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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of China's vehicle emissions on regional air quality in 2000 and 2020: a scenario analysis" *by* E. Saikawa et al.

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We thank the reviewer #1 for his/her helpful comments on our manuscript. Here, we respond to the comments in the order that were made.

Specific comments: It is very difficult to read the text on many of the figures, especially when printed but even when viewing on a computer screen. The text in Fig. 4 is virtually unreadable and perhaps could be more legible if laid out more horizontally rather than vertically. Please enlarge the font size for all figures.

We apologize for the small font size. We will enlarge the size to make sure that they are legible.

There is a heavy reliance on Borken et al. (2008) for the development of the emissions

projection, including growth rates for vehicle numbers, miles traveled, and emission factors. The authors should ensure that all the pertinent information from that paper (including the methods and numbers used) is restated here since readers may not have access to that paper.

We have taken year 2000 vehicle numbers, miles traveled, and year 2000 emission factors from Borken et al. (2008). In developing emission factors for the Euro 3 scenario, we have developed our own methodology based on actual regulation. Growth rates for vehicle numbers were taken from Wang et al. (2003). All the numbers are included in the tables, but we will make sure to clearly state in the table where the numbers come from.

Under 2.2.2 Emission Factors, the second sentence of the third paragraph ("Because there is no regulation adopted for motorcycles: : :") seems like it is misplaced, and perhaps should be moved to the second paragraph.

We will move the sentence to the end of the second paragraph.

Is there seasonality to vehicle emissions in China? Does this differ by gasoline vs. diesel? Even if seasonality is not accounted for in the emissions, the authors should comment on whether that is an accurate assumption or misses an important feature of realistic vehicle emissions. This information should come before the discussion of Figure 3 in section 2.3.

We do not account for seasonality in vehicle emissions in this study. The previous research in Tianjin, China, by Oliver (2008) has found that the summer CO emissions were 18% higher than those in spring. For NOx emissions, she found a 3% increase in the summer, and there was only a negligible difference for PM emissions. Considering the seasonal variation in CO, we agree that it would be more realistic to include seasonality in our emissions. However, there is a lack of data and Zhang et al. (2009) also notes that there is less seasonal cycle in vehicle emissions compared to other sources in China. This is why we argue that, with a lack of data, including seasonality in emissions is unfortunately not possible, and we believe even without including this information we do not miss an important feature of realistic vehicle emissions due to the small magnitude of the seasonal difference. We will include this explanation in section 2.3.

Section 2.3, paragraph 4, the sentence beginning “For example, while vehicle emissions share: : :” is unclear and should be edited (e.g. “For example, while the contribution of vehicles emissions is 46%...”).

We will change this sentence to “For example, while the contribution of vehicles for total NOx emissions in BAU is 46%, it is reduced to only 18% in the Euro 3 scenario.”

Section 4.2.2: Would it be possible to evaluate each PM2.5 component separately to corroborate the explanations given for underestimation of total PM2.5? In addition, please clarify the explanation given in the 5th paragraph of this section. Does SO4 really only contribute 2-3% of total PM2.5 at both Rishira and Oki? If so, does the small contribution of SO4 to total PM2.5 reflect the lack of aqueous phase SO4 production in WRF/Chem, as noted in the preceding sentence? Is this really “of less importance” as stated by the authors or actually a major source of underestimation? Finally, can any model performance conclusions realistically be made by comparing simulated concentrations to only two observation sites, one of which (Rishiri) is at the very edge of the model domain?

Thank you very much for your great suggestion. We evaluate the primary PM2.5 component separately. As shown in Table S1#1, SO4 only contributes less than 5% of total PM2.5 at both Rishiri and Oki in April and October. The contributions in January at Rishiri and July at Oki are higher than 10%, and it is clear that excluding dust emissions is still one of the reasons for underestimation of the total PM2.5 concentration in spring. However, considering the low contribution of SO4 in our results compared to Matumoto et al. (2003), which finds SO4 to be the dominant contributor of PM at Rishiri, we agree with the reviewer that the lack of aqueous phase SO4 production in

WRF/Chem could also be one of the main causes for underestimation. On the final point about model performance, we agree that we need more data on PM2.5 to reach a much more comprehensive conclusion regarding model performance. However, with the lack of data that we face, comparison with two stations was the best we could at this point. As more data become available, there is a need for further investigation.

Would "frequency distribution" be a more accurate term than "probability distribution" since no uncertainties in the simulated concentrations are being accounted for and thus the graphs are essentially counts of gridcells with concentrations of a certain value? It would be useful to note whether the gridcells where concentrations are highest are also the most populated or whether they are occurring outside of the most populated areas (though it may be argued that population exposure is outside the scope of this paper).

Yes, we will change the word to “frequency distribution” in our revised manuscript. Comparing the CIESEN population data for 2000 with our results, it is visible that populated areas are where the O3 mixing ratios and PM2.5 concentrations are high. We will include the sentence in the revised manuscript that populated areas are where the concentrations are highest, and that this is important for health analysis.

Section 5.3, paragraph 3, the sentence beginning “Maximum O3 reductions from BAU to Euro 3: : :” is unclear and should be clarified.

We rephrase this sentence to “Maximum reductions in O3 mixing ratios within China from BAU to Euro 3 are 11, 15, 21, and 22ppbv in January, April, July, and October, respectively.”

Section 6, paragraph 4, the sentence beginning “We find that as the result: : :” should be “We find that as a result: : :”

Thank you for this. We will change it.

The authors cite Shindell et al. (2011) in the last paragraph of the paper in reference to additional impacts of vehicle emissions on health, agriculture, and climate. Although a full assessment of these impacts is outside the scope of this paper (as the authors state) the authors should consider whether comparisons of results (e.g. emissions impacts) of the present analysis and Shindell et al. (2011) are possible.

In this paper, we focused on the current Euro 3 emission standards in China, but if we did the similar study for Euro 6 as Shindell et al. (2011) does in their study, comparisons of results would be possible after conducting analysis on health, agriculture and climate.

Reference

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13141, 2011.

Table S1#1. Total $PM_{2.5}$ and species contributing to primary $PM_{2.5}$ at Rishiri and Oki as simulated by the year 2000 simulation in WRF/Chem (top: μg m^{-3}, bottom: share of total $PM_{2.5}$).

	$PM_{2.5}$	SO_4	NO_3	NH_4	BC	OC
Rishiri						
January	0.78	0.08	0.33	0.13	0.05	0.05
		(11%)	(42%)	(16%)	(7%)	(7%)
April	1.53	0.08	0.37	0.14	0.09	0.25
		(5%)	(24%)	(9%)	(6%)	(16%)
July	3.39	0.31	0.46	0.25	0.12	0.14
		(9%)	(14%)	(7%)	(4%)	(4%)
October	2.22	0.08	0.12	0.07	0.13	1.10
		(4%)	(5%)	(3%)	(6%)	(49%)
Oki						
January	4.60	0.10	1.81	0.57	0.21	0.38
		(2%)	(39%)	(12%)	(5%)	(8%)
April	7.19	0.26	2.11	0.71	0.28	0.64
		(4%)	(29%)	(10%)	(4%)	(9%)
July	6.63	0.88	0.94	0.60	0.21	0.38
		(13%)	(14%)	(9%)	(3%)	(6%)
October	3.89	0.19	0.07	0.09	0.16	0.30
		(5%)	(2%)	(2%)	(4%)	(8%)

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Fig. 1. Table S1#1