Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-183-RC2, 2019

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Development and application of observable response indicators for design of an effective ozone and fine particle pollution control strategy in China" *by* J. Xing et al.

Anonymous Referee #3

Received and published: 29 August 2019

The manuscript by Xing et al. on development and application of observable response indicators uses response surface modeling to identify parameters that define key O3 and PM2.5 production regimes, and then correlates these with observable indicators, i.e. ratios of gas and aerosol phase concentrations that are routinely measured. This provides valuable information that could be used to help design effective air quality policy to simultaneously reduce levels of both O3 and PM2.5 which, as the authors point out, has been a challenge in China. The work is thus very relevant and suitable in scope for ACP. The paper is also very clearly written for the most part. My main comments are summarized as follows. The study currently neglect any errors in the polynomial approximations of the full CTM at later stages in the analysis, which I think is an oversight. Further, it is not mentioned explicitly that responses being analyzed here are with respect to domain wide emissions perturbations (I suspect, as it isn't explained clearly). This limits the applicability of these responses for evaluation of regional air quality control strategies, as there would be errors in using these relationships to estimate a response to a regional change in emissions. Lastly, here are a few definitions / concepts that would be useful for the authors to define upfront (definitions of indicators that general audiences may not be familiar with). Overall, the methods and results are interesting and have merit; all of these issues could be addressed with revisions to the text and some additional work on error analysis. Detailed explanation of these comments and other smaller issue are below.

Comments:

69: Please define DSN, GR, and AdjGR. Eventually I see later (line 233) that these are defined in the SI, but it would be more useful if they were defined earlier, or at least reference to where their definition can be found provided earlier.

71: Clarify that by "these" you are referring to indicators for O3. I don't believe this has been done for the SIA indicators such as AdjGR since total nitrate isn't routinely observable from space.

85: Please define PR and FR.

101: What is meant be severe here? Are the goals to address severe episodes in the winter or address longer-term annual averages? As the chemical mechanisms driving the former are not well know, yet, my guess is the focus of this article is on the latter, which should be clarified.

143 - 146: The cited works here are not published yet, so please provide a brief summary of the performance benchmarks and statistics.

155 - 263: I have questions about the spatial dimension of the terms in these equations. The manuscripts says that Xi was fit for every grid cell. Does that mean that in each grid cell it was known from the CTM simulations how Conc responded to each of the precursor emission species perturbed specifically in that grid cell? Or is it how Conc response to emissions perturbed uniformly throughout the entire model domain? If the former, that seems like a prohibitively large number of model runs (number of grid cells x 40). In this case then the response is the national average response?

If the latter, it seems like the applicability of these equations for policy application is hindered by transport, in that it is now known if the change in concentration is occurring owing to changes in emissions in that location or emissions several hundred km upwind. In essence, a map of the response is not equivalent to a map of where the emissions changes need to be to elicit that response, hence this precludes using this information for region-specific changes to precursor emissions. Unless there are policies that aim to uniformly reduce emissions (from all sectors) the same amount throughout the country, it is hard to envision the direct applicability of these relationships for policy. Thus I'm not sure of the value of the province-specific values like those shown in Fig 11 – a PR in a particular province isn't necessarily associated with changes to emissions in that province alone.

Fig 3: Please include units. Also define the domain over which the emissions perturbations are being considered here.

245: I understand why 0 is a lower limit, but why is 2 an upper limit? This seems to cut off a lot of points in April (Fig 5).

General: If a metric like FR and AdjGR don't agree, the authors are placing the blame entirely on the observable indicator e.g. AdjGR. However, there is some degree of inaccuracy in FR, related to the extent to which the pf-RSM explains the concentration responses. The authors should thus begin the results section with a summary of the accuracy of Eq 1, particularly in terms of discussing the residuals of this functional fit and their magnitudes, showing plots of the change in concentration predicted by FR or PF vs the actual change in concentrations.

Next, the magnitude of these residuals should be taken account when considering figures like 4 and 5. I suspect that the distinction of the 4 quadrants in each panel of Figs 4 and 5 directly along the axis is too strict. Rather, corresponding to the magnitude of the residual error in (1), the comparison for Figs 4 and 5 should be to identify points that lie some distance away from the quadrant boundaries, as points near the boundaries could be impacted by the error FR or PF.

Further, it's not clear in the writeup if the change in concentration in Eq 1 is that from the RSM or the CTM – this should be clarified. If the former, then there's an additional source of error that needs to be stated and accounted for, which is the RSM itself.

Lastly, these sources of error should be kept in mind in the presentation of all of the results comparing observable indicator responses vs RMS responses, e.g., discussion of Figs 7, 9, 10, 12. . .

201: As defined as the ratio of VOCs to NOx, it seems rather circuitous to derive this equation only to show that it reduces to the ratio of the coefficients for the linear VOC and NOx terms (i.e. x5/x6).

228: It is interesting that this change reduces down to just the linear response coefficient of PM2.5 with respect to NH3. This makes me want to see an additional plot in Fig 3 which is NH3 vs NOx.

275: Why is there a seasonal dependence to the performance of HCHO/NO2, particularly with such low performance in April?

308: How did the authors arrive at the form of this equation? Were other forms considered? Could something with a more generic expression (e.g. TAˆalpha x TNˆbeta x TSˆgamma have been used and the exponents determined using fitting procedure to design, objectively, the most comprehensive observable indicator?

Fig 6: It's not clear to my why things in this plot are binned in the x direction. Why not just plot the linear fit on top of Fig 4 for each? We would then also be able to see the seasonality of the fit.

Fig 7: Why is the spatial extent of oPR greater than PR? Is this because the slope of the fit between these two on a log scale is much greater than one (Fig 6)? Same question about Fig 9.

384: Could the authors comment on the practicality of this application? I'm having a hard time imagining simultaneous equal %-based reductions to China-wide NH3 and NOx emissions resulting from any real policy, given that these would be coming largely from different sectors, in different locations.

420: What are the control pathways considered here? Ah âĂŤ ok they are mentioned in the figure caption but it would be useful to add to the text.

Fig 14: It's not clear to me how these results show that simultaneous reductions of O3 and PM2.5 are possible in January – as stated in the text. Rather, it looks like they are not except for all but one scenario (NOx:VOC = 1:1, only at the far end of the pathway). Potentially a very interesting figure here but it needs more explanation.

Corrections:

37: subscript on NOx

51: Seinfeld et al. 2017 not in bibliography. Did the authors mean Seinfeld and Pandis (2012)?

407 - 410: There is perhaps a word missing or something from this sentence, please check.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-183, 2019.