

Interactive  
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## ***Interactive comment on “The carbon emissions of Chinese cities” by H. Wang et al.***

**H. Wang et al.**

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We thank the reviewer for the valuable comments and suggestions to improve this manuscript. We have carefully read through the comments, and summarize them into the following eight major points according to our understandings. The point-by-point response will be presented here, and we would also like to explain our plan of revising this manuscript.

1. This paper is a useful, if rather prosaic, analysis of carbon emissions in Chinese cities, as compared with other world cities. The authors present their quantitative estimates, but seem reluctant to delve deeply into the reasons for differences and the implications of their findings. The comparison with non-Chinese cities is adequate, as I believe the methods, and indeed the cities themselves, may be sufficiently different as to render the comparisons of only mild interest. My overall recommendation is that the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



authors take time to extend their analysis beyond the basic presentation of emission estimates.

Response: We appreciate the reviewer's comments that this paper is a useful analysis of carbon emissions in Chinese cities, as compared with other world cities. The main objective of the present paper is to develop comprehensive and comparable carbon emissions inventories of twelve Chinese cities and analyze their emission characteristics. As far as we know, this is the first systematical accounting of GHG emissions at the city-scale based on a bottom-up methodology in China where the emissions are also compared with global cities. The paper could present the reader with basic information of the carbon emissions of Chinese cities, and the results are helpful for the local governments to design strategies to reduce their emissions.

We understand the reviewer's criticism that the present paper looks some kind of short and simple. But a paper usually focuses on some special topics and could not answer all the questions. It is usually restricted by many factors (such as the basic data availability, research objective, and etc). We try to make the most of available published data to reach our research objectives in this study. Actually, some questions (e.g. embodied emissions at city level) raised by the reviewer are also what we are interested in and deserved making further studies on. We have also indicated this in our reviewed manuscript (last paragraph in P7999).

However, we accept the reviewer's suggestions and will add more discussions when we revise this manuscript. The revision plans for this manuscript will be explained in more details in the following responses.

2. One key issue that is not adequately dealt with relates to the size and composition of the Chinese cities, as compared with the non-Chinese cities. As the authors mention on p 7989, line 5-6, the jurisdictional extent of the Chinese cities is very large (some estimates of area or maps of sizes of the cities would be helpful) and encompasses not only the urban center, but also rural areas and small towns and villages. This is not the

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



Interactive  
Comment

way the non-Chinese cities are defined. But the implications of these different definitions are not adequately discussed. For one thing, it inflates the population estimates and improves the per capita energy consumption for the Chinese cities, because the “extra” people are in rural areas with much lower energy usage than the city dwellers. It also means that these “cities” will find it harder in the future to improve their per capita energy use and reduce their per capita carbon emissions, as people in the rural areas and towns and villages improve their standard of living. This issue needs to be discussed: If China plays up the low level of development and low per capita GDP and carbon emissions today, it will find it harder to show improved carbon levels in the future as development spreads nationwide. I’m also curious as to whether migrant workers are included in the population estimates. They are an important component of the social infrastructure for manufacturing and services in many of these cities and should be included to ensure all parameters are compared on a level playing field. Please explain.

Response: We agree with the reviewer that the broader definition of Chinese cities could have some impacts on our comparison with the other cities in the world. As the reviewer mentioned, this will induce the underestimation of per capita energy consumption for the Chinese urban area, because the “extra” people are in rural areas with much lower energy usage than the urban dwellers. However, the definitions between “city” and “urban area” are different in China. And as far as we know, China’s published statistical materials are focus on cities but not urban areas. The availability of statistical data on urban area in a specific city is very poor, especially for the energy consumptions, industrial products and etc, which directly determine the calculation of carbon emissions. Therefore, it is impossible to calculate the carbon emissions from the urban area of each Chinese city at this stage. We would like to add more discussions about this when we revise this manuscript. Of course, although we have not come across any published statistical data of urban energy balance and industrial production for Chinese cities, it will be great help if the reviewer have some and could share with us the references.

Interactive  
Comment

We agree with the reviewer's comment that the per capita carbon emissions are hard to reduce in China in the near future. On the contrary, the per capita carbon emissions will increase as development spreads nationwide. Actually, this has already happened during the past five years for the cities in China (Wang et al, 2011; Bi et al, 2011). We will add a figure and some discussion about this when we revise this paper. Therefore, although carbon emission intensities (per GDP carbon emissions) will decrease gradually for most Chinese cities, the total and per capita carbon emissions will still increase in the near future.

The population used in this study means the resident population, which include the domicile and non-domicile (including the migrant workers) populations. Therefore, the migrant workers are included in the population estimates in this study.

## References:

Wang, H., Zhang, R., Bi, J., Liu, M., Wu, Y. 2011. China Urbancarbon emission Accounting: Case study of Wuxi City. *China Environmental Science*, 31: 1029-1038

Bi, J., Zhang, R., Wang, H., Liu, M., Wu, Y. 2011. The benchmarks of carbon emissions and policy implications for China's cities: case of Nanjing. *Energy Policy*, 39: 4785-4794.

3. Biofuel A related matter concerns biofuel use. I did not see biofuel use mentioned in the paper, but rather the term commercial energy consumption. I assume, therefore, that biofuel use is not included, but it will certainly be a common energy source for the rural inhabitants of these cities. I think it should be included (statistics are available, though uncertain), as it is definitely a source of CO<sub>2</sub> emission and one that could be reduced by future energy transformations. It is not adequate in this context to say that CO<sub>2</sub> emissions from biofuel use are not relevant because the CO<sub>2</sub> emitted was previously absorbed in the growth of the vegetation; first, because of inefficient combustion in the burning of crop residues in stoves, there is no one-to-one relationship between CO<sub>2</sub> uptake and CO<sub>2</sub> release; and, second, the carbon balance will be disrupted as

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

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Interactive  
Comment

fuels are upgraded in these communities in the future. I think a comprehensive analysis of the carbon situation in these Chinese cities should face the issue of biofuel use directly, and perhaps also the burning of agricultural residues in the field. This still happens in China in areas very close to urban centers and certainly, I believe, within the boundaries of the cities chosen for this study.

Response: In China, biomass use (including biofuel and open burning) contributes nearly 14% of the total CO<sub>2</sub> emissions (including fossil fuel use, cement production and biomass use), and the contribution shares varied from 0 to 45% for different provinces in 2005 (Zhao et al, 2012). We agree with the reviewer that biofuel use is a source of CO<sub>2</sub> emissions (Levine, 1994). However, it is largely balanced by vegetation re-growth over the next year (Houghton and Hackler, 1999). Thus, the contribution of biofuel use is often omitted in long-term analyses of atmospheric CO<sub>2</sub> data (Yantosca et al, 2004; IPCC, 2006). As carbon emission inventory at the city-level is more valuable for local carbon management, which focuses more on long-term results, we didn't consider GHG emissions from biofuel use in this manuscript. Furthermore, there does exist some statistics about the biofuel use at China's national and provincial levels until 2007, however, we didn't find similar data for the cities. It would be great help if the reviewer has some information about biofuel use at city-level and could share with us the references.

It should be noted that biomass burning is an important seasonal source that must be included in analyses of short-term data, especially for the air pollutant emissions inventory. However, that's a different scientific problem.

#### References:

Yantosca, M., Xiao, Y., Evans, J., Streets, G., Vay, L., Sachse, W. 2004. Improved quantification of Chinese carbon fluxes using CO<sub>2</sub>/CO correlations in Asian outflow. *Journal of Geographical Research*, vol. 109, D18S18, doi:10.1029/2003JD004362.

Levine, J. 1994. Biomass burning and the production of greenhouse gases, in *Climate*

Full Screen / Esc

Printer-friendly Version

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Discussion Paper



Houghton, R. A. and Hackler, J. L. 1999. Emissions of carbon from forestry and land-use change in tropical Asia. *Global Change Biol.*, 5, 481– 492.

IPCC (Intergovernmental Panel on Climate Change), 2006. IPCC Guidelines for National Greenhouse Gas Inventories; Prepared by the National Greenhouse Gas Inventories Programme, Eggleston, H. S., Buendia, L., Miwa, K., Ngara, T., Tanabe, K., Eds.; IGES: Japan.

Zhao, Y., Nielsen, P., McElroy, B. 2012. China's CO<sub>2</sub> emissions: How accurate are available estimations, recent trends, uncertainty quantifications and provincial distribution. Submitted to *Atmospheric Environment* (in revision).

4. CO<sub>2</sub>e It is not necessary or appropriate to refer to the emissions as carbon dioxide equivalents, CO<sub>2</sub>e (why subscript “e” anyway?). This is normally used to mean that gases other than CO<sub>2</sub> are included (like CH<sub>4</sub>) and converted to CO<sub>2</sub> equivalents using radiative forcing measures. I think this study only includes CO<sub>2</sub>, and therefore the use of the term “equivalents” is unnecessary.

Response: CH<sub>4</sub> emissions from landfill waste, CF<sub>4</sub> and C<sub>2</sub>F<sub>6</sub> emissions from aluminum production, N<sub>2</sub>O emissions from nitric acid production and etc were calculated for Chinese cities in this study, and they were converted to CO<sub>2</sub> equivalents. We will add some explanations in the revised manuscript to make it clearer to the readers.

5. Electricity I think a little more explanation is needed about the treatment of electricity. I understand what the authors did, but it's not clear to me that using the electricity mix of a very wide grid-based region is appropriate. Is it true that a Chinese city receives an “average” electricity mix from a huge service area and not electricity mainly from one or more local power plants within its boundary? I wanted to check the source of these fuel mixes, but the reference (Page 7992, line 12) is a number (19) not a name

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

and therefore of no use. This needs to be corrected. Is the assumption made by the authors the same as in analyses of the other world cities? It seems to push perhaps a sizeable amount of CO<sub>2</sub> emissions from the city into surrounding regions, where it is not completely accounted for. Is self-generation of electricity by industrial facilities included?

Response: Thanks for the reminding. The (19) should be (SSBC, 2009) which has already been listed in the references of the reviewed manuscript. The materials are in Chinese. Of course, we could present the data of fuel mix of each major power grid for reviewing, if the reviewer wants to check it.

The city does receive electricity from the power grid in China. Therefore, we use the average emission factor of each power grid to related city in this study. In some Chinese cities, especially for the developed cities (like Shanghai, Beijing, Hangzhou, and etc), the local power plants within its boundary could not present enough electricity for their production and consumption and they have to import some electricity from outside. The other cities in the world also calculate their electricity related carbon emissions from the consumption perspective, which is the same as ours.

The self-generation of electricity by industrial facilities has already been included in the sector of industrial energy in our study.

#### References:

State Statistical Bureau of China): Electricity Yearbook of China 2009, China Statistics Press, Beijing, China.

6. Embodied emissions The issue of manufacturing goods for export is an important one and deserves more explanation and perhaps some analysis. It affects all parameters: emissions, GDP, and even population. In at least some Chinese city yearbooks, it is possible to extract information on the production of goods for export, so it might be possible to separate a city's activities into those for export and those for domestic

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



(i.e., within China) consumption. It would be interesting to see how that has changed over time and affected the carbon emissions in cities. There is a great deal of interest in the research community these days about carbon emissions embodied in international trade and how this should be handled in climate treaties (the work of Peters, Hertwich, Matthews, etc.). This paper could delve a little more deeply into industrial-CO<sub>2</sub> and how it divides into export and domestic components in each city. This type of thing is why I initially characterized the paper as “prosaic”: when opportunities arise to address key issues, the authors of this paper do not pursue them.

Response: We agree with the reviewer for addressing an important issue of embodied carbon emissions, which we also mentioned at the end of the reviewed manuscript (P7999). Just as the reviewer states, a great deal of interest in the research community about carbon emissions embodied in international trade. However, all this kind of researches focus on emissions at the national level, because there exist some published statistical data at this level. If you want to process similar researches at the city level, it would be too difficult in China now, because there are no such kinds of basic data to support this kind of research. Furthermore, the embodied emission is another type of topic which has already been out the range of this study. This is why we indicate this merit future study at the end of the reviewed manuscript.

7. Inter-city analysis It could use some sections of deeper analysis with additional disaggregation of data and graphical representations of inter-city differences in parameters that can then be the basis of conclusions about the importance of different factors affecting carbon emissions in Chinese cities. Another type of creative analysis would be to inter-compare the results for the twelve cities to elicit understanding of the various factors influencing carbon emission and its relationships with population and economy (alluded to on Page 7996, lines 9-14). How important, for example, is geographical location and hence heating/cooling demands? I don't personally find Table 1 very helpful in explaining trends and relationships, as discussed for example, in Section 3.3. I think a couple of creative graphical presentations would be more helpful – perhaps scatter

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

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Interactive  
Comment

plots for all the cities or time trends of calculated parameters. And in 3.3, I don't think GDP is the correct term (which usually is applied to countries, hence "domestic") and something like "urban economic production" is needed. Also, if Table 1 is to remain, please do not cite any numbers to seven significant digits! And the column "sig", which I am not clear on, seems pointless, as there are only two non-zero values: : : To help the analysis of industrial activities and emissions, I would separate power generation and industrial energy consumption. It is mentioned that the downward trend for Wuxi between 2007 and 2008 may be associated with economic recession affecting exports. Why is this same trend not seen for Guangzhou or even Shanghai, which also have large export activities?

Response: There are already some discussions about the difference in carbon emissions of Chinese cities in the reviewed manuscript (Section 3.1-Section 3.3). However, we would like to add one section named "Inter-city comparison of China" in our revised manuscript to separately discuss the differences in carbon emissions of Chinese cities and analyze the importance of various factors, such as geographical location according to the reviewer's suggestions.

We consider Table 1 is more concise and include more information than the scatter plot figure, so it is to remain but the significant digits of the data will be revised according to your suggestion. "Sig" is a statistical parameter, which could simply reflect the possibility of the incorrect result. If the sig value is smaller, the result is usually more reasonable. GDP will be substituted with "city economic production" in the revised manuscript according to the suggestion.

We accept the reviewer's suggestion and will separate power generation and industrial energy consumption. Some discussion about this will also be added in the new section of "Inter-city comparison of China" in our revised the manuscript.

The discussions of emission trends for the twelve Chinese cities will be added into our revised manuscript. The possible reasons of the differences among the cities (e.g.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Wuxi, Shanghai, Guangzhou, and etc) will also be analyzed.

8. Language Even though the use of the English language is acceptable, the authors should read over the text carefully for mistakes, such as: Page 7987, Line 13: should be “: : : methodologies are thought : : :” Page 7988, Line 16: should be “: : : countries are compared : : :” Page 7993, Line 10: should be “: : : 5% from 2007 : : :” Page 7999, Line 14: “cities” not “cites”

Response: Thanks very much for this specific suggestion. The mistakes you mentioned here will be revised soon. We will also check the whole text carefully and the revised manuscript will also be polished by a native English speaker to improve the language quality of this paper.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 12, 7985, 2012.

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12, C1879–C1888, 2012

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[Full Screen / Esc](#)

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[Interactive Discussion](#)

[Discussion Paper](#)

