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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Decadal regional air quality simulations over Europe in present climate: near surface ozone sensitivity to external meteorological forcing" *by* E. Katragkou et al.

Anonymous Referee #1

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General comments:

The authors present a decadal modeling study investigating the impact of using two different regional climate fields on summer and winter ozone concentrations over Europe under present-day conditions. One set of regional climate fields is derived by using ERA reanalysis fields to drive RegCM3 while the other set is derived by using output from ECHAM5 to drive RegCM3. While the analysis presented in this manuscript focuses on present-day simulations, the ultimate purpose of using the ECHAM5/RegCM3 system to drive CAMx is to study the impacts of climate change on air pollution. Given this premise, I would have expected analysis showing that the

ECHAM5/RegCM3/CAMx simulations for the present day climate are able to reproduce observed meteorological and air quality climatology similar to simulations by the ERA/RegCM3/CAMx system that have been evaluated by Tegoulias et al. (2009, in preparation). However, the manuscript only describes differences between these two simulations and leaves open the question whether the ECHAM5/RegCM3/CAMx is a suitable tool for addressing climate change impacts on ground-level ozone over Europe. I have a number of major questions and concerns about this study that I would like to see addressed before I would recommend publication of this manuscript.

1) Because the model evaluation reference Tegoulias et al. (2009, in preparation) only covers the ERA/RegCM3/CAMx simulation and is not available yet in any case, it is difficult to judge the quality of the model results presented in this study, especially the quality of the ECHAM5/RegCM3/CAMx simulations. Therefore, a comparison of simulated ozone and NOx concentrations from both simulations against observations should be included in the analysis.

2) The average temperature differences of up to 2-4 degrees between the ERA and ECHAM5 driven RegCM3 runs are very large and require further analysis. Assuming, as the authors do on page 10,680, that the ERA driven run is closer to real atmospheric conditions, one would conclude that these differences indicate model error of the ECHAM5 driven RegCM3 simulation. For example, it appears that the ECHAM5/RegCM3 simulation underestimates seasonal variability (it is warmer than the ERA run in winter and cooler in summer), i.e. does not represent present-day climate adequately. Before analyzing ozone and NOx concentrations from the CAMx runs, I strongly recommend that the authors present a thorough comparison of both the ERA/RegCM3 and ECHAM5/RegCM3 runs against meteorological observations. If there are significant discrepancies between observations and the ECHAM5/RegCM3 runs, I am not sure it would be justified to use these fields to drive an air quality model under either present day or future air quality simulations.

3) Since the goal of the manuscript as stated in the title is to investigate the sensitivity of

ozone to external meteorological (not chemical) forcing, NOx fields should not be used as a predictor for ozone in the regression analysis. Consequently, the finding stated in the abstract that changes in NOx explain about 40% of the ozone variability is true but not related to the objective of the paper. Rather, the question is which meteorological differences cause these NOx differences in the first place. Furthermore, in addition to the meteorological variables already considered in the analysis, I recommend including boundary layer height as an additional variable.

4) The use of correlation coefficients between maps of seasonal average changes in variables due to the different RegCM3 simulations to determine significant linkages between variables needs to be further justified. First, the analysis needs to take into account the effect of spatial autocorrelations in each of the maps. Second, since ozone is a secondary pollutant with a lifetime of up to several days, spatial patterns of changes in ozone could be shifted with respect to spatial patterns of changes in relevant meteorological variables due to transport and chemical processing.

5) Results for spring and autumn should also be provided.

Specific comments:

Page 10,676, abstract, lines16-18: "biogenic emissions ... are more temperature than radiation dependent". This statement is repeated several times in the manuscript. To judge whether it really is a major finding worth stating in the abstract and summary, the authors need to provide the equation of how isoprene emissions are calculated in their model from landuse vegetation data, temperature, and radiation. For example, if the parameterization is proportional to the square of temperature but linear related to solar radiation, the results of the correlation analysis would be expected. This equation should be provided in section 2.

Page 10,677 line 7 – page 10,678 line 5. This section could be removed because this manuscript does not deal with the air quality impacts of climate change.

Page 10,679, lines 7-10. Please provide a reference for the specific RegCM3 simulations used in this paper. Where have the simulations been evaluated? Which biases and errors were found? How did the simulations capture the location, frequency and persistence of synoptic transport patterns?

Page 10,679, lines 23-25. Why was the top layer for CAMx set to such a low value? Differences between the two RegCM3 fields are expected to also include upper air longwave patterns that can affect transport of ozone which may mix to the surface, especially in springtime during convective events and tropopause folding events.

Page 10,679, lines 25-26: Please specify the ozone values used at the lateral boundaries. Did the lateral boundary conditions vary by season? Figure 1 shows quite different ozone values at the boundaries for the summer and winter simulations.

Page 10,680, line 4: Please provide a reference for the landuse dataset used to calculate biogenic emissions. As stated above, please provide the equation for the calculation of biogenic emissions, in particular the functional form of the dependence on temperature and solar radiation.

Page 10,681, lines 5-7. This statement is based on an outdated (1991) U.S. EPA guidance document. The current guidance document was published in 2007 and is called "Guidance on the Use of Models and Other Analyses for Demonstrating Attainment of Air Quality Goals for Ozone, PM2.5, and Regional Haze", EPA-454/B-07-002, 262 pages. This final guidance does not suggest any MNBE and MNGE threshold criteria to determine acceptable model performance in regulatory applications (see section 18.6 of the above document). Instead, in the context of using regional climate / air quality models for studying the impacts of climate change on air quality, model evaluation should compare observed vs. simulations relationships between meteorological variables and ozone, observed and simulated distribution functions, intra- and interannual variability, the frequency and persistence of observed and simulated high ozone episodes, etc.

Page 10,682, lines 24-26. A correlation of -0.48 is not a minor effect.

Page 10,683 line 13 – Page 10,684 line 2: Almost all of these studies looked at summertime, so this paragraph is not the best way to introduce the results of Figure 5 which shows analysis for wintertime.

Page 10,684, lines 10-11: Which conclusions can be drawn from this finding?

Page 10,687, section 3.5: Please provide differences in domain total isoprene emissions for both runs for both summer and winter.

Page 10,688, section 3.6: Please discuss the role of spatial autocorrelations in the individual fields. In other words, the 9200 cases are not independent of each other – how does this impact the results of this analysis?

Page 10,692, lines 7-9: Given that the differences between the ERA and ECHAM5 driven run cannot be considered as random because the ERA driven run is closer to real atmospheric conditions, in my mind the magnitude of these differences raises serious questions about the suitability of the current modeling system to study the impacts of climate change on ozone.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10675, 2009.