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Interactive comment on "Assessing the nonlinear response of fine particles to precursor emissions: development and application of an Extended Response Surface Modeling technique (ERSM v1.0)" *by* B. Zhao et al.

Anonymous Referee #2

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1 General comments

This is an interesting paper which develops an advanced modelling technique for particulate matter concentration responses to changes in emissions of primary particles and precursor gases. The authors apply the model to analyse the sensitivity of PM concentrations in Chinese cities to the emissions from different economic sectors and spatial origin. Given the recurring episodes of high pollution in Chinese cities, this is a very important and timely topic and fits well to this journal. Considering the still rather

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high number of CTM simulations needed I am not fully convinced that this is an easily applicable technique for source response modelling in general, but the example provided in the paper demonstrates feasibility of the approach for a limited set of source regions (4) and sectors.

The article is generally well written, although the methodology section is in parts rather difficult to follow. I recommend the paper to be published after addressing the comments below, mostly regarding clarifications in the methodology.

2 Detailed comments

Sect. 2.1, general question: What is the time scale for the ERSM development? Do the authors aim to develop annual mean coefficients, monthly coefficients, summer/winter coefficients? Would full-year coefficients be feasible?

p. 5055 l. 7ff: "The ERSM technique first quantifies the relationship..." - What is missing in the general description of the methodology here is a simple description of how this functional relationship is quantified. This should be explained briefly to readers who are not proficient with the RSM technique and the MPerK program.

p. 5055 l. 12ff: How about formation of secondary PM in the source region followed by transport to the target region? Is this process missed or is just the formulation unclear at this point? If it is missed, do the authors have an estimate of its contribution? How about interactions of precursor emissions from different source regions?

p. 5058 l. 4ff: Similar to above: Is something missed due to this assumption? In fact, as it is formulated it reads rather straightforward and I don't quite understand why

GMDD
7, C1997–C2001, 2014

this assumption is needed. How should PM2.5 in A be influenced by precursor gas concentration changes in A other than through local chemical formation?

p. 5058, l. 10ff, Eqns 8 and 9: This paragraph is a bit unclear to me. If [PM2.5_Trans]$_{B\rightarrow A}$ is calculated through Eq. 8, how is Eq. 9 used? Or is Eq. 8 merely the definition and Eq. 9 is in fact used for calculating the transport contribution from B to A? This should be clarified.

p 5059, l. 4ff and Eq. 11: It would be helpful if $[\mathrm{NOx}]_{A,min}$ were defined explicitly. Is it certain that $[\mathrm{NOx}]_{A\rightarrow A,min} = \mathrm{RSM}^{\mathrm{NOx}}_{A\rightarrow A}(0,0,0)$? Is the case (0,0,0) i.e. all precursor emissions equal 0 covered in any case? From a random draw it could even be higher?

p. 5060 l. 15 ff: δ as defined in l. 16 is not an interval. I assume the actual transition *interval* is $([\mathrm{NOx}]_{A,min}, [\mathrm{NOx}]_{A,min} + \delta_{NOx})$ for NOx and equivalent for NH_3? If so, this should be clarified in the text.

I do not completely understand why the physical transport versus chemical production diagnostics module is used in the second approach outside the minimal precursor concentrations, but not within the first approach. For example, could it not be useful for distinguishing chemical and transport contributions as in Eq. 7 and 8? This should be better motivated by the authors.

Given that two different approaches are used here, the authors should comment on how smooth the transition is between the two regimes considered, i.e. how large the deviations are in the overlap interval.

GMDD

7, C1997–C2001, 2014

p. 5062 l. 15: “the cases where all control variables are controlled stringently” – If I have not overlooked something, these cases are mentioned here for the first time. They should be mentioned in the text describing the scenario generation in the last paragraph of Sect. 2.2.

p. 5063 l. 13 and 17: changes of total emissions: in which regions? Does this refer to all regions reducing at the same time? This should also be mentioned in the caption to Fig. 3.

Figures 3-5: If the authors would like to show the comparison for all three regions, then they should discuss differences and characteristics – otherwise one example would be sufficient.

Figures 6-7: Is there an easy explanation for the sometimes significant differences between effects of reducing individual pollutants, and reducing all of them together? In most cases the combined effect seems lower than the sum of individual effects, which might be explained by overlap effects of reductions in both species involved in the formation of ammonium sulfate and ammonium nitrate, but also the opposite is the case? It would be interesting if the authors could comment on this.

3 Technical corrections

p. 5056 l. 29: “the same as NH_3”: I assume what is meant is “equivalent for NH_3”.

p. 5058 l. 21: “changes”: change

GMDD

7, C1997–C2001, 2014

p. 5059, l. 9: “the same as NH_3”: I assume what is meant is “equivalent for NH_3”.

p. 5060 l. 17: “interpolates”: interpolate

Figures 3-5: Axes font sizes are very small and hardly readable, I suggest to increase them. Also, I wonder whether there is a reason for the upside-down colorbars, otherwise I’d suggest inverting them.

Interactive comment on Geosci. Model Dev. Discuss., 7, 5049, 2014.