Atmos. Chem. Phys. Discuss., 13, C2466–C2478, 2013
www.atmos-chem-phys-discuss.net/13/C2466/2013/

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.

Anonymous Referee #3

Received and published: 17 May 2013

The article by E. Velasco et al. is carefully presented and its topic is relevant for biogeochemical cycling of urban ecosystems, urban planning and green space management in cities. I'd like to credit the authors for providing the first study that quantifies the effect of land-cover / vegetation on the carbon-balance (and possible land cover change effects) in a tropical city - and it is good to have two independent methods.

Unfortunately, the overall methodology – in particular approach 1 – has major flaws (major comments 1 and 2, see below) as well as severe limitations (comments 3 to 5). This requires recalculations before publication.

1 Major comments

1.1 Soil respiration includes effects of above ground respiration

The methodology quantifying the role of vegetation through Approach 1 is flawed in my view. What the authors quantify as 'biogenic flux from vegetation' (or ≈ vegetation effect) excludes entirely all CO_2 emissions from the sub-surface, i.e. summarized as soil respiration R_S. Soil respiration is the sum of autotrophic (plant) and heterotrophic microbial) respiration that happens below-ground. Hence it includes part of the CO_2 respired by plants.

Specifically I disagree on p. 7277, l. 6 that 'the difference between the measured flux by EC and the estimated emissions represents the biogenic flux from vegetation'. Note that in the same paragraph authors specify that their estimated emissions are $E_T + E_B + R_H + R_S$, hence *include* already soil respiration (R_S) and remove it from the 'biogenic effect'. Only above-ground autotrophic respiration (R_V) minus photosynthesis (P_V) in their argumentation, represents the biogenic flux. This is incorrect. It neglects that trees also directly cause and indirectly control respiration below-ground (autotrophic respiration of roots + heterotrophic respiration [= decomposition of dead organic matter that was once vegetation by microbes]). In fact, below-ground autotrophic respiration is typically much larger than above ground autotrophic respiration. In other words, in absence of trees or any vegetation, soil respiration in a 'climax' would be different (and ≈0), because the CO_2 leaving soils is directly (autotrophic) or indirectly (heterotrophic) caused by the living vegetation biomass or earlier residuals of vegetation.

A number of conclusions are therefore incorrect - on p. 7289, l. 12 ff., Authors conclude thatn'the difference between measured fluxes and estimated emissions is -1.4 ton km^{-2} [and day^{-1}]. The negative values indicates a net assimilation of CO_2 by vegetation' or 'Photosynthesis captures 22% of the CO_2 but dark respiration [=autotrophic above-ground respiration] returns 14%, resulting in a net uptake of 8%'. This is neglecting the effect of trees on below-ground soil respiration. Note that the Q_{10} values taken from the literature already includes the effect of roots etc (unless it would be determined in a 'root-exclusion plot')

I argue the effect of the urban biomass should be instead R_V + R_S - P_V (plus also lateral removal of biomass - see major comment 3). I admit that it is challenging to separate between autotrophic and heterotrophic respiration in soils, so a direct attribution to trees vs. microbes is not possible - but does it matter for management implications? Microbes decompose anyway organic matter that was once urban vegetation and hence part of the vegetation C-cycle.

To solve this flaw, soil respiration (section 2.3.4) should not be modelled, but be part of the the difference between the modeled fuel emissions and the total measured flux. Or alternatively, it can be modelled, but then the difference between EC and model should be just labelled the above-ground biogenic flux (i.e. R_V - P_V) and the modelled soil respiration should be added on top of R_V-P_V to calculate the overall biogenic flux or NEP of the urban vegetation.

I agree that Authors should exclude humans as part of the 'biogenic flux' (which is correctly labelled 'biogenic flux of vegetation') and therefore include it in the modelling of the other emissions. From a carbon offsetting viewpoint, human respiration is not relevant. It recycles plant material.

The biomass approach (Approach 2) seems appropriate and is not affected by soil respiration, as it quantifies biomass accumulation over time which is already the actual carbon sequestered in trees and soils (as authors also included root biomass and dead organic matter in my understanding).

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

On page 7277, line 8 authors say that 'R_V = 0 during day' (R_V is above ground respiration). Authors argue that times around sunrise (06:00 to 08:00) 'almost exclusively represent contributions from E_T, E_B, R_H and R_S'. This is incorrect. Above ground respiration R_V happens over 24 hours, although it changes in magnitude driven by P_V and other plant physiological controls (as also soil respiration R_S is a 24h process). Only CO_2 uptake due to photosynthesis is absent during night (and only if CAM plants are neglected).

Hence the methodology assuming sunrise and sunset as periods of zero flux does not hold. Strictly the zero-crossing (if any) of the R_V - P_V curve should happen AFTER sunset, and the second one BEFORE sunset. The actual timing depends on the strength of P_V vs. R_V [or PV vs. R_V = R_S (see comment 1)] and on environmental controls. There might be days when PAR irradiance is so low and vpd so high that (R_V + R_S) - P_V might stay positive (source) all day.

Also the magnitude of the gradient of ∂NEE/∂t at this time is usually large - so how is a 2h frame centered around sunset and sunrise justified?

The reference to acclimation periods mentioned on l. 19, p. 7277 is correct but in my understanding applies to shorter-term changes (i.e. seconds to minutes). It therefore does not support the assumption of zero-flux at sunrise and sunset. The justification needs to be corrected.

1.3 Carbon fluxes due to maintenance, pruning and removal

Although hard to estimate in terms of carbon fluxes, urban vegetation is heavily managed, and hence a full carbon balance (or judgement of the effect of vegetation) should also consider exports of carbon due to pruning, cutting, collection of dead leaves and removal of trees if those organic products are removed from the neighborhood and decompose somewhere else (burnt, compost etc.) - (see for example Kellett et al. 2013, or the discussion on p. 7293, lines 4 ff.). I don't think those fluxes can be measured, but this limitation should be definitely discussed in the context of 'vegetation offsetting', because otherwise the sequestration is overestimated.

Further, vegetation will also have indirect effects on fossil fuel consumption, e.g. reduced cooling demand (well discussed in the conclusion by the Authors). I propose to call the NEE (after correcting the major comments 2 and 3 above) the 'direct effect of urban vegetation on the land carbon balance'.

1.4 Tree biomass equations and growth rates

Authors determine biomass and growth rates in city based on approaches that were developed for application in tropical forests (e.g. Chave at al., 2005). They implicitly assume that tree allometry and growth is the same in tropical cities compared to tropical forests (p. 7272, l. 25, and p. 7291, l. 16 ff). There are many concerns with the transfer of allometric equations and/or the MTE model from forests to cities, and some are properly discussed on p. 7294, l. 3 ff, but other factors not mentioned there include: Enhanced or reduced tree growth due to favourable irradiance (isolated trees) or shading by buildings, unrestricted crown growth, effects of air pollution, irrigation, management, fertilization, enhanced water stress of urban trees (isolated vs. moist microclimate of forest canopy) etc. Those effects need to be accounted for in the models, and the assumption that they are not relevant should be justified in the methodology.

1.5 Direct comparison of two methods

I must also admit, that I am surprised by the magnitude of the fluxes from the second approach (also the 1st approach, but I assume the first approach is incorrect). Although

the neighborhood is composed of productive evergreen vegetation, the 500 g C m^{-2} Net Ecosystem Productivity (NEP) are surprisingly high, as productive as the most efficient forests reported in FLUXNET (see e.g. Baldocchi et al. 2008). In my judgement, I am assuming that Authors report g C not g CO_2 - the units are not provided with CO_2 or C, a minor point discussed below. If its is g CO_2, then the results are more realistic.

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.

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}.

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.

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

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.

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

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.

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)

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

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.

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

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.

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.

p. 7272, l . 15 - 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)

p. 7273, l. 5 - *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.

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

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?

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.

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

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).

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?

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.

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?

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 P_V and hence the two balance each other out roughly? (see also major comment 1 that soil respiration should be included in biogenic flux).

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

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.

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.

p. 7787, l. 11 ff - The difference between EC and modeled approaches, which authors argue is equal to R_V - P_V 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 P_V is absent, R_V is about 3-4 times larger than R_S. This is in disagreement with most forest ecosystem studies (see FLUXNET for example).

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).

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.

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

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

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)?

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 above-ground autotrophic respiration, but neglects the root respiration of the vegetation (see also 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?

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.

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 = R_V + R_S influenced by tree).

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).

p. 7293, l. 13 - The direct comparison between the two approaches is not justified because Approach 1 is P_V-R_V while the second approach is P_V-(R_V+R_S) - 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}.

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)

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?

p. 7295, l. 2 - '[Vegetation] can offset a significant fraction of the anthropogenic CO_2 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?

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

Figure 1 - Unit should indicate whether CO_2 flux is mg C m^{-2} s-1 or mg CO_2 m^{-2} s-1 .

Figure 2 - Unit on y-axis should indicate whether CO_2 flux is ton C km^{-2} hr-1 or ton C km^{-2} hr-1. (or convert to μmol m^{-2} s-1)

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% R_S vs. 14% R_V is unrealistic compared to forest ecosystems.

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

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

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

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

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.

sectionReferences

Baldocchi, D. D. (2008). TURNER REVIEW No. 15."Breathing" of the terrestrial biosphere: lessons learned from a global network of carbon dioxide flux measurement systems. Australian Journal of Botany, 56(1), 1.

Cairns, M.A. et al., 1997. Root biomass allocation in the world's upland forests. Oecologia 111, 1–11

Kellett, R., et al., 2013. A systems approach to carbon cycling and emissions modeling at an urban neighborhood scale. Landscape and Urban Planning, 110, 48–58.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7267, 2013.