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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.

Anonymous Referee #2

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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 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.

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.

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

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

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

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.

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).

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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.

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?

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

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

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?

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.

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?

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 measure-

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ment, etc.). If measurements were made hourly, N=840 means 35 days of data and 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.

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

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.

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

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

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).

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

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

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

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?

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

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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.

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.

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

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?

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.

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.

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

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)?

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.

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

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 (particularly 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.

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

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.

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 nighttime 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 eval-

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

p. 8374, 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.

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

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

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

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

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

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.

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

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per month per year) and limits the reader's assessment.

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

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

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

p.8379, l.14: Replace "and" with "an".

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

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

Fig 9 caption: "as a function of".

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