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Geoscientific Model Development
Discussions

Interactive comment on "Black carbon modelling in urban areas: investigating the influence of resuspension and non-exhaust emissions in streets using the Street-in-Grid (SinG) model" *by* Lya Lugon et al.

Anonymous Referee #1

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The underestimation of BC concentration in air quality models has been a comment issue and this work provides some perspectives that would be important to solve this problem. Those founding are important and could be of the interest of readers and scientific communities in this field. However, the languages and overall structure of the manuscript should be improved before publications. My comments are as follows:

Major comments:

1. The introduction is too long and requires serious streamlining. I would suggest

the author summarize those modeling literature and emission numbers using one or two tables. And only discuss the technical difference between those works. Also, to make the overall structure more clear, the authors could consider rearranging the literature part based on the following order: observations, modeling methods, model performance, gaps need to be addressed in current models.

2. The description of different simulations conducted in this study is confusing. After careful and difficult reading, I guess there are two different sets of simulations. One set is 6 simulations with Polairs3D-SinG-MUNICH model, another one is 1 simulation with Polairs3D-MUNICH model. If that is the case, the author should make it more clear in Section 2. Also, the author should improve the simulation scenario description by adding key massages in Table 1, such as the model/source used for wear emission calculation.

3. The author should explain why the simulation results present in Section 5 are not evaluate with observations. Without the model performance compared, it is impossible to evaluate the necessity or advantage to adopt the two-way coupling method compares to the one-way coupling method.

4. The author takes the entire section 2 to describe a new "deposition-resuspension" framework but concludes that the whole "deposition-resuspension" process is insignificant in improving BC underestimation (line 405-410). Instead, the selection of emission factor is the key to solve the problem. This made the work of the new "deposition-resuspension" framework meanless. The author could add one more simulation with the setting of simulation 4 while turn off the new "mass-conserved deposition-resuspension" framework, to properly evaluate the potential of this framework. If not, I would suggest the author move entire section 2 to the appendix and only briefly introduce the new framework in the "Simulation setup" section.

5. In general, the languages used in this manuscript could be improved. Try to avoid long sentences with multiple subordinate clauses that really made the text difficult to

follow.

Minor comments:

1. Line 7-9, the statement after "i.e." is confusing, please rephrase. Do you mean the BC from one street can be transported to another street at regional scales?

2. Line 22-23: Please rephrase. For example, Here comparisons are performed using ... factors from literature, and we found ?? are improved?

3. Line 58: Can you provide the LDV and HDV definition (citation?), why the MDV (mid-duty vehicles) is not considered in this study?

4. Line 75-80: Feels like most of the non-exhaust important studies are PM10 focused. Can you discuss the size distribution difference between exhaust and non-exhaust emissions?

5. Line 84-85: Is it the same ratio for both PM2.5 and PM10.

6. Line 87: How is this related to non-exhaust emission? What is the source of this species?

7. Line 91: "Some models" Please provide citations. And also, I don't think even if is appropriate here, most would be a better wording.

8. Line 92: "HERMES" Please provide a citation of this model here.

9. Line 103: Why is it supposed to be independent? Are describing the NORTRIP assumption or making general claims? Please clear, if it is the latter, you would need a citation to justify your claims.

10. Line 317: Why selecting those two days? Are they more representative of the general average?

11. Line 324: "PM10 fraction" fraction of what? PM10 fraction of TSP?

12. Line 330: What is LCFs stand for by each letter?

13. Section 4.1: Not clear if those 6 simulations are conducted based on Polairs3D-MUNICH-SinG model combination or on a simple SinG local/box model.

14. Line 350: How dose those fractions applied for different size bins?

15. Table 1: Can you also add the model SOURCE in the table? Like EMEP, HERMES, and NORTRIP?

16: Line 395-398: I don't understand here. If the non-exhaust wear emissions are very low, there should be a very small amount of mass available for resuspension. So, no matter how high the resuspension rate is, there should have no mass supply for resuspension, as demonstrated by simulation 5. So, how could a high resuspension rate along explained the much higher BC concentration in previous simulations? Isn't it just contradict your simulation 5?

17. Line 442: Very confusing statement here, do you mean SinG and MUNICH are two different systems that can replace each other? But based on section 2, MUNICH is part of SinG. Or, maybe you want to say, the comparison is between Polair3D-SinG-MUNICH and Polair3D-MUNICH (without SinG)? Same question for line 465.

18. Line 444-448: What is the point to mention the one-way feedback here? Is it used in this study? Do you mean the Polair3D-SinG-MUNICH is using two-way feedback, and Polair3D-MUNICH is using one-way feedback? And you are comparing between those two methods? 19: Line 469-470: Can you explain why this SinG results to higher BC concentrations compares to MUNICH for those streets?

20: Line 477-478: How do this double-counting works? Please provide more detailed discussion here. Why does this not seeing on high emission street?

21. Section 5: Is the SinG simulation in Section 5 the same as simulation 4 in section 4? Is the MUNICH simulation in section 5 have the same model setup for all other SinG simulations? Why simulation result from the MUNICH simulation not compared with street-level observation (Boulevard Alsace Lorraine) as in Section 4? There is no

way to judge which method has better model performance MUNICH or SinG.

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