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

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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. 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 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. 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 CO2 emission and one that could be reduced by future energy transformations. It is not adequate in this context to say that CO2 emissions from biofuel use are not relevant because the CO2 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 CO2 uptake and CO2 release; and, second, the carbon balance will be disrupted as 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. It is not necessary or appropriate to refer to the emissions as carbon dioxide equivalents, CO2e (why subscript "e" anyway?). This is normally used to mean that gases other than CO2 are included (like CH4) and converted to CO2 equivalents using radiative forcing measures. I think this study only includes CO2, and therefore the use of the term "equivalents" is unnecessary. 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 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 CO2 emissions from the city into surrounding regions, where it is not completely accounted for. Is self-generation of electricity by industrial facilities included? 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? 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 (i.e., within China) consumption. It would 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-CO2 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. To help the analysis of industrial activities and emissions, I would separate power generation and industrial energy consumption. 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 per-

sonally 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 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. . . 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" My overall recommendation is that the authors take time to extend their analysis beyond the basic presentation of emission estimates. By ACP standards, the paper is short and rather too simple. 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. 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. But ACP needs greater depth and detail, I believe, than is exhibited by the paper as it stands.

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