Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., author comment AC1
https://doi.org/10.5194/acp-2022-502-AC1, 2022
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# Reply on RC1

Yu Lin et al.

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Author comment on "Decoupling impacts of weather conditions on interannual variations in concentrations of criteria air pollutants in South China – constraining analysis uncertainties by using multiple analysis tools" by Yu Lin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-502-AC1, 2022

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We greatly appreciate Dr. Shi for providing the constructive comments, which have helped us improve the paper quality. We have addressed all of the comments carefully, as detailed below.

*This paper used two machine learning techniques to remove the meteorological effects on air quality trend in South China. The paper contains new and publishable results. There are new developments in machine learning, which should be considered (see below). I suggest that the paper may be published after a major revision.*

**Response:** In the revised manuscript we have included more parameters in machine learning modelling and also revised the abstract and re-organized Results and discussion accordingly.

*Line 19: define "in annual scale"*

**Response:** We meant "on an annual scale". One whole year data cover from May to the next April, e.g., the first-year covered from May 2014 to April 2015, and the last year covered from May 2020 to April 2021. We revised this part to: "To constrain the uncertainties in the calculated deweathered or decomposed hourly values, a self-developed method was applied to calculate the range of the deweathered percentage changes (DePCs) of air pollutant concentrations on an annual scale (each year covers May to the next April)."

*Line 21: define "consistency". Explain what does consistency of 70% or 30% means*

**Response:** In the revision, the part reads as "Consistent trends between the RF-deweathered and BRTs-deweathered concentrations and the ICEEMDAN-decomposed residuals of an air pollutant in a city were obtained in approximately 70% of a total of 42 cases (for seven pollutants in six cities), but consistent PCs calculated from the three methods, defined as standard deviation being smaller than 10% of the corresponding mean absolute value, were obtained in only approximately 30% of all the cases."

*Line 27: expand this section on results*

*Abstract focused more on methods but not results. Is this a methodological development* *paper or usual academic paper? What is the take-home message? Abstract should be re-written.*

**Response:** This study has several goals: (1) the first one is to assess the performance of several existing machine leaning methods used in decoupling weather impacts on concentrations of air pollutants; (2) the second one is to proposal the best approach that can be used for generating trends with minimum uncertainties; and (3) the third one is to extract trends for pollutants monitored in south China and validate the efficacy of emission reduction policies. To balance these goals, we have added some trend analysis results in the abstract, which reads: "The calculated PCs from the deweathered concentrations and decomposed residuals were thus combined with the corresponding range of DePCs calculated from the self-developed method to gain the robust range of DePCs where applicable."

*Methods: For secondary pollutants, it is important to include back trajectory clusters. Other met factors such as solar radiation might also be important. Please read relevant literature and include these important parameters.*

**Response:** We agree with the reviewer on this point, and we have included more meteorological factors such as boundary layer height, total cloud cover, surface net solar radiation, surface pressure, and total precipitation, which are extracted from the European Centre for Medium Weather Forecasting's Reanalysis-5 (ERA5) hourly data (https://cds.climate.copernicus.eu/) , and air mass clusters based on the Hybrid Single-Particle Lagrangian Integrated Trajectory (HYSPLIT) 72-hour back trajectories at an hourly resolution (https://www.ready.noaa.gov/HYSPLIT_traj.php), and re-run the machine learning modeling accordingly. The newly generated results substantially increased the consistence between the observed and predicted $PM_{2.5}$ concentrations with a slight increase in the consistency for $O_3$, but no evident increase in consistency for other pollutants. The manuscript has been revised on basis of the newly generated results.

*Line 135: R2 is not as good as other recent studies, why?*

**Response:** $R^2$ values fluctuated due to different characteristic of various pollutants. For the newly generated results, the range of $R^2$ in this study were 0.85~0.95 ($PM_{2.5}$) and 0.88~0.95 ($O_3$), comparable to those reported in earlier studies, e.g., 0.906±0.001 ($PM_{2.5}$) and 0.63~0.92 ($O_3$) (Hou et al., 2022; Ma et al., 2021).

*Line 144: explain why using meteorological variables randomly resampled from the study period (2014−2020) is fit for purpose for this particular study? Note there are different methods - they are there for different purposes.*

**Response:** We used the deputy design in RF and BRTs models for processing meteorological normalization. Based on invariant predicted hourly values during most of times in a year, the randomly resampled 1000 types of meteorological conditions from the study period (2014−2020) had demonstrated reasonably well representative. We also try 2000-time and 3000-time predications for meteorological normalization, the difference is negligible. The averaging 1000-time predictions has been also used in Hou et al (2022). The reference has been added.

*Line 149: why not enhanced secondary pollution?*

**Response:** Agree. The enhanced secondary pollution cannot be excluded and thereby added in the revision.

*Line 283: O3 changes are the result of emission changes of O3 precursors and changes in chemistry. Revise*

**Response:** Revised as suggested.

*Line 301-302: I don't understand the argument here. Please explain in more detail*

**Response:** The part has been revised as "Thus, the 39%-55% $O_3$ increases from 2014 to 2020 likely attributed to the emission-driven enhanced $O_3$ formation. In addition, the first three PCs values for ($NO_2$+$O_3$) were smaller than those of $O_3$ by 10%-16%, which represented the reduced $O_3$ depletion via the titration reaction (Li et al., 2019a; Wang et al., 2017)."

*Line 309: Would it be more reasonable to present $O_3$ and $NO_2$, and then $O_3$+$NO_2$*

**Response:** Agree. The order has been adjusted.

*Figure 3: The prediction appears to be relatively poor. Fig. a shows three distinct areas. It appears to me There is something wrong – I would suggest that the authors check the codes and re-run the results, particularly including other parameters mentioned above.*

**Response:** We checked and re-run the codes by adding other meteorological factors as above-mentioned, and adjusted the range of y-axis. The original y-axis didn't start from point (0,0) and were thereby corrected in the revision.

*NO2+O3 is often defined as Ox. But you cannot add these two together based on mass concentration. Please turn NO2 and O3 into ppb first and then add.*

**Response:** The concentrations of ($NO_2$+$O_3$) were calculated by adding those of them with the molecular weight correction, i.e., [$NO_2$+$O_3$] = [$NO_2$] *48/46+ [$O_3$]. This has been clarified in the revision. We are sorry for missing the information in the original method.

*Discussions are inadequate, more or less repeating the results rather than an in-depth discussion. Two suggestions: interpret the results, in the contexts of literature and clean air policies; examine the implications of the results – e.g., what policies are effective and what are not. Suggest to remove all discussions in the Results, and move to Discussions as needed*

**Response:** Discussion in the original manuscript aims to use the self-developed independent method to constrain the uncertainties of the percentage changes of air pollutant annual average concentrations estimated by the two deweathered methods and one decomposed method. Based on the comments, the authors realize that it is misleading as an independent Discussion Section. In the revision, we re-organized Results and discussion Section. Discussion section in the original manuscript has been converted to Section 3.4 with subtitle as "Constraining analysis uncertainties". We hope that subtitle can help solve the misleading to some extent.

Air pollutant emissions have not been updated in the annual reports of ecology and environment issued by local governments at the city level since 2014 in China. In the south China, the air pollution has been largely relieved before 2014. The implemented clean air policies therein were not issued in public domain, except the national clean air policies, i.e., Air Pollution Prevention and Control Action Plan in China (2013-2017), and Three-year Action Plan to Fight Air Pollution (2019-2021). The national polices are too general to link with the concentration trends of air pollutants, particularly for no detailed implement measures and corresponding air pollutant emission data. Thus, the emission-driven trends in air pollutant concentrations are critical to accurately evaluate the achievement of every-three-year national targets in south China.