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comment**

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.

Anonymous Referee #2

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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 PM2.5 in the Sichuan Basin. 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 PM2.5 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.

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.

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)?

132: It would probably be worth clarifying here that although SOA source contributions are not included, that SOA itself is included in the model.

Section 2.1: Is anthropogenic fugitive dust included (e.g. Philip et al., ERL, 2018, https://doi.org/10.1088/1748-9326/aa65a4).

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.pdfâĂÍ 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

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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.

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.

Editorial:

50: of such areas –> such area

51: home for –> home to

57: times of –> times

68: receptor-based models. Air –> air

213: is in –> is located

228: In summary, the –> The