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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The role of vegetation in the CO_2 flux from a tropical urban neighbourhood" *by* E. Velasco et al.

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The authors would like to thank the referee for this very thorough and helpful review. We have tried to reply to all comments and questions. In particular we agree that soil respiration cannot be separated from the total biogenic flux. We now clarify in the revised manuscript instances when we refer to the aboveground vegetation flux only and when to the total biogenic flux. We have added to the conclusions that the biogenic component, which includes respiration from soil and plants as well as photosynthesis, makes a positive contribution to the total vertical flux of CO2 (i.e. emission). The specific comments are discussed below (to help readability, only abbreviated versions of the authors extensive original major comments are repeated below).

1. Major comments

1.1 Soil respiration includes effects of aboveground vegetation.

We agree that soil respiration cannot be excluded from the biogenic flux. The manuscript has been rewritten accordingly. We have clarified when we refer only to the aboveground vegetation and when to the total biogenic flux. The conclusions now indicate that the sum of all biogenic components have a positive contribution to the total CO2 flux. It is important to point out that this study does not include lateral fluxes, only vertical fluxes to the atmosphere.

1.2 Respiration is a 24-h phenomena and not zero during day.

We have clarified that Rv corresponds only to aboveground vegetation during night and that Pv includes photosynthesis and aboveground respiration during day.

Our study did not include measurements of PAR, and therefore we are unable to estimate the precise time at which the CO2 flux from aboveground vegetation changes direction. However, according to the references and reasons provided in the article, as well as feedback from this reviewer it seems reasonable to assume that the CO2 flux from aboveground vegetation changes sign during the 2 hours centered on sunrise and sunset. Note that the anthropogenic contribution, particularly during the morning and afternoon rush hours is much larger than the aboveground biomass contribution.

1.3 Carbon fluxes due to maintenance, pruning and removal.

As mentioned before, this study does not include lateral fluxes, only vertical fluxes of CO2 to the atmosphere. We agree that a full carbon balance ads to include horizontal transport (probably mostly export), the estimation of which is beyond the scope of the present study. We also agree with the suggestion to call the NEE the 'direct effect of urban vegetation on the land carbon balance'

1.4 Tree biomass equations and growth rates.

We are aware of the potential uncertainties of the approach based on allometric equations and MTE. The article explains many of the potential uncertainties indicated by the reviewer caused by the use of biomass and growth rates for tropical forests instead of urban trees and we have made them even more explicit in the revised version. We will verify that all these potential uncertainties are included in the discussion.

1.4 Direct comparison of two methods.

All CO_2 flux figures are given in units of ton CO_2 km-2 h-1 or ton CO_2 km-2 yr-1. See also response to the following comment.

2. Minor comments

General - all units in the text should indicate whether a CO_2 flux is given in kg C per time and per area or in kg CO_2 per time and per area. In forest and agricultural meteorology, net uptake is usually given in g C m^{-2} day^{-1} instead of ton km^{-2} day^{-1}. I propose to do use those units in the article and abstract as well.

The units to report CO_2 fluxes have been determined based on the purpose of the study, flux magnitude, and spatial and temporal scales. We recognize that in forest and agricultural meteorology CO_2 fluxes are usually given in g C m-2 day-1. However, we have decided to use units of ton CO_2 km-2 h-1 or ton CO_2 km-2 yr-1 because they are the usual units to report urban CO_2emissions by many urban climatologist and environmental authorities.

The revised draft includes a sentence clarifying that the CO_2 flux is always expressed in units of CO_2 mass per area and time throughout the manuscript.

General - In several cases the units for fluxes lack the time (i.e. per hr, day, or year?). Example: p. 7268, l. 22 (Abstract) - Add time unit to 3.95 ton km^{-2}, i.e. it should be 3.95 ton km^{-2} day^{-1} and 2.55 ton km^{-2} day^{-1}.

All flux units now include the time. In many cases the time units are expressed in the text along the same sentence.

p.7268, l.4 (Abstract) - Sentence starting with 'Negative daytime...' does refer to results from other studies and is not overly relevant for the abstract - I therefore propose to delete the sentence.

Agreed and removed.

p.7268, l.7268 - 'Most important GHG' - On which time scale? Maybe say 'with the largest radiative forcing with a 100 year GWP'?

We have replaced "most important GHG" by "the GHG with the most important global warming potential".

p. 7269, l. 1 - Burning of fossil and biomass fuels is not the only source of CO_2. There is also cement production, forest fires, potentially volcanic sources that emit CO_2 and oceans that take up CO_2. I propose to specify the statement by adding 'In an urban environment...' to exclude forest fires, oceans etc. Cement production is potentially still relevant in industrial areas.

The sentence already indicates that the listed emission sources correspond to urban centres.

p. 7269, l. 11 - A reference to previous studies that defined the CO_2 metabolism would be appropriate at the end of this paragraph.

Prairie and Duarte (2007) has been added as a new reference.

p. 7269, l. 21 - 'even they can be important sources or sinks' - is there evidence from previous published studies that this is the case? What is considered important? If so, citing representative studies would be appropriate here.

A comprehensive discussion of this point is presented in the introduction, including all appropriate references according to our knowledge. No study about the importance of urban vegetation and human respiration in the CO_2 flux has been conducted in tropical cities.

p. 7270, l. 6 - EC is probably not the only direct measurement approach - there are also flux gradient methods possible, or chamber measurements for components of urban vegetation (e.g. over turfgrass)

Eddy covariance is the only existing method to measure directly CO_2 fluxes that include all major and minor natural and anthropogenic sources and sinks (Velasco and Roth, 2010). The flux gradient method relies on Similarity theory, while chamber measurements are only good to evaluate the flux of individual contributors, such as turfgrass, and therefore its application to a particular footprint will require assumptions.

p. 7270, l. 11 - its application to urban ecosystems (Velasco and Roth...).

It is implicit when we mention at the end of sentence that the method works best at the neighbourhood scale.

p. 7270, l. 12 - not only uniform land use / land cover (at which scale?) but also uniform roughness (building form, height and density) is a relevant prerequisite.

We clarify in the revised manuscript that uniform building morphology is also needed. The appropriate scale is that of a neighbourhood and in particular the flux footprint.

p. 7271, l. 3 - 'vegetation fraction' is not defined yet. Plan area fraction of vegetation? Crown coverage? Leaf area index?

It is now defined in the revised manuscript as the plan area fraction of vegetation.

p. 7271, l. 26 - 'dark respiration' is not equal release of CO_2 by biosphere during night (see major comments 1 and 2). If leaves of interest are enclosed in a cuvette, and exposed to artificial lights of various intensities then at zero light, dark respiration occurs. Important is that dark respiration also occurs when light is available, i.e. also during day, but cannot be separated as NEP measured by the cuvette -> NEP = P - R Hence the statement in brackets in incorrect implying this happens naturally only during night.

We have removed “dark respiration” and leave only “release of CO_2 during night time”.

p. 7272, l . 4 - “Capture” is probably the wrong word here (also p. 7273, l. 5 and other instances), as it implies that the CO_2 is immobilized for a long time. What is the typical turn-over rate of CO_2 in urban vegetation? How often are trees pruned or cut? Same on l. 15 'absorbs' is incorrect term. It is not the process of “absorption” that removes the CO_2 - it is photosynthesis.

We have replaced “capture” and “absorption” by “sequestration” in the revised manuscript. The turn-over rate of CO_2 and the pruning frequency vary from one site to another. The majority of studies cited in the present manuscript do not provide such data. This will be useful information when trying to estimate the horizontal transport (removal or import) of biomass for our sampling volume.

p. 7272, l . 15 - Tropical urban vegetation.

Correct, it must be tropical urban vegetation.

p. 7272, l . 20 - 'bottom up approaches'. Here a statement is missing that the bottom up model does NOT include vegetation (but soils? see 'Major Comment 1' above)

We have replaced “biogenic flux” by “aboveground biogenic flux” in line 21.

p. 7273, l. 5 - urban vegetation

Correct, it must be urban vegetation.

p. 7273, l. 22 - 'land-use' is an incorrect term in this context -> Authors probably mean 'land cover'. ('land-use' would mean “residential', 'commercial' etc.). I would also state that this are plan area cover fractions.

Correct, it must be land cover. We have added in the revised manuscript that those fractions correspond to plan area covers.

p. 7273, l. 24 - what is underneath the 11% tree crowns? lawns? buildings? How were fractions determined?

Eleven percent refers to the plan area covered by tree crowns. This fraction was determined from aerial photos. The area underneath tree crowns is mostly lawns. The revised manuscript now includes this information.

p. 7274, l. 15 - Is there any evidence that an urban heat island circulation (UHIC) really exists in Singapore? I would have thought that land-water differences are dominating in the geographic setting, and possibly roughness influenced flow changes? Any references for a UHIC in Singapore?

A possible urban heat island circulation given Singapore's coastal setting and high proportion of urbanization is discussed by Chow and Roth (2006). The revised manuscript will include this reference.

p. 7274, l. 26 'well above the average height of the roughness obstacles. Figure S1 suggests that some buildings are as tall as 20m. Although their plan area fraction is low, they might still disproportionally influence the turbulence in their wakes. Authors should probably comment on those isolated higher buildings in forming a higher blending height the 'Methods' section as well.

It is true that those few buildings over 20 m of height may influence the turbulence in their wakes. However, the (co)spectra analysis shows that our system is capable to measure representative fluxes from the underlying suburban area. In an upcoming article we will present a detailed analysis of the turbulence characteristics at this site.

p. 7277, l. 9 - are times given in LST (GMT + ?h) or LMST (Local mean solar time)?

All times are given as local standard time (LST). The revised manuscript includes the corresponding clarification.

p. 7277 - l. 9 period with low or near-zero NET biogenic fluxes - because (P = $-$R), crossover. Fluxes P and R might be still substantial but just of opposite sign. Of course, respiration also happens during day, so R is a 24 h phenomena and P is only a daytime

phenomena (see major comment 2).

See response to major comment #2.

p. 7278, Section 2.3.2 - How is space cooling powered in Singapore? Are there gas fired cooling engines found in the area? If so the timing would also depend on cooling demand. Or is it all electricity?

Air conditioning in Singapore's households is by electricity.

p. 7279, l. 13 ff. - I don't see why a conversion using ambient temperature and pressure is needed here. The respiration mass flux (or molar flux) of a human does not depend on temperature and pressure, and the flux on the tower top neither. Needs an explanation.

These meteorological variables are needed to convert from volume to mass of breathing air.

p. 7280, l. 1 'model has no rational basis' - probably 'the relationship has no physical basis', nevertheless an increase with increasing temperature makes sense (is rational) from a biological viewpoint. Isn't it?

Agreed, it has to be “no physical basis” instead of “no rational basis”.

p. 7280, l. 11 - any reference that supports that soil respiration rates are higher in cities? If there is maintenance, watering and fertilization, will this not also affect PV and hence the two balance each other out roughly? (see also major comment 1 that soil respiration should be included in biogenic flux).

Since we could not find any reference we have replaced “knowing” by “assuming” in line 9 in the revised manuscript.

p. 7782, l. 12 - how did Authors determine that Chave et al. (2005) provides the 'best predictive allometric equations'?

This is explained in Section 3.2, second paragraph.

p. 7783, l. 25 - It makes sense that in a dense tropical forest light is the limiting factor for growth, but is this also applicable to cities where other factors are present (water limitation, air pollution etc.). Later on p. 7284, l. 12 authors argue that the forest sites experience a similar climate as Singapore. This applies to the macroclimate, but the microclimate (urban heat island, wind, vpd) might be different - see also major comment 4.

We recognize that the alternative model of MTE for tropical forests has important limitations like those raised in this comment, but we do not have all information to be able to fully discuss them. The influence of some factors, such as e.g. water limitation is widely discussed by Muller-Landau et al. (2006) in the original article introducing the model. Regarding to the climate, we refer only to general variables (i.e. ambient temperature, humidity and precipitation) which will indeed be slightly modified by the urban context but will still be generally similar to the persisting macroclimate.

p. 7784, l. 25 - carbon used for understory growth (is this root growth?, or vegetative reproduction above-ground?), for reproductive organs (flowers etc.) and emitted as VOC - All this carbon is eventually going back to the atmosphere in a short time (either be removed from the area by maintenance when flowers etc. fall to the ground or by VOCs) - so I thought that the factor for adjustment should be _20% less, i.e. 1/1.2 not 1.2 in the biomass production estimate. Maybe I don't follow the Author's argumentation correctly here.

The present study only considers the vertical flux of CO_2, no lateral fluxes and neither fluxes of other compounds containing carbon. According to the reference provided in the manuscript, a factor of 1.2 is on the conservative side.

p. 7787, l. 11 ff - The difference between EC and modeled approaches, which authors argue is equal to RV - PV is extremely large. Soil respiration (sum of heterotrophic and autotrophic respiration in soils) is usually stronger than above-ground autotrophic

respiration. The results suggest that during night, when PV is absent, RV is about 3-4 times larger than RS. This is in disagreement with most forest ecosystem studies (see FLUXNET for example).

This is true for natural environment without buildings, roads and impervious surfaces. In our case the plan area fraction available for soil respiration is only 15%.

p. 7288, l. 1 - the diurnal asymmetry is also observed in many other forest and agricultural ecosystems and is in part driven by the higher vpd due to entrainment of dry air from the free troposphere under strong convection (see textbooks).

We agree and the higher afternoon vpd is already mentioned in the manuscript as a possible reason.

p. 7288, l. 8 - the fact that the PLUME is variable does not imply that FLUXES are variable. The concentrations in the plume are controlled by mixing and wind / weather while the fluxes reflect the relatively steady metabolism of the city. The variability is more likely due to changes in the metabolism with varying footprints.

We agree partially. The EC flux measurements are conducted under the assumption of homogeneous distribution of emission sources and sinks.

p. 7288, l. 19 - this is not surprising because - as Authors say - the periods were used to adjust fluxes.

Indeed.

p. 7289, l. 12 - 19.3 ton km^{-2} should be 19.3 ton km^{-2} day^{-1}. (also other instances below).

The sentence starts saying "on a daily basis ..."

p. 7289, l. 26 - the reduction of soil respiration in cities might be reduced because of the impervious surfaces, but nevertheless soil respiration will be more intense in the areas where soil is present. Also why is soil considered 'natural environment' but not part of 'vegetation' - it includes the tree and lawn roots that respire (see also major comment 1)?

As indicated in section 2.3.4 we selected the maximum soil respiration rate reported for tropical forests in the global database of soil respiration (Bond-Lamberthy and Thomson, 2010) to account for the reasons given in this comment.

p. 7290, l. 2 - 'Photosynthesis captures 22% of the CO_2 but dark respiration returns 14%, resulting in a net uptake of 8%' - Incorrect, because it is not the ecosystem net uptake, nor the net-uptake by the vegetation - again this is only including aboveground autotrophic respiration, but neglects the root respiration of the vegetation (see also major comment 1).

See response to major comment #1.

p. 7290, l. 17 ff. - I can't follow the objective for this section, because I don't see why biomass is estimated and not the change in biomass over time. How can biomass be related to the flux?

Biomass growth is estimated from gross photo synthesis. Section 2.4.2 explains how the change in biomass over time was calculated.

p. 7292, l. 11 ff. - '23 small trees are needed to replace one large tree' - The larger trees will have a larger spatial extent of the root network, and hence disproportionally contribute to the soil respiration (which is not included, see major comment 1). Hence the statement needs likely a revision.

This is an interesting point. We have added this caveat to this particular conclusion. Unfortunately, the approaches used here do not allow us to evaluate how a larger root network could disproportionally contribute to the soil respiration in an urban ecosystem. This is an open question to the community.

p. 7292, l. 16 - What justifies to relate the size of trees to their overall sequestration rate. Smaller trees will grow faster and hence disproportionally grow. Old trees (>_

50 - 100 yrs depending on species) will likely not sequester any carbon (P = RV + RS influenced by tree).

Our approach is based on the size of the trees, not their age. Knowing the trees' age we could properly address this comment.

p. 7294, l .24 - 'Large fruit production' - What happens to the fruits? They will presumably fall to the ground and then disposed?, Or they are either eaten and released back by human respiration, or decomposed in landfills or composts? So I cannot see how the fruits sequester carbon in the long term (scale of years to decades).

We agree if we want to consider the full net long-term carbon budget. Our study evaluates only vertical fluxes to the atmosphere at the neighbourhood scale. If the coconuts and other fruits are not consumed at the site they must be removed by the city cleaners. What happens to the fruits at the end is beyond the scope of this manuscript.

p. 7293, l. 13 - The direct comparison between the two approaches is not justified because Approach 1 is PV -RV while the second approach is PV -(RV +RS) - see again major comment 1. Adding soil respiration to the equation, the difference would be quite large and the biogenic flux would be a net source in Approach 1. 14% + 12% - 22% (Fig. 3) = 4% -> Urban biosphere would be a source of +0.7172 ton $year^{-1}$.

We agree that the total biogenic flux (plants and soil respiration, and photosynthesis) has a positive contribution (i.e. emission) to the atmosphere of 4%. We have revised the manuscript to clarify this point. From approach 1 we can get an estimation of the CO_2 sequestered by photosynthesis and compare it with the CO_2 associated with the growth rate of the trees estimated by allometric equations and MTE.

p. 7293, l. 13 - unit lacks area (per km^{-2}?, or for the entire 500m radius study area?). Also is it CO_2 or C? Values should be expressed in g C per m^{-2} year-1. If I assume it is per km^{-2} and C only then this translates to 500.1 g C m^{-2} $year^{-1}$ – this would be equal to the NEE of the highest productive ecosystems reported worldwide. An unrealistic result for the small vegetation fraction in this area (see major comment 5).

We missed to indicate that those figures are per km2. The sentence indicates clearly that they correspond to CO2 sequestration.

p. 7294, l 22 - Authors should explain how a measurement of carbon isotopic ratios (del13C) could be used separate between respiration and gasoline. I understand that both respiration and gasoline have a del13C or roughly -27 per mil so I am afraid that a Keeling plot will lead to the same intercept, not?

An explanation of how to partition carbon isotopes is beyond the scope of this article. To avoid errors we will replace "stable isotopes in CO2 (12C and 13C)" by "the isotopic composition of CO2".

p. 7295, l. 2 - '[Vegetation] can offset a significant fraction of the anthropogenic CO2 flux'. This is an incorrect (because of the methodology, see above) and dangerous statement. In fact Authors reverse their conclusion, and the last sentence says 'The present vegetation [...] reduces the carbon footprint of the residents [...] by only 0.4%'. A large fraction of of the GHG emissions - as stated correctly in the conclusion (which is excellent) - are emitted outside the study area. So why open the conclusions with this incorrect statement?

We have replaced vegetation by aboveground vegetation in the revised manuscript and clarified that when soil respiration is included, the total biogenic flux contribution is positive (i.e. emission).

p. 7296, l. 1 - 688 kg yr-1 cap-1 (i.e. add cap-1)

The sentence indicates that it is per capita.

Figure 1 - Unit should indicate whether CO2 flux is mg C m−2 s-1 or mg CO2 m−2 s-1 .

The legend indicates clearly CO2 flux (mg m-2 s-1).

Figure 2 - Unit on y-axis should indicate whether CO2 flux is ton C km−2 hr-1 or ton C km−2 hr-1. (or convert to μmol m−2 s-1)

Same response as to previous comment.

Figure 3 - Same as for Figure 1 and 2 (Unit). The symbols of the sun and moon are misleading. Respiration can happen all 24h long (see major comment 2). Also the 12% RS vs. 14% RV is unrealistic compared to forest ecosystems.

Same response to previous two comments regarding to the units. The sun and moon indicate that during night- and daytime the total flux from aboveground vegetation is negative and positive, respectively. Remember that the plan area covered by vegetation is only 15%.

Figure 4 - Same as for Figure 1 and 2 (Unit).

Same response to comments for Figures 1 and 2.

Figure 5 - Same as for Figure 1 and 2 (Unit).

Same response to comments for Figures 1 and 2.

Figure 6 - Same as for Figure 1 and 2 (Unit). Y-Axis cannot be labelled 'biogenic flux' as it is only RV - PV and does not include RS. See major comment 1.

Same response to comments for Figures 1 and 2 regarding to the units. We have replaced "biogenic flux" with "aboveground biogenic flux".

Figure S2 - What is the source for tree height and building height? How was this determined?

The revised manuscript includes a line indicating the instrument used for the height measurements of trees and buildings.

Figure 1 and S7. Shown are not average footprints, but the average extent in each wind sector. In other words: The footprints are not taking into consideration the frequency distribution of wind directions. The definition of footprint extent is different compared to the cumulative footprints defined in Chen, B. et al. 2009. Assessing tower flux footprint climatology and scaling between remotely sensed and eddy covariance measurements. Boundary-Layer Meteorology 130, 137-167.

The reviewer is correct. Figures 1 and S7 do not show average footprints. They show the average footprints for each wind direction. This has been clarified in the revised manuscript.

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