

Interactive comment on “Nonlinear response of ozone to precursor emission changes in China: a modeling study using response surface methodology” by J. Xing et al.

Anonymous Referee #1

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This is an interesting study where a response surface model (RSM) was created from simulations of the CMAQ air quality model for one month over Eastern China. The RSM was then tested for its accuracy relative to the original CMAQ model, and it was then used to explore the sensitivity of ozone to changes in precursor emissions (NO_x and VOCs). The use of a RSM for this purpose is likely to be of interest to readers, as is the improved understanding of ozone in highly industrialized regions of China. Therefore, I am hopeful that this paper can be published.

However, I find that the paper is far too unclear in its presentation to be accepted in its current state. The writing is complicated and seems to be missing important elements

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that would explain the methods and findings, and explain more of why the authors are doing what they are doing. Many technical terms are undefined. See particular instances of this below.

I also find that there is little or no validation of the model against observations. The authors refer to previous papers where these comparisons are made, and that is acceptable. However, the statements made in the paper about the sensitivity of ozone to changes in emissions are made as factual statements, as if the model is absolutely correct, with no discussion of uncertainties in the sensitivities inferred in the model. Knowing ozone sensitivity is unfortunately a very difficult exercise – it is necessary to demonstrate that the model reproduces ozone well, but also reproduces ozone precursors well. Typically, emissions inventories themselves are highly uncertain, and that leads to strong uncertainties in the ozone sensitivity. At least, the authors should discuss these uncertainties and not give the impression that ozone sensitivity is known with precision. At most, the authors should use their model to quantify uncertainties and explore how robust their predictions are. If there is other evidence showing that the sensitivities in this model agree or disagree with other models, or based on observational studies, those should be discussed in this context.

I also do not understand whether the model has been run over a big domain (Fig 2) or three small domains (Fig 8).

The figures are extremely small, where often the legends or axis labels or color scales cannot be read without being magnified, and the figure captions do not adequately explain the figures. I suggest removing unnecessary labels on the figures (for example the words on top of Figure 2b), making the individual panels larger, and making sure all text on the figure is large enough to read. Similarly, the figure captions need to be much more descriptive as these multi-panel figures are extremely complex.

Finally, the English writing is not bad, but is difficult enough to get through that it hinders communication. Since one of the authors is based in the US, I hope that he/she can

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help improve the writing.

Because of these problems, I do not find this paper acceptable in its current form. The work appears to be technically sound. I say “appears to be” because the presentation hinders my ability to understand and evaluate its technical merit. Nonetheless, I am hopeful that the paper can be accepted following a thorough rewriting.

More particular comments:

p. 29810, l. 18-19 “Effectiveness of NO_x emission control is growing along with stricter control efforts” – I don’t know how to interpret this, nor how it relates to the rest of the sentence. Does it mean that NO_x emissions are going down due to emission controls? Or are those reductions now more effective at reducing ozone? Or something else?
p. 29815, l. 6-25. I do not understand the “area of influence analysis”. How were the “ratios of inner-influence among the three regions” determined and what do they mean? In what way does fig. 2b show “interactions”? What was the model simulation on which this was based? p. 29817, l. 10-25. “emission ratios” are not defined – I think this is the factor multiplier for emissions describing the range over which emissions are varied. Then the “weight coefficients” are not defined nor does it describe where the values come from. I think all that is happening here is: $(tNO_x) (RtNO_x) = \text{SUM} (NO_{xi})(RNO_{xi})$, and so the weight coefficients are just the NO_x emissions, but this presentation doesn’t make that clear. p. 29820 – Methods like the LOOCV should be described more completely, possibly through a simple illustration. p. 29822, l. 22-27. Here it is not clear how the authors determine that one indicator is “more robust” than another – what is being compared with what to reach that conclusion? p. 29829, l. 17. There is no discussion of costs of emission reductions in the paper, so I don’t think the authors can conclude about “cost-effectiveness”. Should only judge “effectiveness”. Table 4 – Why are these combinations of reductions selected? Fig. 3b – Why is “sum 4 variables” shown here and what is its relevance for this study? Is this the sum of 4 numbers, each randomly selected from the same distribution (0-1)? If so, then wouldn’t values greater than 1 be possible? Fig. 5a – what method is used for this plot? Fig.

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6b – Is that correct that all symbols mean n=160? Fig. 7 – I do not know what the labels of the figures mean, such as “8vs2 – scale6” Fig. 9 – Consider plotting height on the vertical axis. Fig. 10 – This is a very complicated figure that isn’t explained. It seems that reductions of NO_x have little effect. Sorting days high->low is fine but should be explained. Based on which RSM are these results? Are b and c figures for averages of multiple days? Fig. 11 – Would it be more straightforward to plot d(O₃ concentration) against Emissions, rather than $d(O_3)/(1-dEmis)$? I find it difficult to understand what this plot means. Plots b and c then present the effects of VOC and NO_x “with synchronic control” and “with control of NO_x from power plants”, but I don’t see where synchronic control is defined and I don’t know how to interpret “with power plants” since power plants are the yellow and orange bars.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29809, 2010.

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