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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observed changes in the temperature dependence response of surface ozone under NO_x reductions" *by* Noelia Otero et al.

Anonymous Referee #1

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In this work the authors examine longterm O3 data from surface stations in Germany, comparing hourly ozone changes with various ambient conditions such as temperature and NOx levels. Using generalized additive models across two halves of the total temporal domain (1999-2018) to model and combine the influence of these driving factors, the authors conclude that these two time periods show differences in the O3-temperature relationship, driven only in part by observed NOx emissions reductions over that period.

While the topic of pollution production and its atmospheric influences is important and complex, I think this manuscript needs considerable development to be considered a novel and meaningful contribution to the existing literature on the subject. In particular, I have the following concerns:

- The choice of temporal division (analyzing the full time series in two evenly divided chunks) strikes me as arbitrary and problematic. Unless the year 2009 has some special significance that is not discussed in the text, I see no reason to set up the binary comparisons between time periods as performed here. The division is ostensibly made to compare a higher NOx time period (1999-2008) to a lower NOx period (2009-2018), but not only is this assumption not necessarily valid for all stations during all years (see Figure 2), it also neglects the wide variety of other changes that may have occurred over the two decade span that could influence ozone and its relationships with ambient conditions. Compared to other methods of distinguishing between higher and lower NOx conditions (for example, by leveraging the so-called weekend effect), comparing consecutive decades individually and ascribing their differences to only one factor (NOx emissions) strikes me as flawed. The authors' observation that "decreasing NOx concentrations are not the only factor causing the observed changes" underscores this fact, and raises the question of why they chose to dissect their long term data set in this fashion at all. I would recommend rethinking the approach here, and identifying a methodology that is less subject to non-stationarities in external variables.

- On a related note, while this study considers an assortment of ozone-influencing covariates alongside temperature and NOx, it conspicuously ignores others. For example, VPD is considered to represent dry deposition rates, and temperature is identified as a surrogate for biogenic emissions, but there is no mention of changes in the plants responsible for these effects in the first place. Changes in land cover, whether in the form of ongoing biosphere growth and aging, losses due to anthropogenic land development, or shifts in plant speciation can all have drastic impacts on biogenic emissions, their temperature dependence, and other surface/atmosphere connections such as ozone deposition velocities. It is surprising, therefore, to see no model inclusion or even mention of how changes across the temporal domain could influence O3-temperature dependence in this study.

- The primary conclusions of this paper are generally either unsurprising and under-developed. The correlation between NOx emissions and the O3 climate penalty has been consistently observed, modeled, and dissected in studies performed all over the world, and there doesn't seem to be much added to the conversation here. Furthermore, areas of potential interest, such as the observation that "NOx reductions alone can not explain the changes in the temperature dependence of O3" go largely unexplained, leaving open the questions that could lead to more significant and meaningful answers.

- Figure and text quality are highly inconsistent, with some glaring issues scattered throughout. Puzzling color and layout choices make it difficult to make sense of visualizations. For example, Figure 1 includes a color scheme to show station altitude, but these colors show no obvious consecutive progression, making the ready comparison of sites awkward and unintuitive. Panels of contour and ribbon plots might show features of interest, but, aside from textual description of very basic features, they don't receive much development or interpretation in the text. Grammar, spelling, and phrasing mistakes often impede manuscript fluency and flow.

- Data filtering seems to be extremely strict, and it is unclear how this filtering process may itself have resulted in spatiotemporal differences. Were there any discernable patterns with respect to the percentage of hours kept for analysis across station and year? Could changes in the frequency of removed hours over time, or between stations, confound comparisons? This seems like a potentially large source of statistical artifacts, if not examined and accounted for.

- Model selection deserves more attention and description. It is stated that the goal was "a common model well defined across all of the stations", but later it is mentioned that "the model selection procedure was applied separately at each station and period." Does this mean that forward selection was performed individually by station and time? If so, this is a major problem in the interpretation of model output. If not, it's unclear how these two statements are reconconciled. How was forward selection applied in a way that resulted in a common model across all stations, while also being applied

separately by station and period?

All in all, I think that this manuscript contains the foundation for an interesting paper, but that it is not there yet. I recommend that it be rethought and far more significantly developed, in particular addressing some of these concerns, before being considered for publication again.

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