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Comment on acp-2022-232

Benjamin Wells (Referee)

Referee comment on "Statistical and machine learning methods for evaluating trends in air quality under changing meteorological conditions" by Minghao Qiu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-232-RC2>, 2022

Overall, I think this is a novel approach to the characterization of meteorological adjustments to air pollutant concentration trends and it is worth publishing. The use of air quality models to create a synthetic dataset by which meteorological adjustment methods can be evaluated has not been previously published that I am aware of. The selection of air quality model, statistical adjustment methods, and regions are appropriate and the presentation as a whole is clear and well thought-out. I have several minor concerns listed below that I think could be addressed through minor revisions.

- From the manuscript, it isn't clear how the linear trend estimates are calculated. Are they calculated using linear regression or some other method (e.g. Theil-Sen)?

- One of my biggest concerns is that the time period (7 years for U.S., 5 years for China) is too short to calculate meaningful trend estimates, and because of this, the trend estimates themselves may be a source of additional uncertainty. I fully understand the time and resource constraints of running chemical transport models, therefore, I'm not suggesting that this must be done, but merely that this is addressed as a limitation in the discussion. As a potential future research application, one could expand this type of analysis to a set of model runs evaluated over a longer time period, such as EPA's EQUATES series for 2002-2017 (<https://www.epa.gov/cmaq/equates>). The emissions from these

- Another potential source of uncertainty in this application is the choice of meteorological year for the set of model runs where the meteorology is held constant. For example, as the authors describe, the 2011 year which was held constant for the U.S. was unusually hot and dry throughout the central region of the country. As a sensitivity analysis, I think it would be useful to see how much the predictions change by holding the meteorology constant using other years. For example, 2013 and 2014 were cooler and wetter than average for much of the U.S. For the current manuscript, I think it would be sufficient for the authors to address this point in the discussion.

- The choice of the June-August period for ozone does not capture the period of maximum ozone concentrations for all regions of the U.S. For example, the southeast U.S. typically sees its highest ozone concentrations in April or May, while California may experience peak ozone in September or October. I think a period of April to October would be sufficient to capture the peak ozone concentrations in all regions of the U.S. Again, nothing needs to be redone, but it would be useful to discuss this in the manuscript.

- As far as the interaction between meteorological and emissions-based effects, I agree that this is both a concern and a major challenge for any meteorological adjustment approach, and ultimately, it may not be possible to estimate the magnitude of these interactions. One major source of these interactions, especially in recent years, is wildfires: dry meteorological conditions contribute to more wildfires, and more wildfires contribute additional emissions. Wildfires are especially difficult to capture in a chemical transport model due to their unpredictability and the difficulty of characterizing their emissions. As wildfires can be an especially large contributor to PM_{2.5} concentrations, it would be useful to see them discussed in the context of their contribution to met/emissions interactions and overall uncertainty.

- While I don't plan to list them all here, I noticed several typos and minor grammatical errors in the manuscript while reading it.