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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of emissions and climate change on ozone in the United States under Representative Concentration Pathways (RCPs)" *by* Y. Gao et al.

Anonymous Referee #3

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This paper examines ozone change within the U.S. in RCP 4.5 and RCP 8.5 between the 2000s and the 2050s. It uses the CAM-chem model to generate the boundary conditions and the WRF-CMAQ to model ozone at 12x12 km within the U.S. The paper examines the sensitivity to boundary conditions and methane concentrations and examines the ozone response to heat waves.

I found the paper interesting and it presents interesting analysis. However, in some places I feel the paper needs clarification and the analysis needs improvement.

Major Comments:

The plot showing the zonal mean concentrations from CAM-chem should be revised. (1) The focus of this paper is on the US, not the zonal mean. (2) The authors are using the high resolution WRF-CMAQ model to examine the ozone over the US. This model has different emissions than CAM-chem and different resolution. So why show zonal mean distributions in CAM-chem? To me, it only make senses to examine the latitude height cross-sections using WRF-CMAQ. Moreover, I would suggest the authors show these cross-sections over the longitudes of interest (i.e., the US) as well as a more circumscribed latitude range (why show the S.H.?). In addition, I would recommend including a panel showing the sensitivity to reducing the methane concentration. The impact of methane (and the boundary conditions) seems to be essential to determining the future response of ozone over the U.S.

In addition, some of the explanations given in this section seems speculative. There are a number of instances where the authors explain a feature of the figures without giving adequate proof.

The slides showing the PV and their interpretation are not convincing. (1) Increased PV can be indicative of more stratospheric influence, but on the other hand it might simply suggest increased cyclonic flow. (2) It is not clear that this change in PV between the simulations is robust. The authors need to test its significance to determine if it is indeed robust, but point (1) suggests that even if robust an interpretation of this figure is difficult.

More thought needs to be taken in testing the significance of the author's conclusions. This is particularly true for Figure 7. However, it also applies to interpretation of almost all the author's figures. I'm not suggesting the authors necessarily change their figures (although in many cases it would be convenient to show where the results are significantly different), but to be careful of the interpretation. For example, I have no problem in the author's showing in Figure 9 differences in mean ozone and % differences over 75 ppbv and 60 ppbv; however, when the authors claim that there has been a change between one period and another they need to show that this change is not a statistical

fluke (i.e., that it is significant).

Minor Comments:

1) The descriptions of CAM-chem and WRF are not at all consistent. While a great-deal of detail is given on the WRF parameterizations the parameterizations within CAM-chem are not mentioned.

2) Could the authors clarify the relationship between the models? I assume the CAM-chem meteorological initial and boundary conditions were used to drive WRF and that the WRF meteorology was used in CMAQ? Please be explicit here.

3) Page 11321, line 13. "present climate (1850-2005)..." This entire period doesn't really correspond to the "present climate".

4) Species mapping. Which chemical mechanism is used in CMAQ? CAM-chem has many more species than indicated in this mapping.

5) The algorithm used to calculate emissions in CMAQ was not altogether clear to me. Did CMAQ use emissions as calculated in 2005 for the RCPs? Were these RCP emissions then scaled using the EPA emission inventories and SMOKE for the years 2001-2005? Or did CMAQ use EPA emissions inventories for the years 2001-2005?

6) Page 11324 "However, these statistical methods have not been used in climate studies". Assuming CAM-chem was not forced by meteorological analysis, but was run in the historical period using GCM winds and that WRF was forced by CAM-chem meteorology it is hard to understand the emphasis the authors place on paired space and time evaluation (p11324). The model will not replicate the observed meteorology so it makes little sense to compare against observations taken at the same time (in fact monthly averages probably make sense). The authors do not say what the temporal resolution of their emission dataset is. I am assuming it does not include variations on the daily timescale. So this emphasis on paired space and time evaluation is confusing. I assume paired space-time comparisons means comparing at the same point and time

as the measurements were taken. Please clarify how the model evaluation was made and the meteorology used to drive the WRF-CMAQ simulations.

7) The authors do not say what timescale they compare the model and the observations or using what ozone metric (i.e., is it MDA8?) They do not say where the measurements are from or what part of the country is covered. More information is needed here.

8) What do the author's mean that a metric is the least biased (p 11325, l 19)?

9) It is not clear how Figure 6 and 9 were created. Were they created by averaging the ozone over each region and then showing the distributions of regionally averaged ozone, or all points within a region separately into the distribution or how? The methodology changes the interpretation somewhat. Please clarify.

10) I'm having a difficult time understanding the author's explanation on pages 11332, line 16 through 11333, line 8. They are trying to explain why the three regions have show relatively little impact of heat waves on ozone. However, their argument has to do with the number of heat waves in these regions and/or their duration. It is perfectly possible to have a small number of heat waves but for each wave to have a large impact on ozone. Thus, the argument for the impact of heat waves on ozone should not depend on the number of heat waves.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11315, 2013.