark resp. Above ground resp. is continuous but the ms. says otherwise- ie. 0 during the day. If its just

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occurring at night, why would it not be highest near sunset when 'night time' temps are highest and sugars are most abundant?

We are using nomenclature used by most of ecologists and experts in this field. All acronyms are defined in the main text. Note that dark respiration and ground respiration are different.

9) Pg 7276, Ln 5. Why are ranges given (eg. 33-44%) for missing data during the day and for night?

The ranges of periods missing the stationarity criterion provide information about the number of averaging periods used for the data analysis. Some readers may want to know the efficiency of our EC system to collect useful data over an urban surface.

References

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