

## ***Interactive comment on “Refined estimate of China’s CO<sub>2</sub> emissions in spatiotemporal distributions” by M.-M. Liu et al.***

**M.-M. Liu et al.**

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We thank reviewer #2 for the valuable comments and suggestions to improve this manuscript. We have carefully read through the comments. The point-by-point response will be presented here, and we would also like to explain how to revise this manuscript.

The paper investigates an important issue about China’s emission uncertainties by adding new contributions about emission sources and trends from spatial and temporal dimensions. The paper is well structured and well written. The results are clearly presented and I would recommend a publication as soon as possible.

Response: Thanks very much for this positive comment.

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There are a few minor issues I would like to draw attentions from the authors.

1. The Fig 3 is very messy. I understand the authors are trying to show the trends for 31 provinces in China, but the presentation can be better improved by selecting a few important provinces for illustration. Alternatively, the authors may like to use ‘shade’ to group the provinces in terms of geo-economic conditions (e.g. South West, East Coast etc).

Response: Very good suggestions. Though each province has its own characteristics in monthly variations of CO<sub>2</sub> emissions, most provinces are not much different with the national average. Therefore, it will be enough to select some important provinces such as Beijing, Shanghai, Heilongjiang for illustration in the text. We have revised it according to your suggestion (see Fig. 8b). The second way you suggest is also good. Actually, we have already grouped the provinces in terms of geo-economic condition in the original manuscript by using different colors (e.g. the black lines represent provinces in East China, the black lines represent provinces in North China etc).

2. In many other researches, Tibet (XZ) is excluded from the study due to the data limitation. In this paper, Tibet is analysed but without much discussion. Explanations about the data sources for the unusual regions can be useful.

Response: Tibet is really an unusual region for there are almost no any statistical data on energy consumption. Therefore, CO<sub>2</sub> emissions from industrial energy consumption (IEC) and other energy consumption (OEC) are not included in our calculation for Tibet. Besides, we only take the cement industry into consideration in sector of industrial process (INP). The clinker production of Tibet from 2005-2009 are taken from China Cement Yearbook (ACC, 2006-2010), while in 2000-2004 we directly use the cement production (NBSC, 2001-2010) to calculate the emissions due to lack of data on the clinker production. We got the data on vehicle populations by vehicle type of Tibet from the National Statistical Yearbooks (NBSC, 2001-2010) to calculate the transport emission consumption (TEC) emissions. However, only total vehicle populations but

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not vehicle populations by type are given in 2000-2001. So we have to estimate it by multiplying total population in these two years by the proportion of different vehicle type in 2002. More discussion about the data sources have been added in the revised manuscript (see sect. 2.1, paragraph 2).

3. Section 2.3, line 15-25, there are discussions about power plants emissions as LPS, how many plants did you included here, and equal to x% of total production capacity?

Response: We have discussed power plant emissions as LPS in the original manuscript because the position of LPS will greatly improve the spatial resolution of our inventory. In 2009, 489 power plants of 821 LPS have been included here (see Table S5), which almost equal to 80% of total production (see sect. 2.3, paragraph 1) and about 74% of total production capacity. In other years, the number of power plants is different, while the percent of total production capacity don't change a lot. In our study, we select the power plants ranking in the top 80% in terms of electricity production as LPS rather than in terms of electricity production capacity. Therefore, we don't give out the percent of total production capacity in our manuscript.

4. Under section 3.3 (Temporal distribution), the discussion for the December as the most energy consumption intensive month is a bit rough. I am not convinced about the reasons listed in the texts. Perhaps, it will be good to back up the argument by using the physical production outputs (e.g. steel, electricity production etc)? The argument about annual quotas at end of the year is casual. I would not use that as an explanation.

Response: Thanks for your suggestions. We have taken reviewer's suggestions to explain the peak in December by using the physical production outputs, like electricity generation, heating and cement production (see sect. 3.3 for more details). The viewpoint of annual quotas at the end of the year has been used as an explanation in previous studies (Gregg et al., 2008; Guan et al., 2012;) about China's CO<sub>2</sub> emissions. It may be a little casual, but really matters.

5. A couple of place talking about the importance of CDIAC as a data source, you

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may need emphasize once only. For example, page 17460, line 5, you can delete the content in the brackets.

Response: Thanks for this suggestion. We have checked through the manuscript and deleted the content in the brackets in page 17460, line 5.

6. Figure 8a, more explanation about why your data and CDIAC can match very well from 2001 –2008 April, but strong variation in later half of 2008?

Response: We have put more discussion about this issue. The monthly emissions in CDIAC are accounted based on the reported month energy consumption data from 2001 to 2007 (Andres et al., 2011), which will match the reality well in these years. However, in 2008 it was estimated via Monte Carlo methods based on the reported monthly energy data in other years as mentioned in the sect. 3.3 of original manuscript(Andres et al., 2011). Therefore, the monthly curve estimated by Monte Carlo methods in 2008 will be similar to the previous years. However, monthly variations of national emissions in 2008 were very different as compared with other years for some reasons (see page 17462, line 6-12 in our original manuscript). In our study, we used the monthly physical production outputs of 2008 to calculate the emissions, which will reflect the reality more accurately and also lead to the great differences comparing to CDIAC since the beginning of 2008.

7. There are several paragraphs about the implication of data variation to climate models / air pollution models. For example, in page 17459 lines 10-15; and section 3.4. I very much appreciate the discussion, but it may need another paper to tackle this issue in full. I would suggest the authors reconsider whether you would like to include the info into this manuscript.

Response: We agree with reviewer's comments. The issue of the implication of data variation to climate models / air pollution models is so complicated. It will be not easy to explain it clearly in a few sentences. Besides, the results here seem to be more of a bias offset according to the comments of reviewer 1. It's not enough to support our

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original conclusion. Moreover, these paragraphs are not the main content of this study. Therefore, we are going to exclude this part from this manuscript. We will tackle this issue in full in another paper.

#### Reference

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 13, 17451, 2013.