Anonymous Referee #2
The referee's comments are noted in italics below, followed by our responses.

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 comments.

We thank the referee for these 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.

We agree with the referee that those other factors are important for air pollution meteorology, which is why we examine them in this paper. We discuss Nolte et al. (2008) to place the current manuscript in the proper context with prior work, but an evaluation of the "current" climate variables in that study is outside the scope of the present manuscript. Here, we focus our analysis of historical (1995-2005) climate fields on temperature and precipitation, the two most important and most commonly evaluated variables in regional climate modeling studies. As the reviewer notes, temperature is important due to its relationships with other variables. Rather than evaluate mean values for additional climate variables, we chose to investigate temporal and spatial variability in order to understand the implications of these changes for future air quality.

- 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.

We agree that there is uncertainty in how isoprene and other biogenic VOC emissions will change in response to elevated temperatures and CO_2. The Conclusions section contains a paragraph discussing some caveats and limitations of our study, which includes a discussion of isoprene:
"Although biogenic emissions of VOCs were estimated using the downscaled meteorology, our modeling did not consider changes to prevalence and distribution of species of vegetation, or the potential leaf-scale inhibition of biogenic isoprene emissions due to elevated atmospheric CO_2 concentrations (Tai et al., 2013; Sharkey et al., 2014)."

- 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.

We agree with the reviewer's concerns and have expressed this within the last two sentences of the second-to-last paragraph:

> **Finally, there is substantial interannual variability in air quality due to year-to-year changes in meteorology. Though we conducted four sets of 11-year continuous simulations to account for interannual variability to the extent that our computational resources made practicable, 11-year simulations are insufficient to represent the full range of natural variability in the earth's climate system (Garcia-Menendez et al., 2017).**

Specific Comments

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

We have removed the parenthetical "(RCPs)" from the title of the manuscript.

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

The abstract has been revised as suggested by the reviewer:

> **The analysis isolates the future air quality differences arising from climate-driven changes in meteorological parameters and specific natural emissions sources that are strongly influenced by meteorology. Other factors that will affect future air quality, such as anthropogenic air pollutant emissions and chemical boundary conditions, are unchanged across the simulations.**

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.

The last sentence of the previous paragraph reads "All other input variables, including anthropogenic emissions, chemical lateral boundary conditions, and land use and land cover classifications, were unchanged across the air quality modeling scenarios." We have restructured this paragraph to place the discussion of domestic emissions prior to the discussion of lateral boundary conditions, in the same order as mentioned above. We additionally specify that the emissions referred to in this paragraph are "air pollutant" emissions.

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.

As noted in the previous paragraph, the evaluation of the historical period is focused on (1) **monthly and seasonal means** and (2) **selected percentiles** of regional temperature and precipitation. The paragraph at the end of page 5 and Figure 2 pertain to seasonal means. The following paragraph and Figure 3 discuss percentiles of daily maximum temperatures for different regions of the U.S. We feel that analysis of the distribution of temperatures (and ozone concentrations) is a strength of our paper, which goes beyond the use of mean temperatures as has been done in most prior studies.

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.

We analyze daily maximum and daily minimum temperatures, along with precipitation, to evaluate how well the model is representing the climate during the historical period. As stated above, we feel that analysis of distributions of temperature and ozone is a strength of this paper. Good model performance for the historical period provides some degree of confidence in the use of the model for future climate projections. In addition, MDA8 O3 concentrations are often more strongly correlated with daily maximum temperatures than with daily mean temperatures.

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.

Here we show the changes in seasonal mean MDA8 O3 for each of the RCPs at 2025-2035 relative to the 1995-2005 baseline. We agree with the Referee that plots of changes are all affected by the common baseline, but we do not see how that undermines the point we are making, which is that the locations of the seasonal changes are generally consistent across the three scenarios.

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

As shown in Fig. S8 of the SI, the changes in sulfate are small (less than 0.3 $\mu g\ m^{-3}$), are mixed in sign, and vary by season and scenario, likely due to a complex combination of factors including chemical formation rates and changes in transport. The changes in ammonium are somewhat larger and more clearly track the changes in nitrate to maintain thermodynamic equilibrium.

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.

This sentence has been deleted.