

Interactive comment on “Refined estimate of China’s CO₂ emissions in spatiotemporal distributions” by M.-M. Liu et al.

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We thank reviewer #1 for the valuable comments and suggestions to improve this manuscript. We have carefully read through the comments, and summarize them into the following 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 manuscript explores the Chinese provincial data sets to calculate the magnitude of fossil fuel and industrial carbon dioxide emissions and distribute those emissions on a 0.25 degree map. Comparisons are made with other time series and maps. Uncertainties are qualitatively discussed and for the magnitude calculation quantified.

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The scientific significance of this manuscript is that it provides another look at Chinese emission both in terms of input data sets as well as spatial and temporal distribution methodologies. The results are new, but not unexpected given the different methodologies involved. Overall, the significance is rated as good.

Response: We appreciate the reviewer’s comments. The different methodologies involved in our manuscript are the most important contributors to the new results. Moreover, we also take advantage of information not available to the previous studies.

2. The scientific quality of the manuscript is rated as good. There are some significant assumptions in the methodology used and the authors identify some of these. The authors are perhaps too critical of previous studies. For example, Figure 4a shows their new magnitude calculations with uncertainty estimates. Most of the results shown are statistically similar despite differences in methodology and included sources.

Response: Thanks for your comments. The presentation in the original manuscript may be not so good. We just want to clarify the differences from previous studies but not criticize them. In the revised manuscript, it has been noted that though there are some differences between our results and the previous studies resulting from the differences in methodology and included sources, most of the existing results are statistically similar. Please see sect. 3.1.

3. Another example is the comparison to CDIAC maps in which the differences between the maps are clearly explained by the differences in the new methodology and the 17-year old CDIAC methodology (section 3.2).

Response: You are right. One of the most important explanations for differences between the maps should be the differences in methodologies. We have highlighted it in the revised manuscript (please see the second paragraph in sect. 3.2). The three explanations in the original manuscript are the detailed description about the differences in the new methodology and CDIAC methodology.

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4. A final example is Figure 8a comparing the new monthly methodology which takes advantage of information not available to the CDIAC monthly methodology.

Response: We agree with reviewer's comments. It has been revised in the new manuscript (see sect. 3.3).

5. The presentation quality of the manuscript is good. Suggestions for improvement are included below. Detailed comments keyed to the manuscript:

5-1. page 17452, line 8. on monthly -> on a monthly

Response: Thanks for your suggestion. We have substituted "on monthly" with "on a monthly" in our revised manuscript.

5-2. page 17452, line 18. And the resulting ->The resulting

Response: We have revised according to your suggestion.

5-3. page 17453, line 11. Wang et al., 2012a is not in the reference list. An a and b need to be added to the two existing Wang et al. (2012) references to agree with the text.

Response: In the revised manuscript, an "a" and "b" has been added to the two existing Wang et al. (2012) references.

5-4. page 17453, line 23. impracticable -> impractical

Response: It has been revised according to your suggestion.

5-5. page 17453, line 24. the governments do not report monthly fuel uses by sector. ->the provincial governments do not report monthly fuel uses by sector. Andres et al. (2011b) did account for Chinese emissions by month at the national level.

Response: Thanks for your suggestion. I can't agree with you more. It has been modified in our revised manuscript.

5-6. page 17455, line 18. EFs undefined previously in text.

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Response: It has been defined in the revised manuscript.

5-7. page 17457, line 10, 9% confidence interval. This is an unusual confidence interval to calculate. Perhaps you mean 90% or 95%?

Response: It should be "90%". We have corrected it in the revised manuscript.

5-8. page 17457, line 17. The annual average growth rate (AAGR) for -> The AAGR for you have already defined AAGR in the previous paragraph.

Response: It has been revised now.

5-9. page 17458, line 10. highest annual average growth rate (AAGR) at -> highest AAGR at

Response: It has been revised now.

5-10. page 17458, line 12. Figure 4a is called to in text prior to Figure 3b.

Response: Thanks for your suggestion. We have revised it according to your suggestion. Please see sect. 3.1, paragraph 2 in the revised manuscript for more detailed information.

5-11. page 17459, line 10, The results show... While this sentence is not incorrect as it is written, it is somewhat misleading. See the Supplementary file, Figure S2 comment below about magnitude, trend, and bias offset. You may want to write a more balanced sentence here.

Response: Thanks for your suggestion. We have excluded this part in the revised manuscript. Please see the response to the comment of Figure S2 (5-18) for more details.

5-12. page 17459, line 28. Mongolia are becoming -> Mongolia is becoming

Response: It has been revised now.

5-13. page 17461, line 2. In CDIAC. As -> in CDIAC and our distributions (where

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population is used as the spatial proxy, see Figure 1). As

Response: Good suggestion. Yes, we also use the population as one of the spatial proxies, which will introduce some uncertainties. The sentence has been revised according to this suggestion.

5-14. page 17463, line 19. Wang et al., 2012 is not in the reference list. An a and b need to be added to the two existing Wang et al. (2012) references to agree with the text.

Response: In the revised manuscript, an a and b has been added to the two existing Wang et al. (2012) references. In page 17463, line19, it is Wang et al., 2012b. We have revised it.

5-15. page 17470, Figure 1. OEC and TEC undefined previously in text.

Response: We have now defined them in the section 2.1 of the revised manuscript.

5-16. page 17473, Figure 4a. It is good that you explicitly included your 90% confidence interval here. With the uncertainties for your new data set explicitly shown, one can more clearly see that the global approaches taken by IEA, US-EIA, CDIAC, and PBL and the national approaches taken by Guan and Zhao are in relatively good agreement with each other, both in trends and magnitude. This agreement occurs despite differences in methodologies and exact sources included in each data set. page 17473, Figure 4b. The thin and purple Zhao line is too close in appearance to the thicker and purple Guan line to easily discriminate between them. Please change one of them.

Response: Thanks for your suggestion. The thin and purple Zhao line has been changed to a thin and green line for Fig. 4a and 4b. It would be better now.

5-17. page 17478, Figure 9, (compared to 25 %). I do not understand the parenthetical element and it is not explained in the text.

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Response: In Figure 9, the y-axis represents the difference between the fractions of CO₂ emissions from electricity generation in summer of each 31 provinces and 25%. We have explained it in Figure 9 of our revised manuscript.

5-18. Supplementary file, Figure S2. While the data derived here seem to lie closer to the monitoring data curve than the CDIAC curve for much, but not all, of the time (as stated in the text), what surprises me more is that the data derived here more closely match the CDIAC curve, both in magnitude and monthly trend than either do the monitoring curve. This magnitude and temporal match were achieved despite the relatively more detailed statistics used to derive the data derived here than those used for CDIAC. That the concentration curve constructed from the data set derived here is greater in magnitude than the concentration curve constructed from the CDIAC data is not a surprise since the data derived here includes sources that CDIAC does not (see also your fig. 4a.). This explains the “closeness” of the two curves relative to the monitoring curve. When the monitoring curve is greater in magnitude than the other two curves, the data derived here are closer to it. When the monitoring curve is less in magnitude than the other two curves, the CDIAC curve is closer to it. The results here seem to be more of a bias offset than anything else. I suspect that if your confidence interval was propagated through this same calculation, you might not see any statistical difference between your data derived here and the CDIAC data.

Response: We appreciate reviewer's comments about the implication of data variation to climate models. We agree that the results in Fig. S2 are not enough to support our original conclusion. Therefore, we will take the suggestions of reviewer 2 to delete this part. In our future work, it will be further explored.

5-19. Supplementary file, Figure S3. Your discussion of this figure is mostly about year 2000 emissions in three areas. It would be most useful if those areas were explicitly labeled in Figure 3a and Figure 3b could be deleted as it is not called in the present text.

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Response: Good suggestion. Some important areas have been labeled in Figure S3a. Figure S3b have been deleted.

5-20. Supplementary file, Table S1. Coke Oyen Gas -> Coke Oven Gas

Response: It has been revised according to your suggestion. Much appreciated.

5-21. Supplementary file, Table S1. Many references given here, but they are not listed in a supplementary reference section.

Response: Those references given in Table S1 have been added in the supplementary reference section. Thanks for your suggestion.

5-22. Supplementary file, Table S4. Superscripts on three of the column titles are not explained elsewhere on the page.

Response: Thanks for your suggestion. The explanations of superscripts on three of the column titles have been given in Table S4.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/13/C6982/2013/acpd-13-C6982-2013-supplement.pdf>

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