

Interactive comment on “CO₂ emissions inventory of Chinese cities” by Yuli Shan et al.

Anonymous Referee #1

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In this study, the authors proposed a method to compile emission inventories for Chinese cities. The authors applied the proposed method to 20 Chinese cities and compiled inventories. I fully agree with the authors that quantifying and understanding emissions from cities will be a key for better greenhouse gas emission management and maybe the inventory compilation method the authors proposed could be a helpful tool for China. However, as a reviewer of ACP, I do not recommend this manuscript for publication because of several reasons:

1. The relevance to ACP

First of all, I do not think this study falls into the scopes of ACP. Emission inventories can be used as an input for atmospheric transport model simulations. In that way, I do think an emission inventory development does fall into the scope of ACP. However, this study only touched the methodological aspect of emission inventory development (which is maybe fine if published as a tech report) and I did not see any direct scientific

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implications for atmospheric chemistry/physics. I have no doubt that this manuscript would be more appreciated if submitted to more relevant journal that discusses climate change, emission management/mitigation and etc, given the contents of the work (especially the suggested emission mitigation pathways presented in the conclusion). I believe the authors would be able to find much more appropriate arena to discuss this study. I personally highly doubt that this study (given the current form) would meet the interest of the audience of ACP.

Ref: http://www.atmospheric-chemistry-and-physics.net/about/aims_and_scope.html

2. The conclusions are not yet reached

The authors emphasized that the importance of developing consistent and concrete emission inventories for Chinese cities (which I fully agree with). However, the method proposed in the manuscript does not have any consistency that holds scientific significance. I do acknowledge that the authors calculated emissions for cities in a systematic way in the framework the authors proposed. But it does not mean the emission estimates can be fully compatible to emission estimates from other studies for instance. Given the different levels of deductions implemented for different cities (to compensate the lack of data), I highly doubt that emission estimates for different cities can be compatible. It is hard to believe that without examining the method proposed and evaluating the emission estimates calculated. The authors could compare their results to different studies, although the numbers of previous studies are not so large. At the section 2.2, the authors defined two challenges in developing Chinese city emission inventories: 1. The definition of cities (difficulty in dealing with lateral inflow/outflow) and 2. Data availability. First, I think these are common issues for any other cities, not just Chinese cities. Second, this study is not addressing those issues at all. The authors did not acknowledge very clearly, but issues of inflow/outflow still remain in this study and cannot mitigated by just changing the definition. The authors tried to address the data availability issue by deducing some information lacked. But without showing any validity of the method, the authors cannot claim that the issue is addressed. It is

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hard to believe that emissions are calculated accurately (and consistently) based on information deduced from data with limited availability. Some of the assumptions are really major (e.g. use province as a proxy for cities). The authors just touched on this as a limitation in the conclusion, but the assumptions need to be tested at least if it is valid or not, especially some of the conclusions are drawn using the results (e.g. emission mitigation pathways). The text is well written, but the authors need to support their claims by the results. In addition to the lack of evaluation and validation, I would like to point out the lack of traceability as well. I could not reach to Yearbooks from the citations. I don't think that is just because of the fact that I do not know Chinese language.

3. The scientific significance

Given the lack of the evaluation, validation and traceability, I do not think this study is ready for discussing its scientific significance. The suggested emission mitigation pathways (again, this does not provide direct science implications to atmospheric chemistry/physics) seem to be a significant contribution to emission management in China, but they were based on the analysis where provinces are used as proxy for cities when deducing information). Given that, I thought the same conclusions could be drawn just looking at province-level data (which are more data rich and consistent than the deduced city data/emissions) instead of trying to obtain emission inventories at challenging city-level using deduced data. Back to the section 2.2 where the authors defined two major challenges in compiling emission inventories for Chinese cities (which again I do think those are common issues for any other cities), the real challenge in compiling emission inventories in China is the potential biases that are suggested by the two of the authors of this manuscript. After Guan et al. (2010) and Liu et al. (2015), the science community is also well aware of the bias issue and I thought it is very strange that it was not discussed in this manuscript at all. It is easy to imagine that, given the uncertainty is so large at national and provincial level, it would be extremely challenging to achieve an accurate estimate at city level. If the method proposed in this manuscript is

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sensitive to the biases suggested by previous papers (I think it is), the method cannot be readily useful for emission management. It was also surprising to me that the word "uncertainty" was never used in this manuscript. Most of emission inventories do not come with uncertainty estimate. However, IPCC methodology does defined two methods (sectoral and reference) to check the validity of emission estimates. Although the use of the two methods would not be able to detect a type of biases suggested by Guan et al. (2010) and Liu et al. (2015), it would be able to check the validity of the emission calculation done as defined. The authors refer to the IPCC methodology many times in the manuscript, but I thought it was very confusing. The method proposed here is not consistent with the IPCC method (this study also used the updated emission factors suggested by Liu et al. 2015). The intention of the use of new emission factors was to obtain accurate emission estimates I believe. Which is totally fine. But in that case, the authors cannot keep consistency with other estimates based on the IPCC method (the authors could say they do not value the consistency). Also, I do not see much value to partly follow the IPCC method. The authors could come up with their own best method to obtain the best estimates. I do not see a consistency between the goals of this study and what was done.

Line by line comments

P1, L1: As this study only addressed limited aspects of city emission inventory development using so many assumptions, this needs to be more specific defined not to confuse the audience of this manuscript.

P1, L19: Even challenging at national level as reported by previous studies. So how would it be possible to get accurate estimates at more challenging scale? Maybe this study is one of the best efforts, but that fact does not support the accuracy of the emission estimates.

P1, L21: The authors did use the categories defined in the sectoral approach defined by the IPCC methodology. But it does not have significance if the proposed method

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yield biased results. Mitigation policy will be done country or local government not globally. It does not make much sense to claim that we need to have the same global standard for city scale issue. The IPCC method has been used to monitor international compliance of reduction of greenhouse gases.

P1, L23: How accurate those can be if you disaggregate those information?

P1, L27: I agree with this sentence. But I do not think this study has fully achieved it. This study only provides emissions and associated analyses obtained from consistently using their method. I also disagree with the authors claiming "concrete". What was the result that supports the concreteness? I do not see any logical reason that this method outperforms others.

P2, L51: The method is not shown as concrete. What is this study consistent with?

P3, L3: The uncertainty is relatively new discussion in this topic, but emission inventory has been important for many years.

P3, 59: I believe some inventories are at least partly based on state level information.

P4, L97: These challenges are common for all cities almost. The real challenges specifically for China is the potential biases recently reported. I do not think the use of territorial emission approach mitigates the inflow/outflow issue in emission development. It is a data problem and there are not enough data to overcome. I do not think the data availability cannot be mitigated by deducing information from limited data. The authors are compromising instead of mitigating and the validity needs to be shown.

P5, L121 Why did the authors think this calculation is superior?

P5, L140: It is great to have detailed information, but the significant amount of information were created based on assumption w/o any evaluation.

P5, L131: This was not clear to me. The import/export info is not fully available. If so, how did the author exclude the portion? And this issue is not just for electricity. There

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should be cars and airplanes going in and out for example.

P6, L148: Data source please. Enough information was not given to us to keep track of what the authors did in this study.

P6, L163: The disaggregation should also introduce errors/uncertainties. This study implicitly assumed the disaggregation introduces zero errors. An assessment should have been done.

P7, L180: Again, the data availability cannot be mitigated by deducing information from limited data. If the authors think it is possible, the authors needed to show why it is possible by showing results.

P7, L182: Emission inventories are developed using different deduction methods (see Table S4). I am not sure the emission estimates can be treated without uncertainty estimates. I would hesitate to call city emission estimates obtained using different deduction methods as consistent estimates.

P10, L257: I am very confused by section 3.6. If the IPCC methodology is emphasized, the authors should used the emission factors defined by the IPCC. Although the paper by Liu et al. (2015) was published, I believe emission factors reported in the paper were not published yet and thus IPCC (and the scientific community) cannot use them. The use of IPCC emission factors might yield biased estimates. However, it will give a compatibility with other emission estimates obtained using the IPCC method and traceability. If the authors would like to obtain accurate emission estimates, the authors would not have to do follow the IPCC method and came up with own procedure to get the best estimates. I was not clear what the authors would like to achieve through this study. I did not see consistency between the goals of this study and what was done.

P11, L282: What do you mean by "different developmental stages"? How are they different?

P11, L285: 2011 statistical yearbook. Data source please.

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P14, L386: The use of the word “consistent” here is very misleading. This method is not fully consistent with IPCC method. Emission inventories from Chinese cities are estimated using different methodology based on the data availability. I do not know if that was consistent. The author did not provide any uncertainty quantification and we do not even know how to consistently compare emissions from different cities (which are estimated from different method). I do not see any consistent way presented in this study.

P14, L387: Yes, but limited to people who understand Chinese. I was not able to get any one of city-level yearbook (so I acknowledge that this review was not fully completed by the fact too). In reality, people don't need to calculate the emission inventory individually if the government provides one. The community just needs an emission inventory with rigorous uncertainty estimates and traceability.

P14, L391: The use of the word “representative” is also misleading. These 20 cities were mainly chosen to present a possible case of scenarios that affect on the results of this method (see P11, L282). Beijing, and Shanghai were not included while the authors claimed the representative 20 cities.

P15, L396: (Assuming the emissions are all validated). This is a good insight for emission managements. But I am not sure if ACP is the best place to discuss.

P15, L400: Some efforts – for example?

P15, L411: How do you reform the industrial structure?

P15, L412: These are again from 20 cities selected for these cities, and do not the best representative of Chinese 30 cities (it should have megacities too). So I am not sure if it is fair to draw a general conclusion from the results from this study (again, there is no evaluation/uncertainty quantification/validation)

P15, L419: As mentioned above, these limitations are very critical to conclude this study. I would not recommend this manuscript for publication (even as a technical

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report) before addressing these.

L15, P421: How could the authors do that? It is not consistent. There is no supporting information for this statement.

L15, P422: I am not sure how to respond to this sentence.

L15, P423: We do not know how accurately emissions are calculated even in this study. Then how do we know an improvement in emission estimates?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-176, 2016.

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