Atmos. Chem. Phys. Discuss., 12, C5149–C5175, 2012
www.atmos-chem-phys-discuss.net/12/C5149/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal and annual variation of carbon dioxide surface fluxes in Helsinki, Finland, in 2006–2010" *by* L. Järvi et al.

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Received and published: 31 July 2012

We thank both referees for their valuable comments which clearly improved the manuscript. In addition to the more detailed corrections and comments listed below, the language of the paper was carefully checked. Figure 1 was also re-plotted as a better source for the aerial photograph was found. In addition rmse was changed to RMSE in figures 7 and 9.

Kind regards, Leena Järvi

Anonymous referee #1

The paper presents data from long-term measurements of carbon dioxide at the well documented SMEAR III station in Helsinki, Finland.

CO2 is the most important greenhouse gas and urban agglomerations play a key role as source areas of this trace gas due to human activity, i.e. fossil fuel burning. Direct quantification of CO2 fluxes by means of Eddy-covariance (EC) measurements has become a common method for vegetative surfaces and was proofed over heterogeneous urban areas several times until now. The scientific relevance and unique feature of the given data set is the long time period being analysed (in comparison to other published studies). Given the large number of influencing factors within urban areas steering the description of inter-annual variability of CO2 exchange is of high interest by evolving urban CO2 flux measurements around the globe. Since continuously operated urban flux sites are scarce, effort in producing long-term data sets is of high interest within the atmospheric sciences.

I recommend the paper for publication in ACP after minor revisions.

General comments

In all parts the paper fulfills formal scientific standards. The paper is clearly structured; the use of the English language is adequate and needs no further review. The methods are described sufficiently and the cited literature represents the state of knowledge in an appropriate manner.

1) Methods for gap-fill flux data including the statistical modeling with artificial neural networks (ANN) have been described and compared before with the focus on vegetated surfaces. In urban areas the usability of several methods is strongly reduced due to the large number of processes controlling the exchange of CO2 in the urban boundary layer. ANN modeling is the most promising method for urban flux data although it remains a “black box” in terms of describing physical/biological/anthropogenic processes. Hence, the networks and the modeling results are highly site specific and a direct transferability to other sites and/or the urban surface in general is not given. Although the authors are right with their demand for a better systematization of gap-filling of urban flux data, this general weakness of ANN in difference to a systematically pro-

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cess modeling is an issue which needs to be discussed with more profundity within the paper (and the scientific community in general).

- The authors fully agree that the downside of ANN is that it is a black box without information on the actual processes and therefore same networks can't be directly applied at other sites. Text "Thus, ANN does not simulate the physical and biological processes itself and therefore the same network cannot directly be utilized at other sites" was added at the end of Section 3.3 (L468 - 469).

2) In the paper wind direction (WD) was splitted into nine binary variables for ANN training. An alternative way would be to transfer WD into fuzzy values as it was done with the time variables. This could lead to a reduction of training variables and to a higher resolution of WD information for the ANN. This is supposed to be beneficial for the model since it was shown that Fc generally shows strong variability with WD in urban areas due to the heterogeneous land use which is evident for the given site, too. According to the 30 min fluxes shown in Fig. 4b one can suspect the 40° sectors as not coding the impact of WD on Fc sufficiently. Maybe the advantages of the used method have to be discussed more deeply within the paper.

- The authors are aware of the advantages of treating WDs as fuzzy values, but as we wanted to create time series for the different surface cover areas, division to wind direction sectors was made. We also tested the networks using narrower wind direction sectors (10-20°) but this did not improve the performance of ANN. Short explanation "Wind direction was not treated as a fuzzy variable since ANN will be used to generate artificial time series for the different wind direction sectors." has been added in section 2.3.1 (L284 - 286).

3)As described in the text, all gap-filling methods underestimate large fluxes (slopes of regressions in Fig. 7, text on page 8370, 19-21). Are there further explanations for this behavior? High fluxes can be generated by non-stationary conditions in trace gas concentrations whose likeliness is increased over urban areas. What QA/QC steps

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have been performed to ensure steady state conditions?

- We have made all recommended QA/QC steps in our dataset including stationarity test, flux skewness and kurtosis, and visual inspection. Detailed information can be found from Nordbo et al. (2012) in Tellus B. However, it is still possible that some unstationary peaks are left as urban areas are highly heterogeneous and measurements can include emissions from some untypical sources, such as vehicle at the parking lot. Text "The inability of the methods to predict extreme values can be associated with unstationary situations that the recommended quality assurance routines cannot remove." was added in Section 3.3 (L455 - 457).

4) The behavior of Fc in the summer of 2006 differs between the data shown in Fig. 3 and Fig. 6. The first is supposed to be based on the non-gap filled data set while the latter shows the gap-filled sums. The strong difference between the low/negative fluxes in June/July and the strong emission in August (according to Fig. 3) cannot be reproduced in Fig. 6. There, the period in which the sum is decreasing (i.e. the occurrence of negative fluxes) can be seen during August and even September. Is this effect caused by the gap-filling (and, if yes, why?) or is it simply an erroneous axis caption?

- We assume that the referee means Fig. 8 instead of Fig. 6. In Fig. 8, the x-axis was wrong and has now been corrected. Now the peak carbon emissions in August 2006 are visible in the yearly cumulative plot.

5) Exchange sums are given in g C m-2 a-1. Since only CO2 fluxes have been measured and no CH4-C or other OC exchange was quantified the usage of this unit is a bit misleading. Although it is used in terms of CO2 fluxes over vegetated areas in nearly all papers (since CO2 dominates the exchange of C in the ABL) the usage of g CO2 m-2 a-1 should be considered.

- For consistency and comparison purposes we use same units as has been used above vegetated areas. These units have also been used in other urban studies.

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Specific comments

1) Abstract, line 20: “the without” has to be “the one without”

- Text was corrected.

2) 8358, 19: “Kordowski and Kuttler (2008)” must be “Kordowski and Kuttler (2010)”

- Reference was corrected.

3) 8360, 22-23: The description of the heating system has to be clarified. What effects do the given power values have on gas temperatures/vapor saturation within the gas flow?

- Tube heating does not affect the measured CO2 fluxes as closed-path analyzer measures the mixing ratio of CO2 relative to dry air and this is conserved during thermal expansion/compression and through hydrological processes.

4) 8361, 12: Given the linear regression between open and closed path fluxes – what is the slope and the offset, respectively?

- The linear regression slope and offset have been added to the manuscript in Section 2.2.1 (L166 - 167). Previously, RMSE and squared R were given as lower/upper limits for regressions made for different seasons. Now fitting parameters for all data are given, and therefore the values of RMSE and squared R have slightly changed.

5) 8361, 13-18: Have the data gaps been analysed for a day/night-bias, i.e. if there were probably more gaps during night-time due to quality assurance? If yes, does this raise any issues when performing the ANN training?

- We have checked the amount of gaps in day/night basis and at our site more data is missing around noon (20% more) than at night over the five years of analyzed data. The effect of this on ANN training is assumed to be negligible as the diurnal behaviour is strongly present in the input variables of ANN.

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6) 8362, 4: “in the measurement tower” has to be “at the measurement tower”. Given the 31 m Ta for ANN training one can suppose a near-ground measured Ta would be more appropriate to explain influences of meteorology on Fc. Was there a near-ground Ta (2 m) available and, if yes, why was it not used?

We prefer using “in” instead of “at”. Unfortunately, we do not measure air temperature at two meters so we use the air temperature measured at higher level.

7) 8363, line 5-15: The main driver for soil respiration is soil temperature which – depending on depth – differs significantly from air temperature in both, the amplitude and temporal behavior. The authors comment the absence of measured Ts as being unfortunately in a later part of the paper. The reviewer fully agrees.

- Yes. In future it would be nice to have continuous soil temperature measurements in several places around the tower. So far, we have measured those over a short campaign and therefore they were left out from the manuscript.

8) 8364, line 17: The term “overfitting” should be explained shortly since this is ANN specific and may be new to some readers.

- Explanation “(random noise modeled as true fluxes)” was added in Section 2.3.1 (L268 - 269).

9) 8364, line 26: Although it is, to my opinion, worth thinking about what “fluffy variables” could look like in terms of ANN modelling, I suppose this must be read “fuzzy variables”.

- Yes, there was a typo in the text. Fluffy has been changed to fuzzy.

10) 8368, line 5-6: “daily average minimum” should be read “minimum daily average”.

- Text was corrected.

11) 8373, 2: Only data with snow cover was used for the derivation of road traffic emissions. I suppose this was done to reduce the impact of biological processes. This

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issue should be clarified.

- A sentence ". . .and biological activity is assumed to be minimal"was added in Section 2.4 (L353 - 354).

12) 8377, 2: The explanation of the differing CO2 exchange in the summer of 2006 must be reconsidered. The summer is explained as being exceptionally warm and sunny which corresponds to the stronger negative correlation of PAR and Fc. Drought conditions instead are supposed to have a weakening effect on the carbon sink strength (which maybe is the case in August 2006).

- Indeed drought would have a weakening effect on the carbon sink strength. Around our measurement site extensive irrigation takes place and despite the exceptionally warm and non-rainy summer, plants would have enough water. However, as suggested by the other anonymous referee (see below). We have removed the whole paragraph.

13) Table 1: Sector degree information for "Road" and "Vegetation" is duplicated.

- The degrees were corrected in Table 1.

14) Fig. 11: Filled symbols for night-time data are not explained in the legend and it is hard to distinguish between the symbol shapes. The layout should be reconsidered.

- The figure lines were modified to make the plots more clear. Also the resolution of the figure was improved. Closed symbols were explained in the last sentence of the figure label.

Anonymous referee #2

This study reports on long-term eddy-covariance CO2 flux measurements over a high latitude city. The measurement site being located at the interface of primarily vegetated, built and paved (road) sectors, CO2 fluxes over different urban cover types were analysed. Seasonal variation was related to environmental and anthropogenic variables. Three gapfilling techniques were assessed and annual net emissions are

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reported. Soil respiration contribution to net CO2 exchange was also investigated.

The paper presents original work with a rare long-term dataset of urban CO2 fluxes as well as a useful evaluation of gapfilling techniques for EC measurements in urban environments. Annual estimates of net CO2 emissions for different urban surface covers as well as characterization of seasonal variability of CO2 fluxes represent significant contributions. With few exceptions, methods and results are well described and clearly presented. The manuscript is well structured.

However, minor edits would be needed. Some interpretations appears to be speculative to some degree or lacks nuance and would need further discussion or should be left out. Also, the authors should be careful with phrasing that suggests causality as it is misused on several occasions throughout the text. Words like “controlling” should not be confused with “relate to”. Finally, review of the text by a native English-speaking person might be useful as wording looks odd at times. Details are provided below.

Detailed comments

p. 8356, abstract: Soil respiration measurement is worth mentioning in the abstract.

- Sentence “Online traffic counts and soil respiration measurements were utilized in the study” was added in the abstract (L17 - 18).

p. 8356, l. 14, abstract: I’m not sure how to interpret the 75% difference, either precise 75% of what (mean annual emissions from road sector) or state the actual absolute number (2630 g C m-2). The latter is preferable since percentages do not provide information on the magnitude of the annual emissions.

- The text was modified to “. . ., the area of the road emitted 3500 g C m-2 whereas the area of high fraction of vegetation cover emitted only 870 g C m-2 showing the effect of surface cover to be large in urban areas” (L25 - 27).

p. 8356, l. 21, abstract: Replace “an average annual emission” with “average annual emissions”.

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- Text was corrected in the abstract (and elsewhere).

p. 8356, l. 25 and elsewhere: Replace “green house” with “greenhouse”.

- This typo was corrected throughout the manuscript.

p.8356, l. 26: Could add a reference to support the claim that most CO2 emissions originate from cities.

- Reference (Rosenzweig et al., 2010) was added (L41).

p. 8358, l. 2: Crawford et al. 2011 and Bergeron and Strachan 2011 investigated the response of CO2 fluxes to environmental factors and to vehicular traffic (for the latter), which help explain seasonal variations. Hence, this sentence is an overstatement. Please reword.

- With this sentence we originally meant that none of the studies have analyzed differences between years or the factors affecting the observed differences. We now however removed the whole sentence as the lack of year-to-year variation is already mentioned in the previous sentence.

p. 8359, l. 13: I suggest the use of the classification by local climate zones (Iain D. Stewart, 2011, Redefining the urban heat island, PhD Thesis, University of British Columbia).

- As this study is not an urban heat island study and we do not have all needed information for the classification proposed by Stewart (2011), we rather use the more traditional one by Oke (2004).

p. 8359, l. 22. What are the surface characteristics of the road sector other than the presence of roads? Please give details on buildings and human activities (as this sector looks industrial). This information could be used in the discussion.

- The area behind the road is mainly residential buildings (block houses) and commercial buildings, but not that much of industrial buildings. Text “. . .and the area behind the

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road is covered with combined mix of residential and commercial buildings" was added in Section 2.1. As emissions from these are expected to be small, the usage of this information is limited in the discussion.

p. 8360, l. 27: I assume the "maximum covariance method" is used to avoid fixed delays between the IRGA and the sonic, especially for the closed-path IRGA which uses a long intake tube. For fluxes around zero, particularly in the case of low friction velocity, the maximum covariance method could yield unstable results, meaning fluxes are calculated using unrealistic delays. How the authors dealt with this?

- The referee is correct that the maximum covariance method can fail in finding the correct delay time. We have resolved this issue by defining a window width from where the maximum covariance peak is searched. If the peak cannot be found within this window, then the observed mean value for the lag time is used.

p. 8361, first paragraph: Could the authors provide information on energy balance closure at the site? Was the fluxes corrected for energy balance closure?

- The energy balance at the site has been studied elsewhere (Nordbo et al, 2012). The fluxes were not corrected for energy balance closure.

p. 8361, l. 12: Could add information on the slope and intersect of the open vs. closed-path regression.

- This was added in Section 2.2.1 (L166 – 167, see also comment for the other referee).

p. 8361, last paragraph: This paragraph is not clear to me. How was the error analysis performed? What does "11 % and 13 % of the data" mean? Was the detection limit used to filter out any data? Is this analogous to u* (friction velocity) filtering? Why one should expect the random error of the EC technique to be different in an urban environment?

- The error analysis is explained in detail in Nordbo et al. (2012). Uncertainty analysis is important for assessing the systematic and unsystematic uncertainties incorporated

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in flux measurements and so far urban studies have not given any details for those. Values for the detection limits are given in order to see how our measurement system is performing and those were not used in the filtering of the data. At our site we do not see the typical relationship between Fc and u* found often above vegetated surfaces and therefore u* filtering was not used. Differences in random errors between urban and less complex environments could arise from the differences in the turbulence structure, and the highly variable surface cover type.

p. 8362, l. 7: Meteorological sensors are generally badly exposed on rooftops: the building can influence temperature (hence relative humidity measurements) and wind fields (hence precipitation measurements). Can the authors provide additional information to help the reader assess the measurement error that these sensors are prone to? Also, careful interpretation is needed.

- Both relative humidity and precipitation measurements have been compared against SYNOP observations carried out by the Finnish Meteorological Institute next to our measurement site and no particular differences has been observed. In any case, precipitation measurements have large uncertainties when made with rain gauges. Unfortunately, no exact measurement errors can be given except those by the manufacturer. However, these were now left out from the manuscript.

p. 8363, l. 17: Were online traffic counts (4 km from the station) performed throughout the 2006-2010 period? If so, please state it. What does “online” stand for?

- The traffic rates were monitored throughout the analyzed period and this information is now given in the beginning of the paragraph (L205). Online means that the traffic counts are measured online with a method based on magnetic fields of vehicles rather than visual observations of the traffic rates.

p. 8363, l. 18-21: This sentence is not clear to me. Please give details on traffic count measurements performed near the station (frequency of sampling, period of measurement, etc.). If measurements were made hourly, N=840 means 35 days of data and

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if measurements were carried out in 2006-2007, does that mean that measurements were performed from dec 2006 to jan 2007? Also, TrSite and TrOffSite are not defined.

- Details related to the campaign-wise traffic monitoring have been added in the paragraph. Also abbreviations are now explained (L207 - 210).

p. 8362, l. 26: 531 h were gapfilled? Is so, please reword to make it clear.

- Yes, 531 hours were gap filled with the method described. The place of "(all together 531 hours)" was changed in order to get it clear (L215).

p. 8363, l. 10: How was soil respiration measurement sites chosen? Why lawns were excluded? Are these sites representative of the study area? Information on the cover fractions of the different vegetation types (meadow, forest, cultivated land, lawn, others??) could help the reader asses the representativeness of the measurement sites.

- The soil respiration measurement sites were chosen to best represent the different vegetation types around the measurement tower. Depending on the approximate surface cover fractions of each vegetation type (unfortunately no exact values are available), the number of soil respiration measurements on each vegetation type was chosen. However, more measurements were taken from cultivated land due to variety of plants and variations in fertilization. Text "The number of measurement points was chosen according to the approximate surface cover fractions with extra points on the cultivated land due to possible fertilization." was added in the paragraph (L231 - 233). Soil respirations were measured from lawns (four points) and were included in the points in cultivated land category. However, now these are given separately (L230).

p. 8364, l. 13. Please define RBS.

- This is now defined in this paragraph (L263) in addition to introduction where it was originally given.

p. 8364, l. 26; What are "fluffy variables"?

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- There was a typo and flyffy should be fuzzy. This has now been corrected. Also more information about the fuzzy variables is now given in the text (L276 - 280).

p. 8365, l. 21: What are the 20 variables? I can only trace back 17 of them (Fc, Ta, PAR, precip, RH, wind speed, season, time of day, WD1-9).

- 20 basic variables are traffic, Ta, PAR, year (four), time of day (four), WD1-9. Hopefully, the more detailed explanation of fuzzy variables helps to follow the text. The variables are also given in the text of Fig. 5.

p. 8367, l.2 and elsewhere: Major source of CO2.

- Was corrected in the text

p. 8367, l. 19-20: Could the authors give more details on the meteorological condition classes used for the look-up table.

- Text "Depending on the prevailing wind direction, three surface roughness values representing built, road and vegetation areas were considered in the look-up tables. Wind speed was divided into 8 classes, standard deviation of the lateral wind speed into four classes and atmospheric stability into seven classes." was added in Section 2.4 (L364 - 368).

p. 8368, l. 16: ". . .coldest and longest winter" since when?

- Coldest winter from the five years of analyzed data. Text "from the analyzed years" was added to the paragraph (L392).

p. 8368, l. 24: Please check PAR units as they are inconsistent with the rest of the manuscript. Also, my experience is that PAR sensors can yield small but positive values at night (but rarely negative values unless the sensor has drifted) and a threshold of 5 or 10 μmol m-2 s-1 is commonly used. Hence, a threshold value of 0 seems too strict and nighttime datapoints could be classified as daytime. Was this assessed by the authors?

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- Units were corrected to μmol m-2 s-1 . The authors agree the referee with the limit of PAR. At our site the limit for the nocturnal positive PAR values is below 5 μmol m-2 s-1, and therefore this will be used as limit for night-time. Fig. 4 was re-plotted with the new limit. Similarly, this limit was taken into account in Fig. 10 and in the fit between nocturnal EC fluxes and air temperature. Fig. 11 was also re-plotted.

p. 8369, l. 1-3: I'm not sure about the logic of this sentence. Downward fluxes are a result of (CO2 exchanges over) an area with high vegetation cover fraction being downwind of the measurement site. Wind direction is used only to discriminate what is downwind (i.e. the source area) of the flux tower but is not a causal factor per se.

- The sentence was rewritten as "Most of the downward fluxes in summer originate from the area of high fraction of vegetation cover (Fig. 2b). This direction is also the most commonly observed wind direction in Helsinki" (L406 - 407).

p. 8369, l. 25-27: Summer CO2 uptake at the SMEAR III station is very comparable to what has been observed at the Montreal suburban site by Bergeron and Strachan (2011). Could be worth mentioning.

- Sentence "Similar daytime uptake (-7 μmol m-2 s-1) was observed at the suburban area in Montreal where same fraction of vegetation cover is observed (Bergeron and Strachan, 2011)" was added (L431 - 433).

p. 8370, l. 10-15: This paragraph could be left out as it repeats information that appears in the Methods section.

- The text in the beginning of Section 3.3 was shortened.

p. 8370, l. 21: "ANNtraffic is the only method able to simulate as high as 50 μmol m-2 s-1 fluxes": in Fig 7a, I cannot see modeled Fc above 40 μmol m-2 s-1. Please resolve the discrepancy. Also, could the authors explain why all the gap filling techniques used seem to be bounded?

- There was a typo in the text and 50 μmol m-2 s-1 should be 34 μmol m-2 s-1. This

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has now been corrected in the text. Also, text related to the inability of the models to predict extreme values has been added in the paragraph (L455 - 457).

p. 8371, l. 22-24: Bergeron and Strachan 2011 report results from two sites in Montreal, one being urban and the other, suburban. It should be stated clearly which one is presented in Table 4 (see comment below). Better, both sites should be presented.

- Both sites are now given in Table 4. Text “Similar emissions with 1420 g C m-2 have been measured from the suburban site in Montreal, where the fraction of vegetation is also 50% (Bergeron and Strachan, 2011)” was added in the paragraph (L491 - 493).

p. 8372, l. 4: “representative” of what and “less representative” than what? I assume “less representative of the annual average than the estimation obtained from long-term measurements as presented in this study”. If so, how is the representativity of the SMEAR III site, with regards to average annual emissions, affected by the presence of heterogeneous source areas around the flux tower? See first paragraph of section 3.4 regarding lower 2009 emissions. These elements should be discussed in more details.

- With the sentence we wanted to say that if there is only one or two years of measurements it might be that those years have been unusual and therefore may not represent the site correctly. The text has now been reworded to “In addition, in most studies the annual estimates are based on 24 or less months and representativeness of the measurement periods are not clear as information about the year-to-year variations are not available.” (L498 - 500)

p. 8372, l. 18-20: Measurement location affects annual CO2 emission estimates, not the emissions per se. Please reword. On the other hand, one major point of this study is that CO2 fluxes from sectors with different surface characteristics can be studied using one measurement site located at the interface of such sectors. Now, the conclusion of the paragraph is that several measurement sites are needed. Can the authors resolve this apparent contradiction here and in the conclusion?

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- Text was modified in the paragraph and in the conclusions. Also the difference in emissions from urban and suburban sites in Montreal has been added for comparison (L514 - 515).

p. 8372, l. 21-28: What is the point of this paragraph? Either clarify or edit out.

- The point of the paragraph is to get information how carbon emissions have varied from the different surface cover areas within the studied years. The text in the paragraph was modified for clarification (L515 - 518).

p. 8373, l.9: Is it reasonable to assume the same fit is valid throughout the year? For example, do we know if the emission factors can be temperature-dependant (higher emissions per vehicle in winter)?

- This is a good point as e.g. the aerosol particle emission factors from traffic increase with decreasing temperature (manuscript in preparation). A sentence "However, as the annual CO2 emissions from traffic are based on a winter time fit, the results should be considered with caution" was added (L551 - 553).

p. 8373, l. 13-15: Is it plausible that CO2 sources and skins in the road sector, other than traffic, cancel each other out? Details on surface characteristics (other than road) and human activities of the road sector would be helpful in that regard.

- As now mentioned in the site description, the amount of other sources is minimal in the area and therefore we do not separate here the different sources/sinks.

p. 8374, l.2: How would emission factor estimates be affected if EC data outside wintertime were used?

- See comment above.

p. 8374, l. 9-10: "possible indicator for changes in fuel content and age structure of the vehicles": this conclusion is purely speculative, since emission factors derived from EC measurements suffer rather large uncertainty due to the methodology used (par-

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ticularly the footprint analysis), and should be left out unless the authors can provide support from additional references. Are the authors aware that such changes in fuel content and/or age structure of the vehicle fleet have occurred over the last years? If so, the authors should provide appropriate references.

- Change in age structure of vehicles and in fuel content (particularly the increased usage of biofuels) has been reported to occur in Finland in studies made by Ministry of Transport and Communications. Reference was added (L575-577).

p. 8374, l. 11&19: 3 Ns missing: November 2008, November and Note.

- We do not understand this comment. Ns in each of the three words exists so maybe there was a printing error?

p. 8374, l. 21: Please be careful with this kind of phrasing that suggests causality. Fsoil has been related to Tair using an exponential fit. Please reword here and elsewhere.

- The text was modified to “Soil respiration increases with increasing air temperature following an exponential relationship F_Soil=(2.1±0.8) eˆ((0.06ãĂŰ±0.03TãĂŮ_air)) (RMSE = 0.9 μmol m-2 s-1, R2 = 0.65)” (L589 - 591). Text related to the linear fit between Fc and Tr in Section 3.5 was also modified (L541 - 542).

p. 8374, l. 10-13: It has been shown that comparison between (manual) chamber measured soil respiration and EC-derived ecosystem CO2 fluxes is not trivial for a number of reasons including source area mismatch, time of day sampled, chamber design and handling, CO2 storage and advection, energy balance closure, low friction velocity night-time period, parameterisation and annual budget estimation (e.g. Goulden et al. 1996, GCB 2:169, van Gorsel et. 2007, Tellus 59B:397). Few of these reasons are mentioned in, or could be assessed from, the paper while others cannot be evaluated due to the lack of information. Although a thorough discussion on the matter is out of the scope of this study, it is fair to say that EC fluxes generally tend to be underestimated as compared to chamber fluxes. Hence, it should be made clear that

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the estimation provided (soil respiration accounts for 63% of the NET annual carbon emissions) is a rough approximation that likely represents an upper bound.

- We agree that comparison between EC and chamber measurements of CO2 flux is not trivial due to the above mentioned reasons. We have now added a sentence "Generally EC method has been found to give lower Fc than chambers (Goulden et al., 1996; Launiainen et al., 2005)" (L596 - 597). In addition, we have modified the text related to 63% contribution to "Thus, as a rough estimate soil respiration can be estimated to account 63% of the annual carbon emissions from the area" (L606 - 607).

p. 8375, l. 15-17: Christen et al. (2011) used a modeling approach based on soil temperature and water content, parameterised using summer and winter data, for a site with lower vegetation cover fraction under a different climate. The explanation provided is incomplete and should be reviewed.

- The paragraph was largely rewritten according to the comment (L610 - 615).

p. 8375, l. 19-20: References are needed. Or replace "in a northern city" with "over the studied area".

- "A northern city" was replaced with "over the studied area".

p. 8375, l. 25: the daytime summer Fc...

- Text was corrected.

p. 8376, l. 9: The first part if the sentence suffers from bad wording, please reword.

- The sentence was rewritten: "air temperature has smaller correlations coefficients than PAR in daytime".

p. 8376, l. 9: This sentence suffers from bad wording, please reword. References needed.

- We assume that the referee means lines 11-12, where the sentence was rewritten:

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"...soil temperature has been found to have highest correlation coefficients with soil respiration..." and reference was added (L640 - 642).

p. 8376, l. 13: Soil temperature is tricky to properly measure/estimate in urban environments anyway.

- Yes, we agree with the referee.

p. 8376, l. 16-17: Bergeron and Strachan 2011, Christen et al. 2011 and Crawford et al. 2011, among others, provide valuable information to discuss seasonal variation of urban CO2 fluxes as related to environmental variables. This sentence should be reviewed.

- A text "correlation coefficient" was added in the sentence (L645).

p. 8376, l. 18-...: I would edit this paragraph out as it does not add much to the paper. The discussion looks a lot speculative as it is not supported by references on plausible mechanisms. For example, I would expect vegetation respiration to be mostly unresponsive to temperature when water limited, so I do not get the reasoning behind the last sentence of the paragraph. Also, results are not fully disclosed (r coefficients per month per year) and limits the reader's assessment.

- The paragraph was removed as suggested by the referee. Also the title of the header was changed to "Environmental controllers". However, as one point of the manuscript is to study variations between different years, some parts of the text were moved to Section 3.4 (L520 – 535).

p. 8378, l. 2: Please precise what "other variables".

- "other variables" was replaced with "PAR and air temperature" (L673).

p. 8378, l. 10&20: I suggest avoiding the use of symbols and acronyms in the conclusion section.

- "AnnBE" was replaced with "annual biased errors".

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p. 8378, l. 22-24: Unclear. Do you mean annual estimations from EC measurements? Precise what “more attention” means.

- Sentence was modified to “given to the calculation of annual estimations of CO2” (L694 - 696).

p.8379, l.14: Replace “and” with “an”.

- Text was corrected.

Table 1: Correct azimuth angles given for the vegetation sector.

- These are corrected in Table 1.

Table 4: For Montreal, NEE is from the urban site while Fveg is from the suburban site. Please correct.

- Both of the sites have now been added in Table 4 and the annual emissions and vegetation fractions have been fixed.

Fig 9 caption: “as a function of”.

- Text was corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 8355, 2012.

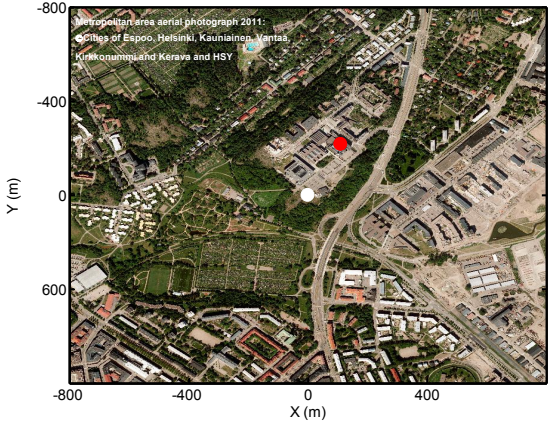

Fig. 1.

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Interactive Comment

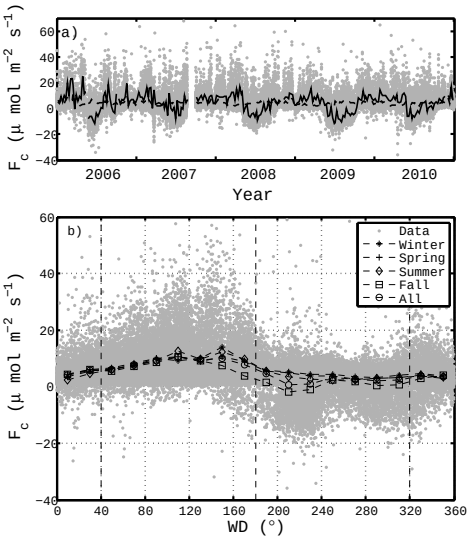

Fig. 2.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

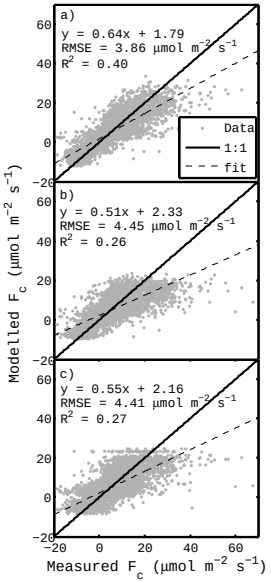

Fig. 3.

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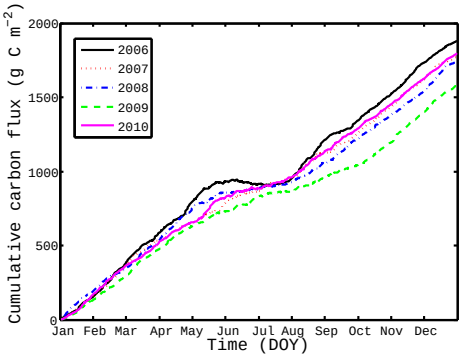

Fig. 4.

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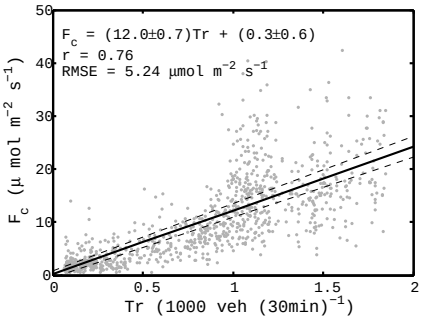

Fig. 5.

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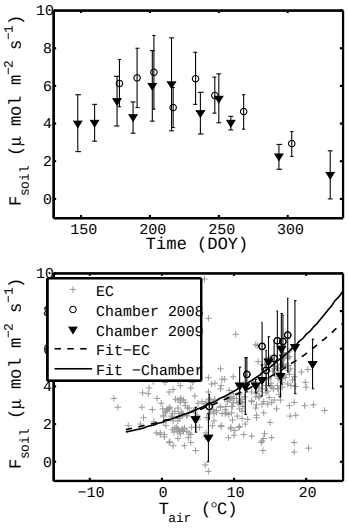

Fig. 6.

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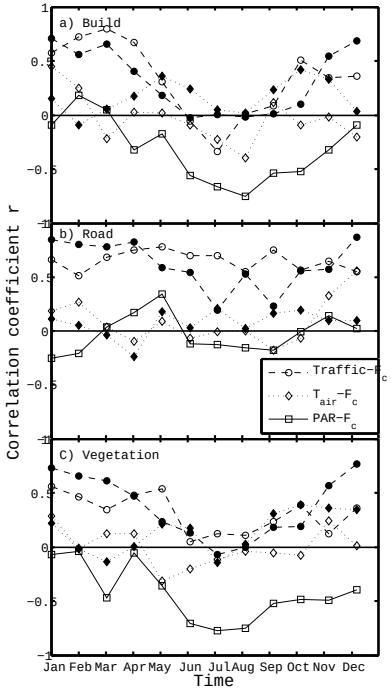

Fig. 7.

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