

Interactive
Comment

Interactive comment on “Eddy covariance measurements of CO₂ and energy fluxes in the city of Beijing” by H. Z. Liu et al.

Anonymous Referee #3

Received and published: 12 May 2012

MAJOR COMMENTS

This paper presents an interesting and valuable long-term dataset of primarily carbon-dioxide fluxes from a unique urban surface (dense residential high-rise) which is not yet discussed in the literature. Unique is the discussion and control of emissions during the 2008 Olympic Games but as Reviewer #1 has already mentioned, the same data is published in Song and Wang, Atmos. Research, 106, p. 139-149. I am wondering to what extent the analysis repeated here adds to this aspect.

Despite the quality of the measurements and the long-term dataset is excellent, the analysis and discussion must be improved in the following points, before the paper can be accepted:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

1) The incorporation of the ‘energy fluxes’ is not satisfying. The authors essentially present and discuss turbulent fluxes of sensible heat and latent heat only, and they omit the driving input, net all-wave radiation and also storage heat fluxes and anthropogenic heat fluxes that are all part of the urban energy balance. The title refers to ‘energy fluxes’ (which includes radiative, storage, and anthropogenic heat fluxes), but essentially only turbulent sensible and latent heat flux densities are reported. I am wondering if the manuscript should contain the turbulent fluxes of sensible heat and latent heat at all. The paper is almost entirely about CO₂ fluxes, not sure if the energy balance sections are necessary.

2) Calculations of fluxes: (2.1) The data processing has been documented quite carefully and seems to follow a rigorous protocol. However it is quite likely that given the height (47m) and the relative high frequency of stable conditions, the EC level decouples from the surface. As a consequence, the timing of fluxes could be incorrect because authors have not accounted for any storage correction (concentration change below measurement volume, see Feigenwinter et al., 2012 regarding the importance of this term -> “Eddy Covariance - A Practical Guide to Measurement and Data Analysis” - Chapter 16 on urban EC measurements). There is no justification given why a storage correction has not been incorporated or a discussion of what are the implications of omitting this correction. (2.2) The u^* threshold discussion should be more prominent and supported by data (see also other reviews) - the high frequency of stable conditions suggests that this could be relevant. Omitting any u^* correction should be justified and discussed in more detail in the article.

3) Filtering: (3.1) The MDV approach (l. 203) is not appropriate / justified if the adjacent days are a weekend and the day to be filled is a weekday or vice versa. It could be assumed that the emission patterns and the magnitudes on weekdays and weekends are quite different (and authors demonstrate this). (3.2) The model by Hui et al. (2003) has been developed for non-urban surfaces and it is highly questionable if the controls of emissions in the current urban ecosystem are indeed air temperature (maybe for

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

space heating requirements), humidity (why?), wind direction (OK). But what about day-of-week?, time of day? traffic load?. The gap filling section needs a more careful description and discussion. (3.3) The reader should be convinced that the gap filling works properly, for example - artificially (randomly) removing data and then quantifying how well the gap-fill model performs relative to the actual (artificially removed) dataset.

4) The authors should provide more spatial and temporal information about the controls on emissions - The quality of the manuscript's discussion is lessened because a number of conclusions about the controls on F_c are not proven but rather just speculated - including heating requirements, traffic, behavioral patterns, cooking etc. Many of the speculations listed could be relatively easily verified using appropriate data (heating degree days, a survey of heating systems, population density around tower, traffic counts on highways). The paper unfortunately lacks a rigorous discussion of the properties and metabolism of the surrounding urban area that control fluxes. I agree with both previous reviewers that this aspect should be improved. 5) Units of CO₂ flux densities - Generally, all units of the carbon-dioxide flux densities that are expressed in mass must include if the mass refers to CO₂ or C only (Example: l. 245: $\text{mg m}^{-2} \text{s}^{-1}$ must become $\text{mg C m}^{-2} \text{s}^{-1}$ OR $\text{mg CO}_2 \text{ m}^{-2} \text{s}^{-1}$ (whatever authors have calculated). An alternative would be to provide fluxes in $\mu\text{mol m}^{-2} \text{s}^{-1}$ (and on l. 270 and 273 $\mu\text{mol m}^{-2} \text{s}^{-1}$ is mostly used, but on line 274 it is again mg CO_2 OR $\text{C? m}^{-2} \text{s}^{-1}$). For the annual totals, I suggest to stay with $\text{kg C m}^{-2} \text{y}^{-2}$ (e.g. l. 357) because that is the usual unit in the literature.

MINOR AND EDITORIAL COMMENTS

I agree with all the excellent minor and editorial comments made by anonymous reviewer 2 (RC C1781: 'Review', Anonymous Referee #1, 23 Apr 2012 - as annotations in the pdf). In addition to those:

Title - The manuscript does not quantify the total emissions from the city of Beijing, but rather quantifies fluxes from a specific neighborhood. The title should be modified

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

accordingly. (line numbers refer to the line numbers on the print layout) l. 11 (Abstract) - “Long-term measurement of carbon dioxide flux (F_c) AND THE ENERGY BALANCE”

l. 12 (Abstract) - “in the Beijing megacity” -> “in Beijing, China”. The abstract should make clear that fluxes are from a neighborhood and not the entire city.

l. 14 ff. (Abstract): The energy balance partitioning between latent and sensible heat flux is summarized for summer and spring, but no indication of fall and winter is given.

l. 17 - 19 (Abstract): Sentence requires editing.

l. 21 (Abstract) - “automobile traffic” - What supports the fact that only automobiles are relevant and not busses, freight trucks and motorcycles?

l. 26/27 (Abstract) - “Total annual average CO₂ emissions were estimated... “ -> “Total annual average CO₂ emissions in the source area of the tower were estimated... “

l. 37 ff. - “Emissions of greenhouse gases (GHG)”. Even without a city there could be possibly emissions of GHG. Authors should say enhances emission / emissions associated with the combustion of fossil fuels.

l. 42 - 2005 - > update to 2006 - 2009 period.

l. 43 - Change °C to K (for differences the SI system requires the use of K)

l. 51 - complex morphological nature -> be more encompassing, it is the “complex land cover, morphology and metabolism of the urban ECOSYSTEM” (I propose to exchange “Urban Landscape” by “Urban Ecosystem”). In addition it is also the arrangement of emission sources and sinks that makes measurements challenging.

l. 136 - “surface cover sectors”. The terms refer to the Stewart and Oke LCZ scheme and as such authors should rather say four different “Local Climate Zones”

l. 152 -> ‘Constant flux layer’ -> more common is the term “Inertial sublayer” see

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Rotach 1999 for example.

I. 154 -> to assess flow distortion, in addition to the length of the boom, the structure of the tower (open, filled) and the tower's diameter should be given.

I. 163 - 165 - Anemometers are not further used in this manuscript, so why list them?

I. 200 - "approximately 70%". Other losses are given with 1 significant digit, why not here?

I. 215 - 217 - "... monsoon ..." -> not needed in the context of this manuscript. Rather define which months are dry season and which ones are wet season (see below).

I. 234 - "Le" should be " λE " where " λ " is a greek lambda symbol. No space in-between. Applies to all following instances of "Le".

I. 237 - which months are referred to as "dry season"? Which months are the rainy season? I. 248 - Domestic heating - what is the dominant heating system (natural gas and oil released locally, or power / steam that is associated with CO2 emissions outside the study area?

I. 283 - What justifies that home cooking is the source of CO2 and not the start of the heating systems as radiation and air temperature decrease towards the evening?

I. 321 - I understand that the emission reduction of CO2 was not the motivation factory closures and traffic restrictions, but rather a reduction of air pollutants. So the sentence 'where effectively reduced' is not appropriate, authors could maybe say "as a side effect".

I 337 - I expect there must be some traffic counts available in Beijing to support and quantify the "much lower" traffic on cold winter nights. (Same applies to summer I. 339 -> quantify traffic load differences).

I. 368 - not only "annual scale", but also "inter-annual variation".

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive
Comment

I. 372 - “is partially moderated by urban vegetation” - this statement is not supported in the results. What is the evidence that there is uptake by vegetation?

I. 378 - by $2.28 \mu\text{mol m}^{-2} \text{s}^{-2}$ -> Also provide the relative reduction compared to other years (keep the absolute reduction)? Could this be used to infer the overall contribution of the traffic?

I. 381 - It is unclear how the values obtained in this study can be used “for urban development and help to shape policies” and I disagree that they are directly useful. The results and findings could be used to validate emission models, but this would require a spatial analysis. Therefore, I suggest to remove this part of the conclusions.

Figure 1 - requires “%” unit added to 50 and 90. Labels for where the Beijing-Tibet expressway is, and the part that is referred to in the text would help. Is the Korman and Meixner Source Area Model applicable in this morphology?, and what are the inputs? -> should be discussed in the manuscript.

Figure 9 - “Olympic GameS” (add “s”) and is 2006 - 2009 including 2008? or 2006, 2007 and 2009?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7677, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)