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Seasonal and annual variation of carbon dioxide surface fluxes in Helsinki, Finland, in 2006–2010

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

CO_2 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 CO_2 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 CO_2 exchange is of high interest by evolving urban CO_2 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 CO_2 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 process modeling is an issue which needs to be discussed with more profundity within the paper (and the scientific community in general).

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 F_c 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 F_c sufficiently. Maybe the advantages of the used method have to be discussed more deeply within the paper.

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 have been performed to ensure steady state conditions?
4) The behavior of F_c 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?
5) Exchange sums are given in g C m^{-2} a^{-1}. Since only CO_2 fluxes have been measured and no CH_4-C or other OC exchange was quantified the usage of this unit is a bit misleading. Although it is used in terms of CO_2 fluxes over vegetated areas in nearly all papers (since CO_2 dominates the exchange of C in the ABL) the usage of g CO_2 m^{-2} a^{-1} should be considered.

Specific comments

1) Abstract, line 20: “the without” has to be “the one without”
2) 8358, 19: “Kordowski and Kuttler (2008)” must be “Kordowski and Kuttler (2010)”
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?
4) 8361, 12: Given the linear regression between open and closed path fluxes – what is the slope and the offset, respectively?
5) 8361, 13-18: Have the data gaps been analysed for a day/night-bias, i.e. if there were probably more gaps during nighttime due to quality assurance? If yes, does this raise any issues when performing the ANN training?
6) 8362, 4: “in the measurement tower” has to be “at the measurement tower”. Given the 31 m T_a for ANN training one can suppose a near-ground measured T_a would be more appropriate to explain influences of meteorology on F_c. Was there a near-ground T_a (2 m) available and, if yes, why was it not used?
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 T_s as being unfortunately in a later part of the paper. The reviewer fully agrees.
8) 8364, line 17: The term “overfitting” should be explained shortly since this is ANN specific and may be new to some readers.
9) 8364, line 26: Although it is, to my opinion, worth thinking about what “fluffy variables” could look like in terms of ANN modeling, I suppose this must be read “fuzzy variables”.
10) 8368, line 5-6: “daily average minimum” should be read “minimum daily average”.
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 issue should be clarified.
12) 8377, 2: The explanation of the differing CO_2 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 F_c. Drought conditions instead are supposed to have a weakening effect on the carbon sink strength (which maybe is the case in August 2006).

13) Table 1: Sector degree information for “Road” and “Vegetation” is duplicated.
14) Fig. 11: Filled symbols for nighttime data are not explained in the legend and it is hard to distinguish between the symbol shapes. The layout should be reconsidered.