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Interactive comment on "Development of a high-resolution emission inventory and its evaluation through air quality modeling for Jiangsu Province, China" *by* Yaduan Zhou et al.

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Title: Development of a high-resolution emission inventory and its evaluation and application through air quality modeling for Jiangsu Province, China

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We thank very much for the valuable comments and suggestions from reviewer 1, which help us improve our manuscript. The comments were carefully considered and revisions have been made in response to suggestions. Following is our point-by-point

responses to the comments and corresponding revisions.

Reviewer #1

1. General comments: This paper presents the development of a provincial (Jiangsu) emission inventory, which can reduce the uncertainty in previous national level emission inventory and improve capability of air quality forecasting in Jiangsu province. The manuscript is well organized and written. Comprehensive evaluations were conducted, including comparisons with other inventories, with satellite data and with measurements of air pollutants. Another highlight of this manuscript is the inclusion of modeling results and sensitivity analysis of PM2.5 and O3 formation in Nanjing, which provides some implications of emission control strategies. The presentation of this newly developed high resolution emission inventory is suitable for being published in ACP.

Response and revisions:

We appreciate the reviewer's positive remarks on our manuscript.

2. Line 51-52: change "Issued in 2013, for example, the National Air Pollution Prevention Action Plan required" to "For example, the National Air Pollution Prevention Action Plan issued in 2013 required".

Response and revisions:

We thank the reviewer's suggestion and the sentence has been corrected as suggested in lines 51-52 Page 3 in the revised manuscript.

3. line 232: The sentence "For other sources, the temporal distributions in Shanghai investigated by Li et al. (2011)." is not complete.

Response and revisions:

We thank the reviewer's reminder. The sentence has been revised as "For other sources, the temporal distributions for Shanghai investigated by Li et al. (2011) were adopted" in lines 241-242 Page 10 in the revised manuscript.

4. line 284-285: D3 also includes some areas of Anhui, Zhejiang and Shanghai, but the provincial inventory was developed for Jiangsu. How were emissions in D3 treated for provincial inventory case? Please clarify.

Response and revisions:

We thank the reviewer's reminder and admit that we did not clarify the emission inventory applied in the provincial modeling case. In the provincial case, the emissions for the regions outside Jiangsu in D3 were obtained from the relocated 4×4 km regional inventory developed by Fu et al. (2013). Corresponding revision was shown in lines 591-593 Page 22 in the revised manuscript.

5. line 436-437: This implication might be questionable since the difference might be mainly driven by different methods (including different data sources) used in developments of emissions.

Response and revisions:

We thank the reviewer's comment and agree that the implication could not be sufficiently supported. The sentence has been removed in the revised manuscript.

6. Sect. 3.3 shows the comparisons of pollutants from typical sources, how about pollutants from all sources? As shown in Figure 3, these typical sources account for significant but not entire amounts of the selected pollutants. The spatial distributions of total pollutants may substantially differ.

Response and revisions:

We thank the reviewer's comment. The main purpose of Section 3.3 is not to compare the spatial distribution of the total emissions between inventories, but to investigate the influence of different methods and data sources on the spatial distribution of pollutants in emission estimation. Therefore emissions from selected sources are included in the comparisons between inventories at national and provincial scales. For example, industrial combustion was generally treated as area sources in the national inventory,

while information of 6,194 point sources was collected and compiled in this work to develop the provincial inventory. The linear regression analysis between gridded emissions of industrial combustion in provincial and national inventories thus implies the deviations in spatial distributions of emissions resulting from different methods and data.

7. Page 19, line 514: NOx emissions in Jiangsu are only 65% higher than those of Shanghai. Given the large areas of Jiangsu, NOx emissions in south Jiangsu areas are comparable to those in Shanghai, and south Jiangsu is very close to Shanghai. It is hard to say local source in south Jiangsu is dominant. Is "those of Shanghai and Zhejiang Shanghai" a typo? Please correct it.

Response and revisions:

We thank the reviewer's reminder and admit a mistake in the original sentence. Jiangsu's NOX emissions were estimated 282% instead of 65% higher than Shanghai's in MEIC, and they were also 65% and 94% larger than those of another two contiguous provinces Zhejiang and Anhui, respectively. According to the YRD regional inventory by Fu et al. (2013), moreover, NOx emissions of Suzhou and Wuxi in southern Jiangsu were nearly twice of those for the nearby cities Jiaxing and Huzhou in northern Zhejiang. Therefore we believe the local emissions played an important role in air pollution level in southern Jiangsu. In lines 537-538 Page 20 in the revised manuscript, the word "dominated" has been deleted and the sentence has been modified accordingly.

8. Sect. 4.2: Provincial emission inventory was developed but the innermost domain was only configured with focus on south Jiangsu. However, the newly developed emission in north Jiangsu might have impacts on the air quality simulations in south Jiangsu (as shown in Figure S6, some sources are from north). Besides, the evaluations using surface measurements were only shown in Nanjing, how about in other cities, particularly in north Jiangsu?

Response and revisions:

We thank the reviewer's comment. Compared to northern regions, intensive economy and industry are located in the more developed southern Jiangsu, thus the air quality issue is more concerned in the region. In this work, the emissions of SO2, NOx, CO, TSP, PM10, PM2.5, BC, OC, CO2 and VOCs in the five cities in southern Jiangsu were estimated to contribute collectively 54.1%, 53.3%, 47.2%, 52.4%, 48.8%, 48.6%, 42.7%, 30.2%, 56.3% and 48.4% to the total emissions of the whole province in 2012, respectively. As shown in Figure S3, grids with relative large emissions (red color) were mainly clustered in southern Jiangsu. Therefore, we focused mainly on southern Jiangsu in the air quality simulation. As the emissions from northern Jiangsu were just applied in D2 simulation that provided the boundary conditions for D3 simulation, we believe the effects of those emissions were limited.

Regarding the model evaluation, the official data on air quality from surface measurements have been routinely published since 2013, thus the complete data for Jiangsu were unavailable for the years before 2013, except for its capital city Nanjing. Therefore only the observations at the nine sites in Nanjing were applied in this work.

9. Page 23, line 641-642: How were VOCs mapped to VOCs species? Were the same species profile applied for three emission inventories? Please clarify.

Response and revisions:

We thank the reviewer's comment. In this work, total NMVOC emissions for given source type in Table S1 were broken down into individual species using Eq. (1):

$E(i,k)=E(i)\times X(i,k)$ (1)

where E and X are the emissions and the chemical profile of VOCs (%), respectively; i and k represent the source type and individual VOCs species, respectively.

Different species profiles were applied in various inventories. The chemical profiles were mainly taken from domestic measurements, including residential fossil fuel and biomass burning, open biomass burning, on-road transportation, iron & steel, paint

production, solvent use and oil refineries. For sources without sufficient local measurements, results from foreign studies were applied including the SPECIATE database by USEPA (2014). To reduce the possible uncertainty of source profile from individual measurement, Li et al. (2014) developed the "composite profiles" for sources where multiple candidate profiles were available, by revising the oxygenated volatile organic compounds (OVOCs) fraction and averaging the fractions in different profiles for each species. In this work, "composite profiles" were updated following the method by Li et al. (2014), and the most recent source profiles from domestic results were contained.

To meet the requirement of CMAQ modeling, VOCs emissions were assigned to chemical mechanism (Carbon Bond 05) species by multiplying the emissions of individual species and mechanism-specific conversion factors:

$E(i,m)=E(i,k)\times C(k,m)/M(k)$ (2)

where E, M, and C are the emissions, mole weight, and the conversion factor, respectively; i, m, and k represent the source type, individual species, and the chemical mechanism species, respectively.

All the details could be found in another paper specifically for the development and evaluation of provincial VOC emission inventory (Zhao et al., in preparation), and we have stated that in lines 232-234 Page 9, and in lines 681-683 Page 25 in the revised manuscript.

10. Figure S5: the wind vectors and colors are difficult to read, please make clearer plots.

Response and revisions:

We thank the reviewer's suggestion and an updated Figure S5 with clearer plots has been provided in the revised supplement.

11. Page 26, line 716-721: Please clarify the starting time in Figure S6. Are surface winds plotted in figure S5? Wind directions vary at different heights (for example, blue

red and green lines show different directions), so it cannot imply the inconsistence with WRF results. The trajectories include vertical information, but the WRF winds are horizontal winds.

Response and revisions:

We thank the reviewer's reminder. HYSPLIT 24 h back-trajectories at 50, 250 and 500 m are calculated every 6 hours starting at 11pm on 9th October and ending at 5 am on 8th October (The caption of Figure S6 in the supplement has been revised). Figure S6 showed the air flow at the height of 50, 100 and 200m originated from Xuanwuhu site. Given the height of the first modeling layer in WRF was set at 50 m, the back trajectory at 50 m should be more suitable to indicate the air transport from 5 am on 8th to 11 pm on 9th October. As indicated in Figure S6 in the supplement, the air mass at 50 m altitude (the red line) came from northeast at 11pm on 9th October. However, the result was notably different with that simulated by WRF, in which the dominant wind direction was northwest (150°-170°) at that time.

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