

Interactive comment on “Experimental and model assessment of PM_{2.5} and BC emissions and concentrations in a Brazilian city – the Curitiba case study” by Lars Gidhagen et al.

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Please find below the comments made by Reviewer 2. Authors' responses are given after each comment. The references to pages and lines are for the revised manuscript which includes the tracking changes (submitted as Supplement).

Reviewer 2: General comments “The article presents important results regarding campaigns of measures in the scope of the ParCur project. The observational part was

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made based on measures close to the surface, in fixed and “mobile” stations (bicycles), and at the level of 70 m above the ground. This is the richest and most important part of the article. Also, three different models were applied to try to represent the concentrations of Black Carbon, Particulate Material 2.5 and Oxides of Nitrogen. Despite three different approaches to modeling, little useful information has been obtained, making several approaches, both in terms of emissions and in terms of the results obtained, and is therefore the weak part of the article. The general recommendation is to keep only the observational part of the work, with the modeling part being excluded. Here are some specific points that led to this conclusion.”

Authors' response: The authors acknowledge the constructive comments made by Reviewer 2. As for the general recommendation to keep only the observational part of the work, we feel that this comes as a consequence of our first manuscript version failing to clearly describe the methodology applied. We hope that our revised manuscript, together with our responses to the reviewer's specific comments here below, will convince the reviewer 2 of the meaningfulness of the procedures where our conclusions are based on a combination of measurement and model results.

Specific comments 1. “Page 2, line 19: correct the units “ $\mu\text{g m}^{-3}$ ”

Authors' response: Corrected.

2. “Page 2, line 29: Is the year of the reference correct? In the reference list it appears as 2014”

Authors' response: It should be 2014, corrected page 2, line 29 and line 30 (note also that the URL has been updated).

3. “Page 3, lines 17 – 20. The authors said that “As far as we know, this is a pioneering study in South America with the integration of fixed and mobile high spatio-temporal resolution PM_{2.5} and BC measurements, the development of emission inventories and the implementation of modeling tools at different spatial scales for validating the emis-

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sions and to determine the spatial distribution of pollutant concentrations across the city.” Maybe you are referring to studies about Black Carbon over Curitiba, but, in fact, there are at least a dozen of studies combining high resolution measurements with well established modeling tools in São Paulo and Rio de Janeiro (see the works by Professors Paulo Artaxo, Maria Andrade, Luiz Pimentel, and others). There are at least 30 years of air pollution studies in Brazil and a “long road” of knowledge was built. This need to be valued. Your emission inventory does not consider many important features of the Brazilian fleet (content of ethanol in the gasoline, vehicular fleet aging, secondary roads, among others). You are applying different models that do not interact with each other. Your modeling results are far away from the observed concentrations. So, it is exaggerating to consider it as a pioneer study.”

Authors’ response: We agree that this formulation is provocative and we have replaced the word “pioneering” with “innovative”. We still claim that the way existing activity data, measurements and models have been used together is new and has, to our knowledge, not been published for Brazilian cities before. However, we do not say that monitoring and modeling in general is new in Brazil, we do reference a lot of Brazilian studies and the reviewer 2 is correct in her/his list of important contributions, especially from São Paulo and Rio de Janeiro. What we claim:

a) High spatio-temporal resolution PM_{2.5} and BC refer to the biking experiment, whose application in Brazil has been developed by co-authors of this manuscript.

b) Air quality modeling in Brazil: According to an overview given by Andrade et al. (Air quality in the megacity of São Paulo: Evolution over the last 30 years and future perspectives) there is a history of 20 years of air quality modeling, first with offline models and later with mesoscale inline/online models of the type used in this study (BRAMS-CCATT) and WRF-Chem. However, we have not found high spatial resolution model studies of the type used in the present study, neither have we seen street canyon modeling used to assess the traffic impact on the micro-scale.

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c) We agree that our emission inventory is incomplete, covering traffic and industry for PM but only traffic for BC. However, traffic emissions are locally determined by the street canyon modeling and monitoring (making unnecessary knowing the aging of the fleet and specific fuel composition). Larger secondary roads are covered by the VISUM traffic model (see added details on this input data on page 6, lines 5-13) for private transport and the detailed information of the public transport emissions is on a unique level covering all streets where public transport is operating.

d) The high spatial resolution modeling shows PM_{2.5} contributions of local sources that fit the measured urban background in the city center, while the measurements in the residential area clearly point out to an unidentified local source of PM_{2.5}. The main purpose with the modeling in this assessment is to explain the role of the local and regional contributions from sources that are included in the inventory. Any differences between model output and measured total levels are discussed in terms of potential other sources and deficiencies in the different inventories. We claim that this information is more easily and less costly obtained by using dispersion models together with measurements.

4. “Page 5, lines 18-19. The authors said that “Emission factors for bi-articulated buses were extrapolated using information on fuel consumption provided by the Curitiba municipality”. Please, clarify how was it possible to separate fuel consumption from one type of bus to another? Are bi-articulated buses using an exclusive type of diesel that allows that estimation? Page 5, lines 20-21. The authors said that “The use of biofuel lowers the PM and BC emissions by 50%, according to the U.S. Department of Energy (2018)”. What is the relevance of this information for this study? Was this reduction considered in your emissions? This is not clear.”

Authors’ response: Local authorities (URBS) reported type of fuel and fuel consumption for nine types of buses, bi-articulated being one of them using either diesel (S10) or biodiesel (B100). Discussions with emission specialists for the Stockholm bus fleet showed that they were of the opinion that the 50% reduction of PM and BC emissions

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while using 100% biofuel, as indicated by the US reference, was reasonable. Yes, the emission factors were reduced in this way for the buses using biofuel. We have changed the text to make this more clear (page 6, lines 10-11).

5. “Page 5, 30 – 34. What was the procedure to adjust EEA emission factors to the Brazilian reality? There is a recent work from Ibarra-Espinosa et al (2018) that could be consider in your work, since it applies very detailed procedure on building an emission inventory adjusted to the Brazilian conditions. (Ibarra-Espinosa, S., Ynoue, R., O’Sullivan, S., Pebesma, E., Andrade, M. D. F., and Osses, M.: VEIN v0.2.2: an R package for bottom–up vehicular emissions inventories, *Geosci. Model Dev.*, 11, 2209–2229, <https://doi.org/10.5194/gmd-11-2209-2018>, 2018.)”

Authors’ response: We thank the reviewer for giving this interesting and recent reference. The bottom-up approach and the emission modeling in VEIN is very similar to the mobile emission inventory (part of software package Airviro) used in our study. In our study, activity data for private vehicles come from a traffic model, as suggested in VEIN. For public transport we have simulated each bus line using its time table, which should give a more precise information on vehicles per hour passing a certain road link. VEIN suggests the use of COPERT emission factors, using vehicle speed. There is in Europe a debate of using vehicle speed or signed speed limit together with the traffic situation (type of flow, if free, congested, saturated with stop-and-go etc) as a parameter to modify the emission factors, an approach developed in the ARTEMIS project and implemented in the HBEFA emission factors. For buses we used emission factors that considered traffic situations (we assumed saturated conditions), however the EEA emission factors used for private fleet did neither use speed nor traffic situation as a parameter. The main difference in our approach in Curitiba, as compared to VEIN, is that we, after simple assumptions on vehicle technology and speed, confirmed the emission factors through the street canyon modeling and monitoring experiment.

6. Page 7, line 5. Actually, 10 x 10 km² or 50 x 50 km² refers to grid spacing, not resolution. The minimum resolution for these grids would be equivalent to 20 and

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100 km² (2 x delta x,y). The lowest grid spacing probably will represent the urban area of Curitiba by one or two grid points. In that situation, emissions will be poorly represented. Please comment on that matter.

Authors’ response: The resolution of a meteorological model describing weather events is various time larger than the grid resolution. We have changed the wording describing the resolution of the regional model (page 8, line 13-14), from “spatial resolution” to “grid resolution”. However, the regional model was not used to map the air quality over Curitiba, only to provide the long-range transport. The local model had a grid resolution of 200x200 m, while emissions grids were generated with a higher resolution (100x100 m). The Gaussian model gives a continuous decay away from each source, although evaluated only at the 200x200 m grid points. Thus it is fair to say that the horizontal resolution over the city is at least equal and most likely better than the 200x200 m of the evaluation grid.

7. “Page 7, line 24. Please, comment on the constraints regarding the use of neutral stability in your simulations with OSPM.”

Authors’ response: The diagnostic wind model used to generate the wind at roof level takes stability into account. However within the street canyon neutral conditions are assumed, this being part of the OSPM assumptions. To our knowledge this is not an important constraint since turbulence in a street canyon is mainly mechanically-generated. During strong cooling (working towards stable conditions) the heat collected inside the street canyon, together with the turbulence generated by the traffic and the street canyon vortex itself should counteract any stratification tendencies within the street canyon. There has been for street canyons located at very high latitudes (northern Norway) a case indicating a smaller deviation from measurements that could be related to the neutral assumption instead of stable conditions, but this has been shown not to constitute a problem while using OSPM in more mid-latitude cities like Copenhagen. As for convective conditions, it is likely to expect in tropical cities some effects, but there is to our knowledge nothing published around this effect on OSPM results.

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It is difficult to think that the convective effects should invalidate OSPM simulations of wintertime Curitiba street canyon concentrations.

8. "Page 9, lines 3-9. The explanation for high BC concentrations during the weekends of 6-7 and 13-14 August is not clear. If the wind speed and emissions are low, how to explain the high levels of BC if you are ruling out this contributions?"

Authors' response: These high levels occur during weekend nights when traffic emissions are low. Even if the peaks in the city center (MD) and in the residential area (SC) come at the same nights, they are not following each other exactly in time. We interpret this as an effect of one or various unidentified source(s), inside or just outside the city (if it was far away the responses at the two stations should have followed a more similar time variation).

9. "Pages 9 and 10, section 3.3 and 3.4. These sections are very poorly explored. The authors give an impression of a very simple process to simulate. Results of the simulations are not in good agreement with the observed concentrations and the authors use linear correction rates to adjust the concentrations, instead of exploring the errors on their emissions. The processes involved are not linear. There are reactions involved that will be dependent on concentrations and environmental conditions. See for example the procedure described between lines 17 and 21."

Authors' response: The regional model output concentrations, as given in Section 3.3, have not been corrected, only discussed in terms of uncertainties in the emission inventory outside the city. Based on the result section 3.3, discussing Table 5, it is premature to conclude on an underestimation of the long-range BC contribution. We have edited the text in Section 3.3 to only indicate large uncertainties due to the use of the coarse EDGAR-HTAP emission data (page 11, lin 14-16). The conclusion that those emissions are likely to be underestimated comes in connection to the comparison between measured and simulated urban background BC levels, i.e. in Section 4.7, page 16, lines 13-15. As for Section 3.4, which describes the comparison of measured

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and simulated contributions of local traffic inside a street canyon, we claim that NO_x, PM_{2.5} and BC contributions from the local traffic can be assumed inert as long as they stay inside the street canyon. Consequently the assumption of a linear relationship between emissions and concentrations in the dispersion model output can be justified.

10. "Pages 13 and 14. As pointed out by the authors, many features were not included in order to represent correctly the emissions of BC, PM_{2.5} and NO_x. There are many arguments, but little basis for a correct guidance on procedures to be adopted for better public policies aimed at improving air quality. The observational part is rich, but the application of numerical models does not add important or useful information. Thus, the greatest recommendation regarding the article is that the observational part be used, but that the modeling part be withdrawn, since it gives the impression that the problem of air quality is of simple treatment, making adjustments here or there, neglecting important physical/chemical processes and replacing them with mere statistical procedures."

Authors' response: Following our earlier answers, we claim that the integrated analysis with both modeling and monitoring exercises give important information on sources and spatial distributions of PM and BC concentrations. In the text we admit the major shortcomings of the models, the emission inventories and the monitored data; also lifting forward some unique features like the very detailed emission inventory for public transport. Our intention has been to show the contributions from long-range (sources outside the city) and local sources inside the city. We have been able to explain a reasonable local contribution to BC, which together with an underestimated and spatially homogeneous long-range contribution fit to measured BC levels in the urban background. For PM_{2.5} the same comparison points out unidentified sources contributing to locally raised PM_{2.5}, not possible to explain with a spatially uniform long-range contribution. Our experience is that for fairly inert urban pollutants like NO_x, BC and - for smaller cities - PM, the large uncertainty is found in the emission inventories, not in dispersion model formulations.

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About the References 11. “The references “World Medical Association, 2014” and “Zhang et al., 2015” are missing in the reference list.”

Authors’ response: In our version of the submitted manuscript these two final references are found on page 20. We will assure that they are visible in the revised manuscript to be uploaded.

12. “The reference “Targino, A. C., Gibson, M. D., Krecl, P., Costa Rodrigues, M. V., dos Santos, M. M., & de Paula Corrêa, M. (2016). Hotspots of black carbon and PM2.5 in an urban area and relationships to traffic characteristics. *Environmental Pollution*, 218, 475-486. <https://doi.org/10.1016/j.envpol.2016.07.027>”, was not cited in the text.
“

Authors’ response: Correct, the quoting of this reference had by mistake been eliminated. We have introduced it again, first on page 3, line 2; secondly on page 7, line 29.

13. “Change the order of the references Wallace et al, 2011, and VISSIM, 2018.”

Authors’ response: The reference “VISSIM, 2018” has been renamed and moved to “PTV GROUP, 2019”.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2018-1094/acp-2018-1094-AC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1094>, 2018.