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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Significant wintertime $PM_{2.5}$ mitigation in the Yangtze River Delta, China from 2016 to 2019: observational constraints on anthropogenic emission controls" *by* Liqiang Wang et al.

Anonymous Referee #1

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Want et al. evaluated the effect of long-term and emergent emission control strategies on the PM2.5 levels in Yangtze River Delta of China, by combining modeling analysis with observations. They found the decline in PM2.5 concentration during 2016-2019 was mainly due to emission control. The decline would be even greater if the meteorology was not unfavorable. Great potential of further decrease is manifested in analysis of data during G20 period when short-term emergent measures were taken. The discussion is valuable for assessment of past policies and design of future ones. However, to be publishable in ACP, the current manuscript requires further improvement.

Particularly, inadequate credits are given to the existing literature that performed similar analysis of separating meteorology and emission effects on recent PM2.5 trend in China. Instead, the authors tried to impress the reader by suggesting that this study is the first to do so. Just to list a few studies in literature (and I believe there might be more), Zhang et al., 2019. Drivers of improved PM2.5 air quality in China from 2013 to 2017, PNAS Zhai et al., 2019. Fine particulate matter (PM2.5) trends in China, 2013–2018: separating contributions from anthropogenic emissions and meteorology, ACP Zhong et al., 2018. Distinguishing Emission-Associated Ambient Air PM2.5 Concentrations and Meteorological Factor-Induced Fluctuations, EST The authors should review the existing literature and put emphasize on their innovations.

I also have concerns about the methodology. Assimilation is used for calculating the total effect (emis+met) which gives a good representation of PM2.5 distributions, despite any model errors. But assimilation cannot be used for calculating met-only effect. Therefore, model errors may propagate into the met effect. I wonder what uncertainties this inconsistency in two pairs of simulations would cause for the results. The authors evaluated model emissions and concluded the impact is small. But it is not shown if other model errors may be significant. For example, studies have found that model tends to underestimate sulfate production during high RH in China. More evaluation of the model performance may be useful for interpreting the result.

The inclusion of short-term G20 period is interesting. But I am not completely convinced that the mitigation potential map is useful at all. At a first glance, the map does not seem to be very different from conducting a zero-ish YRD emission simulation with the model and then do a subtraction. The problem is that the authors did not provide information about (1) what types or fractions of emissions were shut down during the event; (2) is the emission shutdown implemented in Hangzhou, or Zhejiang, or YRD? Without this information, it is not possible to interpret the mitigation potential.

In addition, the writing needs to be improved throughout the text. Some word choices aren't proper, and in the comments below I picked out some. Some descriptions of the methods need to be clarified as well.

Specific comments Line 41: Not clear from the text whether "> 14 μg/m3, 19%" is PM2.5 levels, or in fact, reduction in PM2.5 concentrations. Please clarify.

Line 42-44: Confusing, as it interrupts the flow and misleads a reader that the decline in Hangzhou (35 ug/m3) is due to G20 control measures. I suggest moving the sentence to either Line 40 after "YRD, China" or to Line 48 before "Compared to the long-term policies..."

Line 46: remove "thus"

Line 99: should -> can

Line 105: "unprecedented" is a too-big word here that I suggest to remove. Same for other occurrences of the word in the paper.

Line 125: Be consistent with the citation format.

Line 137-139: Are meteorological observations assimilated in addition to chemical observations. If so, describe the meteorological observations that are assimilated. If not, I don't think it is sufficient to just use initial and boundary conditions from reanalysis data. The WRF should be run in a nudging mode, so the meteorology is close to reality.

Line 155: "Prior anthropogenic emissions"? Do you optimize emissions at all? I cannot find such description throughout the text. If not, it should not be called "prior".

Line 166: "more" is not a proper conjunction word in formal English writing.

Line 176: grids -> grid cells

Line 176: "potential excellent roles" Rephrase it.

Section 2.4: What is the assimilation window? Daily? hourly? Line 212: "the threshold pinpointing the key value of the 213 correlation coefficients (e−1)". -> e-folding length

Line 217-219: well, it is still static in the relative sense. I don't this means anything. I'd suggest removing this statement.

Line 231: "unify the chemical inputs for the WRF-CMAQ model". What does this mean?

Line 237-240: Up to this point, we still do not know what method the authors use to separate effects of meteorology and emission. A clear description of the method is needed before this point.

Line 257: It is cursory to conclude these three periods have similar meteorology based on Fig. S1. The validity of the analysis is relied on the assumption that they are similar. E.g., one factor that is not analyzed is wind direction. Showing maps of circulation pattern will also help.

Fig. 4. Is Fig. 4 useful? It is no surprising that the assimilated simulation could better reproduce observations, which are used in assimilation. It means nothing.

Line 307-311. There is a jump in the logic of this sentence. I'd remove it.

Line 350-352: Many studies have properly separated the effects from meteorology and emissions, though with different approaches. I don't think the statement is fair.

Line 353: It's unclear to me whether 5% and 3% are relative to mean PM2.5 concentration or mean reduction of PM2.5. Be more explicit.

Line 355: how do you prove it was "under the same meteorological condition".

Line 370: what is "concurrent meteorology"? better to rephrase it.

Line 375-376: Of course the long-term strategies is emission oriented. You cannot change weather easily after all. . . I guess the author wanted to say the long-term decrease in PM2.5 was driven mainly by decreased emissions.

Line 387: what is "stably spatiotemporal state". Rephrase it.

Line 412: remove "unprecedented".

Line 414: what is “stable supply-side structures”? Not directly related to air quality to me.

Line 418: There have been quite a few papers discussing the effect of emission control strategy. To list a few, Zhang et al., 2019. Drivers of improved PM2.5 air quality in China from 2013 to 2017, PNAS Zhai et al., 2019. Fine particulate matter (PM2.5) trends in China, 2013–2018: separating contributions from anthropogenic emissions and meteorology, ACP Zhong et al., 2018. Distinguishing Emission-Associated Ambient Air PM2.5 Concentrations and Meteorological Factor-Induced Fluctuations, EST Although the method may not be the same, the authors should give credit to these studies rather than claim this is the first study trying to separate effects of met and emission.

Line 444: "rudimentary" may not be a proper word here.

Line 449: the statement that “the biogenic emissions are unimportant for IAV of PM2.5” may be true for YRD, but may not be “generally” true for elsewhere in the world. I’d suggest being more specific.

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