

Interactive comment on “Quantification of the enhanced effectiveness of NO_x control from simultaneous reductions of VOC and NH₃ for reducing air pollution in Beijing-Tianjin-Hebei region, China” by Jia Xing et al.

Anonymous Referee #2

Received and published: 5 February 2018

This study explores the effectiveness of simultaneous NO_x, VOC and NH₃ emission control on PM_{2.5} and O₃ using CMAQ and a set of polynomial functions. The methods they propose are innovative and computationally efficient, but the presentation of the results and the significance of the findings still need further improvements. Overall, I think there are a number of issues that should be addressed in order to make this paper suitable for publication.

General Comments:

C1

1. A common problem with statistical polynomial regression is overfitting. The fitting performance will certainly improve with higher orders, but it does not necessarily mean the models represent the true relationships. The authors show a very good fitting performance with inflated R values (0.93 to 1.0, Table 3), but this may actually reflect the models are overfitted. In order for the fitting to be trustworthy, the authors need to prove that the models are not overfitted. You could do so by conducting cross-validation for your model selection by partitioning your data to training and test groups. The test groups should not be used to fit the models, but to evaluate the model performance only.

2. The polynomial functions assume the changes in pollutant concentration only depend on the changes of local emissions, but transport, meteorology, and deposition can also change the concentration. The authors need to provide justification why these factors are not considered.

3. The authors did a good job synthesizing their results concisely, but I would recommend the authors provide more insights into the numbers they reported. The interpretation of the results could be improved by:

1) Considering large body literature behind this topic and comparing your results with previous studies, especially those observation-based studies.

2) Providing more mechanistic reasoning on the results you provided. For example, The authors show the impacts of emission reduction vary in space and time (Section 3.2 and 3.3), but they do not provide any insights for the such variations. There is also no discussion on how the effectiveness of emission control vary with meteorology.

Specific Comments

Page 1 Line 10: It looks like you're talking about the O₃ trend since 2010 (after the emission control), but Li et al. analyzed the trend between 2006 and 2011. I suggest cite a more recent paper that reflects the O₃ trend since 2010, otherwise it's mislead-

C2

ing.

Page 4 Line 30: Why do you choose 4th degree over 3rd degree? The difference is small, and you didn't give any statistical justification.

Page 5 Line 30: Why "the training samples need to be as small as possible"? With small number of samples, the coefficients are very likely to be unstable, especially since you're fitting high-order polynomial functions here.

Page 8 Line 9: It's not clear to me how you set up the two emission control scenarios. Why do the magnitudes of emission reduction differ among species? How would the agreement between CMAQ and pf-RSM change if the emissions are reduced uniformly?

Page 9 Line 5: The word "observe" is misleading. There are no observations in this study.

Page 9 Line 15: Please be more specific how your study is "consistent with findings of previous studies". It's also worthy mentioning how your study differs from previous studies in terms of methodology, results etc.

Page 9 Line 20: I'd suggest the authors compare your model-based findings with observations (e.g. in situ or satellite observations) that use indicator approach to identify the limiting species for the O₃ production.

Page 9 Line 23: The results you show here are just for January and July, but how about other months, especially spring (or fall) when O₃ production transitions from VOC-limited (or NO_x-limited) to NO_x-limited (VOC-limited)? Would you expect the effectiveness of emission control show any seasonality?

Figure 9: How does meteorology affect the day-to-day variability of O₃ and the effectiveness of emission controls?

Table 4: Why are there missing values for HebeiN?

C3

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-2>, 2018.

C4