

***Interactive comment on* “Local and regional contributions to fine particulate matter in the 18 cities of Sichuan Basin, southwestern China” by Xue Qiao et al.**

Xue Qiao et al.

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Received and published: 8 April 2019

Response to RC # 1

Dear reviewer,

Thank you for the comments to help improve the quality of the paper. We have revised the manuscript to address your comments and a detailed response to each comment is provided in this file. The comments are in regular font and the responses are in blue.

The manuscript by Qiao et al. is a follow up to their previous paper on model evaluation. Here the model is applied for source apportionment of PM_{2.5} in the Sichuan Basin.

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Discussion paper



The tools and analysis seem reasonable. The manuscript is legible and the figures are clear. In fact they do a very good job of compressing a lot of source-oriented modeling results into some very interesting tables and figures. They consider the roles of different regions, and also the roles of different species, in impacting local and nonlocal PM_{2.5} concentrations. I appreciated as well that they considered different spatial responses (regional vs city-scale) and temporal responses (also considering just the max daily contributions). I have a few questions that authors might consider to make the article a little more clear or interesting in places, and some editorial corrections, which constitute only minor revisions. Overall, I would say this paper is quite near ready for publication in ACP.

Response: Thanks for the positive comments.

Comments: The abstract reads very well.

71: The description of what type of results are produced from lagrangian back trajectory models is rather vague and not very accurate. These models can be quantitative, but not for chemically active species. They will mostly just reflect the atmospheric dynamics and are not a great method for source apportionment of secondary species. This could be explained more clearly. Response: Thanks for the clarification. We added modified in the third paragraph of the introduction to: “and the Hybrid Single Particle Lagrangian Integrated Trajectory Model (HYSPLIT) can just reflect the atmospheric dynamics so they are not quantitative for source apportionment of secondary species”.

Section 2.1: Could the authors briefly describe how the source-oriented model addresses the formation of secondary species when precursors come from different regions? For example, formation of ammonium nitrate when the nitric acid comes for region 1 but the ammonia from region 2? Is it assigned based on the chemically limiting reagent, or is the source attribution based on total mass (i.e. ammonium nitrate would be ascribed to regions 1 and 2 according to the mass percent of nitrate vs ammonium)?

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Response: The reactions are expanded so the gases with different regions are allowed for reactions between each other. The contributions are based on the mass of the components directly, not by limiting reagents or total mass. We have added the information to section 2.1 as: “For example, NO₂_S1 and NH₃_S2 can be used to represent NO₂ from region 1 and NH₃ from region 2, respectively. After the photochemical mechanism is expanded, the source-tagged species are allowed to go through all processed to form (NH₄_S2)(NO₃_S1) based on additional reactions of NO₂ + OH → HNO₃ and NH₃+ HNO₃ →NH₄NO₃. Thus, the contributions of region 1 to NO₃– and region 2 to NH₄+ are quantified.”

132: It would probably be worth clarifying here that although SOA source contributions are not included, that SOA itself is included in the model. Response: Thanks for the suggestion. We have added a sentence to describe this at the end of section 2.1: “SOA is included in the current model but its source contributions are not resolved.”

Section 2.1: Is anthropogenic fugitive dust included (e.g. Philip et al., ERL, 2018, <https://doi.org/10.1088/1748-9326/aa65a4>). Response: The anthropogenic fugitive dust is included in the EDGAR inventory. Only windblown dust is considered separately.

General: Another interesting metric related to source contributions is the Response to Extra-Regional Emission Reduction (RERER) metric, which ranges from 0 to 1 and can readily be evaluated in a table, fig, etc. See for example: <http://publications.jrc.ec.europa.eu/repository/bitstream/JRC102552/lbna28255enn.pdf>
Response: Thanks for the suggestion. RERER is calculated by using the Equation 1. In our manuscript, the data of non-local contributions shown in Tables 1 and 2 provide information the same as the RERER, except that non-local contributions in this paper use the unit of % and do not include secondary organic aerosols (SOA), as the source contributions to SOA are not tracked in this study (Equation 2).

$$\text{RERER} = \frac{R(\text{Total PM}_{2.5}) - R(\text{PM}_{2.5} \text{ due to local region})}{R(\text{total PM}_{2.5})} \quad (1) \text{ Non-local}$$

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contribution=(Total PM2.5-PM2.5 due to local region-SOA)/(Total PM2.5)×100% (2)

Thus, no changes was made.

Section 3.3: This was nice to see, but I felt the motivation for including this was a bit absent from the paper. Are the authors interested in MDC because of the acute impacts on human health? Or because of a policy reason such as the exceedence of an air quality standard? Maybe a bit more could be added in the introduction motivating this section. Response: In section 3.2, the percentage contributions from different regions to particulate matter are presented but the absolute concentrations due to each region are not shown. In order to better understand the greatest extent of each region's impact on PM2.5 concentration in other regions, MDCs are presented in section 3.3. In the revised manuscript, we have clarified the motivation of this analysis in the last paragraph of the introduction section: "In this study, the percentage contributions and maximum mass contributions from each region to PM2.5 in each city are both presented to better understand the extent of air pollutant transport."

333 and general: Here and elsewhere the authors refer to "transport of SO4" however for secondary species like this they have not really determined if the transport is occurring in the form of the particulate species (SO4) or the gas-phase precursor (SO2). In the winter in particular, the lifetime of the latter can be several days, so precursor transport is a factor. Thus, I would suggest the authors review their language throughout the paper and are careful to describe their results in terms of transport of aerosol or aerosol precursor species, rather than just the former. Response: Thanks. We have modified this throughout the manuscript.

Editorial:

50: of such areas → such area Response: Done

51: home for → home to Response: Done

57: times of → times Response: Done

68: receptor-based models. Air → air Response: Done

213: is in → is located Response: Done

228: In summary, the → The Response: Done

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1214>, 2019.

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comment

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