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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 #2

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This paper examines how seasonal ozone patterns changes due to climate and emission changes under the RCP 4.5 and 8.5 emissions and climate scenarios. The discussion of background boundary conditions versus regional domain influences on surface ozone is insightful. The linkage between high ozone events and heat waves is highly relevant for considering the effects of climate change on surface ozone. Overall this is a most interesting manuscript that most certainly adds to the literature on future changes in ozone air quality. However, there are several issues that merit clarification.

1) The paper shows some results from the CAM-Chem global chemistry transport model and the CMAQ regional chemistry model but these could often be better linked

and clearer. A comparison of evaluation metrics from CAM-Chem and CMAQ in Table 3 would be most useful. Moreover, zonal-mean plots from CAM-Chem form the basis for interpretation of seasonal mean surface ozone. An equivalent of Figure 5 for surface ozone from CAM-Chem would be highly beneficial to draw inferences about the changes in seasonal mean ozone projected by CMAQ under the two RCP scenarios that arise from global model changes.

2) Although CMAQ captures the future emission changes it has not been shown that the three years of future climate (rather than the 10 years displayed in figure 4) captures the climate change signal in ozone.

3) The paper highlights the role of enhanced STE on enhanced surface ozone in CMAQ. Given the nested model set-up some further evidence for this would be beneficial.

4) The shifts in ozone distributions during heat wave and non heatwave events are interesting but for the period June-October the number of non-heatwave days must far outweigh the number of heatwave days.

These points are elaborated on further below under general comments. The text could be condensed and more focused and clarity could be improved throughout (hence the numerous comments below). Finally the English could be substantially improved as the definite article is frequently omitted.

General comments:

1) There is some repetition in text in section 2 and section 3. It would be clearer to have all the text pertinent to dynamical downscaling in section 3. In section 2, there is rather detailed text on the CESM model. There is also detailed text describing how CMAQ is driven by initial and boundary conditions from CAM-Chem. However, there is no text explaining the methods of coupling between CESM and WRF to produce future climate at 12km horizontal resolution. This is likely to have been the most challenging aspect

of the study. Hence text should be added to section 3 to describe how WRF is driven by CESM. Perhaps both WRF and CMAQ are driven by 3 hourly outputs but it would be useful to state this. If space is short some of the more detailed text in section2 on model components could be removed.

2) Why were different period lengths chosen for the present-day (4 years) and future (3 years) simulations? This is rather surprising. Furthermore, the sentence "while future climate in the 2050s potentially captures enough climate change" is vague. Given that there are only 3 years for the future simulation period more evidence should be given that the climate change signal in ozone is clear and greater than that due to interannual variability. Since the CAM-Chem results cover the continuous period from 2001 to the end of the 21st century it should be easily possible to show that the climate change is captured in this subset of years.

3) Fig 2 may well be useful but it is unclear what is exactly shown – are these daily or 3 hourly fields, and why there would be differences other than those due to interpolation between the different model grids? It would be useful to show how the ozone field in CMAQ evolves away from the boundary.

4) Fig 3 and associated text is only useful if intercontinental transport effects are discussed in greater detail. Otherwise a map showing and text describing emission changes in the US region is more relevant.

5) The BVOC BEIS modelling system is mentioned in section 3 but some description of how it simulates BVOC emissions and its sensitivity on meteorology and CO2 and appropriate references should be given. For example, is the CO2 inhibition effect included? It would be useful to add the calculated values to Table 2 since they may (or may not) strongly influence ozone distributions. This would also enable further clarification of whether there may be differences in future simulated ozone between CMAQ and CAM-Chem as suggested (11324 line 5).

6) The whole section on statistical evaluations starts with the apriori assumption that

the regional model will compare better to observations. If this is the premise, this section could benefit greatly by showing this by adding values from CAM-Chem to Table 3 or at least adding similar maps of ozone changes from CAM-Chem to figure 5 (or adding these to figure 4). The text describing Table 3 results could benefit from clearer writing and a description of the "space and time pairs" (e.g. numbers of space-time pairs, map showing sites used, temporal resolution). Furthermore, there are a number of speculative comments and it is unclear whether climate or climate-chemistry studies are being discussed. The evaluation metrics discussed refer to ozone. The text discusses that climate studies do not use these same statistical methods. Climate model evaluation studies use many sophisticated statistical techniques e.g. Taylor diagrams and the fact that they do not use those statistical metrics designed for ozone by the EPA is not a flaw per se. I would like to see a more succinct and less speculative write-up of this section pertaining to regional chemistry model evaluation. In addition: a) are all four statistical quantities needed since the NMB and MNB and similarly the NME and MNE are simply related by 1/N, b)the concept of cut-off values need to be explained more clearly.

7) The paper highlights STE enhancement of surface ozone in the 2050s climate, but are the high altitude changes in STE relevant for surface air quality? Fig 7 shows some possible evidence of this. This figure should be moved to section 5 where the other CAM-Chem results are discussed. Given the model set-up can changes in STE in CAM-Chem can feed through to CMAQ? The CAM-Chem boundary conditions would need to capture the effect of increased stratospheric intrusions (Fig 7) that may occur far away from the domain boundary. It may well be that it is the higher methane concentrations that are solely responsible for the higher surface ozone in RCP 8.5. As such a strong connection is made between STE increases and seasonal and MDA8 ozone changes between present-day and future in figures 5 and 6, some inferences from the literature that discuss STE influences on surface ozone would be greatly beneficial. As discussed above, the more relevant figure for comparison to figure 5 is a seasonal map of lowest model layer changes in ozone from CAM-Chem. Indeed, it would be most

interesting to see if the titration effect is seen in CAM-Chem. Collette et al. (2012, ACP) suggests that global models may be able to simulate such effects.

8) Regarding Figure 4 this figure should at least should use the same periods as used in Figure 5. As it stands, spring and winter features in the lower troposphere in Fig 4. appear similar. The scale should be improved so that values of $\sim$1 ppbv as discussed in the text can be clearly seen. There is a paragraph of text describing the results from a sensitivity experiment that are not shown. Either this figure should be included, or the relevant text condensed or omitted to achieve a more focused discussion.

9) Some further explanation is needed for the methods of determining MDA8 during heatwave and non-heatwave periods. How do the sample sizes vary between these two categories? It would seem likely that there are far more non-heatwave days than heat wave days in the period June-October. It would also be useful to have some text or additional panels that show these quantities and ranges for the present-day period. The explanation for the lack of distinction between MDA8 during heatwave and non heatwave events in three of the nine regions under RCP 4.5 is based on the values of annual heatwave events being less than "3" in these regions. Yet three of the remaining regions also have heat wave events less than "3". A similar argument for RCP 8.5 is less clear. This is why sample size may be important. A statistical test that enables the authors to comment on whether the shifts between heatwave and non heatwave events are significant or not would be most beneficial. Finally, it would ease interpretation to have the values for 60 and 75 ppbv (middle and top rows of each panel) in Fig 9 given as ppbv (as in the bottom row) rather than percentages.

Specific comments:

The abstract could be clearer in distinguishing between global results from CAM-Chem and regional ozone air quality from CMAQ.

11316, Line 8: the wording "trend" is inappropriate. These are differences between two 10-year periods. Also 11328, line 22.

11317, line 22: the text on "impacts of climate . . . on air quality with spatial resolutions of .." reads awkwardly . It would be simpler to state that global chemistry models have resolutions of 1-2 degrees or more.

11317, line 26: global studies of?

11318, line 15: explain "five-summer"

11318, line 18 The linkage between the two results (climate change only, climate change + emission reductions) found by Lam et al. (2011) is rather unclear.

11319, line 17: "under the same climate scenarios" is unnecessary.

11319, lines 26- 11320 lines1- 7, 17, 11325 line 7: "the" should be inserted before model names and before "model top".

11319, line 8, representation of ? in the atmosphere.

11321, line 20, Re-phrase "comparison with observations of CAM-Chem".

11321, line 25 add "ozone" before "air quality".

11322 line 16: Clarify what "driven by the default profile initial and boundary conditions" means.

11323: line 1: change "for example" to "as an example".

11323: line 7: the 1st sentence of this section is unclear, different patterns compared to?

11323: line 14: India is already a major emission source region

11324: line 15: explain what is meant by "retrospective studies".

11324, line 16, "However. . . climate studies.." does not seem relevant.

11324, line 19: Evaluation is usually performed based on the quantity or metric of interest. To state that global models compare zonal and monthly mean values due to

their coarse resolution is overly speculative.

11325, line 22 re-phrase "at present climate condition" and explain what is meant by "meet the criteria".

11327, line 6 clarify "a stronger chemical reaction".

11328: re-phrase "reduced photochemical reactions" as it is the rate that is sensitive to temperature.

11329: line 14: is the higher ozone near the surface in winter in Figure 4 really due to effects at the stratosphere? Please add any appropriate references. Also explain the role of intercontinental transport more clearly referring to Figure 3.

11332, Are the temperatures those simulated by WRF? Explain how future climate as simulated by CESM affects WRF daily temperatures. Comment on whether the heatwave results for RCP 8.5 are the same as those reported in Gao et al. 2012.

11333- 11334 line 5: The conclusions on model evaluation are rather vague and long.

11334, line 15, more details of?

11334, line 18 should be "titration".

11335, line 2: Fig 8 shows the maximum regional heat wave duration is 7.6 days rather than 10 days.

Table 2: clarify "projection factor". Also methane emissions are given. Clarify whether methane emission or concentrations were used in the CAM-Chem or CMAQ simulations.

Table 3: explain that the benchmark is only for two of the metrics listed.

Figure 5 caption: should mention "surface".

Figure 8 caption: further clarity is needed here, explain that these heatwave metrics are based on temperatures simulated by WRF 2.1.

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