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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.

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We would like to thank Reviewer #1 for the detailed and constructive comments.

Our response follows point by point:

GENERAL COMMENT: 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.

RESPONSE: The common strategy in climate studies is to validate a model with what we say “perfect boundary conditions” experiments. The ERA40/RegCM3/CAMx is such a perfect boundary conditions experiment with the ERA-40 reanalysis meteorological fields constraining RegCM3 simulation for the period 1991-2000. The ECHAM5/RegCM3/CAMx simulation of the present decade 1991-2000 is the “control” experiment which is the suitable simulation (or in other words the basis) to estimate the future climate change impacts by comparison with the ECHAM5/RegCM3/CAMx simulations in the future decades thus counterbalancing internal model errors in present and future of the GCM runs (in our case ECHAM5). Of course, we are also interested to know, what are the differences between the “perfect boundary conditions” experiment and the control experiment and that was the purpose of this work. It should be clearly stated that the differences between ERA40/RegCM3/CAMx and ECHAM5/RegCM3/CAMx arise from the differences between ERA40 and ECHAM5 fields so in other words how close is ECHAM5 to the reanalysis fields. The differences between ERA40/RegCM3/CAMx and ECHAM5/RegCM3/CAMx DO NOT SHOW that our modelling strategy (GCM/RCM/AQM) is inappropriate and inconsistent! It is reasonable that ECHAM5 and ERA40 have differences. These differences are transferred in RegCM3 simulations which in turn determine the meteorology of CAMx. We investigate in our work how sensitive are our CAMx results on different meteorological forcing.

COMMENT 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.

RESPONSE: We used measurements of surface ozone available from the EMEP database to compare with results of model simulations. Figure 1 was changed to

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include both model results and measurements overplotted. A paragraph entitled evaluation of the modeling system was added in the manuscript, including results from calculation of bias and error. The reference Tegoulias et al.., in preparation is not yet available so it was removed from the manuscript. More references were added in the manuscript (Let et al, 2007; Andreani-Aksoyoglu et al., 2008; Lee et al., 2009; Dawson, et al., 2009) to support the suitability of the CAMx model for air quality studies. The validation of the model could be naturally more extended to include more species, skill scores and metrics, however it was considered to be out of scope of the paper, which is already quite lengthy anyway.

COMMENT 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 modelunder either present day or future air quality simulations.

RESPONSE: ECHAM5 is a well established global circulation model used as part of the Coupled Model Intercomparison Project (CMIP) simulations used in the IPCC Forth Assessment Report (AR4). RegCM is a widely used regional climate model applied in a variety of climate studies. The modeling system ECHAM/RegCM has been applied in previous studies with success (eg European project ENSEMBLES). Several publications have been added in the manuscript (paragraph entitled “Methodology-Climatic

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simulations": (Giorgi and Mearns, 1999; Giorgi et al., 2006; Giorgi et al., 1993a, b; Small et al., 1999; Sun et al., 1999; Im et al., 2006; Seth and Rojas,2003; Francisco et al., 2006; Hostetler et al., 1994; Giorgi et al., 1992; Hirakuchi and Giorgi, 1995; Diffenbaugh et al., 2005; Gao et al., 2006; Meleux et al, 2007; Giorgi et al., 2002, 2003; Sanchez-Gomez et al., 2007; Raushcer et al, 2009) supporting the eligibility of the ECHAM/RegCM system in climate simulations. Since we do not introduce a novel modeling system, in contrary, we are based on widely accepted modeling tools in the area of climate change, we consider that references to already published literature are sufficient to justify the selection of the above mentioned modeling tools.

COMMENT 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.

RESPONSE: Differences in biogenic emissions and NOx and their impacts on ozone were put together and discussed in a separate paragraph entitled "Differences in biogenic emissions and NOx". Changes in both ozone precursors seen in this work are exclusively meteorologically dependent and that is why they are included in the paper. Following the comment of the referee, we explain the impact of meteorology on NOx and we further elaborate by assessing the impact of NOx on surface ozone. It is shown that even small changes in NOx affect significantly ozone concentrations and that is an important finding. The fact that meteorological changes affect the inter-cycling of O3 and NOx chemistry is worth to be included in the analysis. So, the discussion on the role of NOx was explained more clearly and seen through the perspective of meteorological change. Abstract and conclusions were modified accordingly. The role of PBL

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height was included in the paragraph entitle “Temperature and PBL height changes”. The variable PBL height was included in the statistical analysis.

COMMENT 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.

RESPONSE: The statistical analysis was performed again taking into account spatial autocorrelation. The methodology is described thoroughly in a new paragraph entitled “Statistical analysis”. Table 1a and 1b is now merged into Table 1, which shows only statistical significant results to the 95% and 99% level for winter and summer. When we include spatial autocorrelation in the statistical analysis we explain 40% of ozone variability in winter and 60% in summer, instead of 60% and 70% respectively when autocorrelation is not included.

We could expect shifting in the response of ozone due the changes in meteorological parameters, however, given that we work on a European scale and we are averaging over a decadal time-slice, we think it is reasonable to make this comparison. The assumption seems to be justified, when looking at table 1 which yields statistical significant correlations for changes in O3 and changes in meteorological parameters.

COMMENT 5: Results for spring and autumn should also be provided.

RESPONSE: Our analysis was carried out for all four normal seasons of the year. In our manuscript we decided to show the results for summer and winter because they represent two opposing seasons, the cold and warm season. Although we totally respect the suggestion of the reviewer for adding results from spring and autumn, we thought not to include them because the revised manuscript is already very lengthy

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and we wanted to avoid having an even more extended article thus trying to keep it in a reasonable size with our main messages. Furthermore the results from spring and autumn seasons, although they have their interest as represent the transitional seasons, they do not add something more to the key messages of the manuscript. However, we can provide them as supplementary material, if the editor decides it is necessary to do so.

SPECIFIC COMMENT 1: 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.

RESPONSE: Details on the calculation of biogenic emission and dependence on meteorology are added to a new paragraph entitled "Emissions" in the Section "Methodology".

SPECIFIC COMMENT 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.

RESPONSE : Lines removed according to the referee's comments.

SPECIFIC COMMENT 3: Page 10,679, lines 7-10. 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?

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RESPONSE: More bibliography is provided to support the work already performed with RegCM. Please see response to Comment Nr 2.

SPECIFIC COMMENT 3: 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.

RESPONSE: The top boundary of CAMx reach up to 6.5 km, which is sufficient when focusing on surface ozone studies. Since the regional chemical model is not coupled to a global chemistry model, it cannot certainly account for transportation of species along the lateral and top boundaries. This is a serious limitation in studies aiming at predicting ozone and certainly affects the quality of results mostly in rural areas which are especially sensitive to boundaries. We need to pinpoint the fact that the aim of this study is the sensitivity of ozone to external meteorological forcing. However, since the question on the impact of boundaries is an important one, we perform two sensitivity studies concerning a 5% change of lateral boundaries and a 20% change of top boundaries. The results are presented in a new paragraph entitled “Sensitivity to chemical boundary conditions”

SPECIFIC COMMENT 4 Page 10,680, line 4 Please provide a reference for the land-use 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.

RESPONSE: The information asked is provided in the manuscript in the paragraph entitled “Emissions”

SPECIFIC COMMENT 5 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

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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.

RESPONSE: The outdated reference was removed. The documentation suggested by the referee, includes the calculation of FGE and MNMB as part of evaluating a modeling system (p 203) indeed without setting a threshold limit. We remove from the revised manuscript the sentence about acceptable thresholds. Indeed, a proper evaluation of a modeling system includes several statistics and skill scores, however, the aim of this scope is not to evaluate a modeling systems which has been so far widely used and published, but to focus on the sensitivity of surface ozone on external meteorological forcing.

SPECIFIC COMMENT 6: Page 10,682, lines 24-26 A correlation of -0.48 is not a minor effect.

RESPONSE: The sentence is rephrased.

SPECIFIC COMMENT 7: 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.

RESPONSE: We use this paragraph to introduce our results for summertime.

SPECIFIC COMMENT 8: Page 10,684, lines 10-11. Which conclusions can be drawn from this finding?

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RESPONSE: The conclusion is that differences in temperature seem to partially explain changes in ozone but it is usually a combination of meteorological parameters that interact with each other and affect ozone. Isolation of each parameter may explain only specific features.

SPECIFIC COMMENT 9: Page 10,687, section 3.5: Please provide differences in domain total isoprene emissions for both runs for both summer and winter.

RESPONSE: It could be provided as supplement material. The paper is already lengthy to add more features.

SPECIFIC COMMENT 10: 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

RESPONSE: When we include spatial autocorrelation in the statistical analysis we explain 40% of ozone variability in winter and 60% in summer, instead of 60% and 70% respectively when autocorrelation is not included. This finding is included in the discussion.

SPECIFIC COMMENT 11: 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.

RESPONSE: ECHAM is a model already established in the area of climate change studies (please refer to IPCC reports) and so does RegCM. Several publications are used as reference to the question of the suitability of the current set up. As every modeling system, has naturally its limitations. With the current study we aim at investigating how sensitive our air quality model is to the current model set up ie ECHAM/RegCM. This work is an added value for the future work concerning the use of the modeling

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system for future climate simulations.

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