

Interactive comment on “Multiannual changes of CO₂ emissions in China: indirect estimates derived from satellite measurements of tropospheric NO₂ columns” by E. V. Berezin et al.

Anonymous Referee #2

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It is of great significance to know well about the recent trend of anthropogenic CO₂ emissions in China, which has recently become the world leader in total CO₂ emissions. The study conducted by Berezin and coauthors provides an annual trend for the period from 1996 to 2008 in CO₂ emissions in China based on the simple inverse modeling (top-down approach) using satellite measurements of tropospheric NO₂ columns and also compare it with the corresponding bottom-up estimates (EDGAR v4.2, GCP and PKU-CO₂). As a result, the authors found the significant differences between the bottom-up and top-down estimates of the CO₂ emission trend for the period from 1996 to 2001, while the different kinds of estimates are consistent for the period from 2001 to 2008. Additionally, the potential uncertainties of author's results and their implica-

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tion are discussed. Within the reviewer's knowledge this article is the first study in which the recent annual trend of CO₂ emissions in China are estimated based on the indirect information from satellite measurements of NO₂ columns. Consequently, this reviewer believes that the paper is of the interest of ACP and recommends publishing this paper with minor revisions in response to the following questions, comments, and suggestions.

1. Page 265, Lines 28-29: The authors should explain the reason about why “such underestimation is likely due to a corresponding bias in NO_x emissions”.
2. Page 272, Line 19: The authors should add the explanation about why the uncertainty in spatial distribution causes the differences between the top-down and bottom-up estimates.
3. Page 273, Lines 9-28: The main part of discussion in this section has already reported by Zhang et al. (2007). I suggest that this section is omitted or shortened.
4. Page 277, Lines 3-27: In this section, the authors explain that the PHCP and MIC sectors to the total NO_x and CO₂ emissions from China are predominant. However, according to Fig. 8, the “residential and other sectors” for CO₂ and “other EDGAR sector” for CO₂ and NO_x are also larger sectors. Especially, the emission fraction and annual trend of “residential and other sectors” is different between CO₂ and NO_x emissions. The authors should explain the effects of emissions from these sectors.
5. Section 4.3: Fig. 9 shows that the spatial distribution of emission changes from EDGAR v4.2 inventory is almost constant. This result is quite unreasonable and may indicate that there are some reasons or errors in the spatial distribution of EDGAR inventory. The authors should add more discussion.
6. Line 26 of page 285 to line 11 of page 286: If the multiannual change of sector emission fraction in the EDGAR inventory is not appropriate, the conversion factor “F” may vary with year in the period from 1996 to 2008. It may cause the different temporal

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changes of CO₂ emissions between 2001 and 2003. I suggest the author add the discussion about the uncertainties of the “F” and its effects to the author’s results.

7. Fig. 5: The symbol (red circle) for “(5) NO₂ summer” should be changed to the symbol (red diamond) for data plot.

8. Fig. 10: The legend for province is too small to be clearly visible. It should be improved.

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