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Interactive comment on "The potential effects of climate change on air quality across the conterminous U.S. at 2030 under three Representative Concentration Pathways (RCPs)" *by* Christopher G. Nolte et al.

Anonymous Referee #2

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Nolte et al. estimate the impact of climate change on U.S. air quality by using a pipeline of models (CESM->WRF->CMAQ). The results are not particularly novel, but the method employed by the authors is a step forward in refining estimates of the effects of climate change on air quality. The results are an important addition to the literature. The authors find that impacts on ozone and PM have important regional and seasonal subtleties, but generally reveal an increase in ozone, decrease in nitrate, and increase in organic matter. The manuscript is well-written and presented very clearly. I recommend publication in ACP following sufficient response to the following minor

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Discussion paper

comments.

General Comments - The authors spend a lot of real estate discussing model biases in temperature and precipitation. The principal source of bias in Nolte et al. (2008) was temperature, but what about other factors? After all, part of temperature's explanatory power arises from its ability to be a catch-all for many factors. In the latter half of the paper other factors are revealed to be important (related to T), including cloud cover, isoprene emissions, and stagnation (circulation). If evaluation of air pollution meteorology is important, these additional factors must surely be of interest.

- The authors assume the results of the model are truth, but indeed the model is programed with the assumptions that HNO3 is less soluble at higher temperatures (well known to be so, but what is the sensitivity?) and that isoprene emissions increase with temperature. But are the sensitivities of these factors to temperature accurate? This seems more important than any absolute bias in temperature, the changes in meteorology and air quality are of greatest interest here. At the very least, more discussion of parameters/observational evidence underscoring the principal impacts is necessary, e.g., change in isoprene emissions.

- 11-years is still potentially too short to average out interannual variability and obtain a robust climate signal. It can take decades and an ensemble to do that. I think that this manuscript is a step in the right direction in terms of incorporating multiple climate scenarios and a longer record, but it still need to acknowledge that interannual variability can still distort results.

Specific Comments Title: Perhaps a nitpick, but the parenthetical seems unnecessary in a title

Abstract, line 10-12: It might be worth referencing the changes here to be driven by climate change only. It is a little confusing since the emission scenario is mentioned in reference to GHGs and not O3/PM precursors

Page 4, line 29 - page 5, line 2: Is this description of emission changes related to the lateral boundary condition simulation? If not, this should be reworded to make this clearer given its following of the discussion about the boundary conditions.

Page 5, final paragraph: Is the discussion of max/min temperatures really necessary here since it is rehashed in depth in the following paragraph. This was a bit jarring on the first read.

Page 6: Why are these evaluations important? There should be some discussion here about what a bias in temperature and/or precipitation means for the present study. How much is gained by evaluating the maximum and minimum temperatures, in addition to the daily mean? This goes along with the second bullet in the General Comments section.

Page 6, Line 31: Be a little careful here because the spatial similarities could arise from the common baseline, which is subtracted from each simulation.

Page 8, Line 13-14: Why are sulfate and ammonium decreasing?

Page 9, Line 12: This doesn't really support the conclusion since the model is programmed this way. Only observational evidence would really support the conclusion.

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