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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A multi-model study of impacts of climate change on surface ozone in Europe" *by* J. Langner et al.

Anonymous Referee #1

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Overview

The paper investigates interlinkages between air quality and climate change by focusing on changes in ozone by the year 2050. The main focus regards the identification of the impact of climate on photochemistry. The main strength of the paper lies in the ensemble analysis of 5 different regional models.

The issue of air quality projections has significant policy implications, while offering a variety of scientific and technical challenges. By documenting model spread in regional ozone chemistry under changing climate conditions, this paper contributes to narrow the uncertainties with regards to possible air quality policies.

I support the publication of this study nevertheless the authors might consider relevant to address the following comments.

General comments

While 10 years are modelled for the present and future situation, these 10 years are averaged and only mean difference are discussed in the paper. These 10-years simulation offer a good opportunity to assess the statistical significance of the O3 changes discussed in the paper, hence increasing the robustness of the conclusions. First, some changes discussed in the paper are small, it is important to explain whether these changes exceed the interannual variability observed for the control period before discussion further the implication for climate and air quality interlinkages. Second, it is mentioned that some models appear less sensitive to climate than others, but no quantitative elements are given to estimate their interannual variability. The conclusions regarding the respective sensitivity of the models would benefit from using an appropriate statistical measure of the significance.

Biogenic emissions and boundary conditions constitute two major sources of potential discrepancies in the modelling setup. These factors are well documented in the paper, but these elements are fragmented and a synthesis in a dedicated paragraph would be useful. For example it is mentioned that a set of boundary conditions was provided but, in the description of the models, it appears that a number of exceptions applies. A single paragraph explaining that in a more synthetic way would be useful. Similarly, it is not easy to find out what is being done for biogenic emissions. For example it would be very useful to add monthly isoprene emissions on the seasonal cycle of daily mean O3 for each model (Figure 2). Biogenic emissions are often pointed out as a major driver of projected O3 changes under climate scenarios in Europe. I think a dedicated paragraph in the discussion would be relevant, especially since one of the models involved has zero biogenic emissions. If such a hypothesis could be considered to yield satisfactory results, it would be relevant to highlight it more prominently in the conclusions.

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In this paper, and in previously published works, several processes potentially leading to increases of O3 are mentioned but the investigation of O3 decrease is overlooked. “Reaction with water vapour” is pointed out, for example for the decrease over the Mediterranean modelled with MATCH (P4917L9) but such a statement does not constitute an evidence. No possible explanation is given to explain the decrease over the N-E part of the domain in MATCH and EMEP (P4917L23). The ensemble gathered in the present study constitutes a unique opportunity to isolate underlying processes and more substantive grounds should be sought after.

Specific comments

P4903, para 2: Although the focus of the paper is on O3, the impact of PM on climate should be mentioned in the overview of climate and air quality interlinkages in this introductory paragraph.

P4903, para 3: It is suggested that online coupled models will contribute decreasing the uncertainties in the projections. While these models will certainly offer a more satisfactory representation of the processes involved, it is anticipatory to suggest that uncertainties will be reduced

P4905L26: in addition to the estimate of the difference in temperature between 2000 and 2040, the absolute bias (if any) of the climate control simulation for the present day should be given.

P4906L12: Is the landuse identical for all models? Difference landuses would presumably influence biogenic emissions.

P4907L10: Since aerosols are not addressed in the paper, and excluded from the inventory of anthropogenic emission, it is not clear why secondary inorganic aerosols are included the boundary conditions.

P4908L5: Using annual mean values in the boundary conditions for DEHM seems contradictory with the monthly values mentioned in Section 2.3. Would it be possible

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to confirm this apparent contradiction?

P4909L16-18: The similarity of DMI-EnvClimA and Enviro-Hirlam are explicated, but not the differences. It would be interesting to explain briefly what makes EnvClimA more appropriate for climate studies.

P4909L10-24: The added value of EnvClimA compared to RegCM should be more explicit. At this stage, I understand that the dynamical core is that of RegCM, while chemistry/aerosol processes and feedback with the dynamic are modified. If correct, could this be stated more clearly in the text?

P4914L20: The low bias of O3 of EnvClimA in Winter is attributed to the feedback of O3 on climate while no evidence is given to support this statement. It is not clear how O3 will influence regional climate in winter. This item should be discussed more in depth pointing towards specific underlying processes and giving quantitative evidence.

P4915 para 3: EMEP is the only model to exhibit a local minimum in June-July that is also seen in the observations. It would be useful to discuss further this feature and explain why the other models fail to capture it. In order to provide a quantitative support to the discussion of this paragraph, the authors could consider adding a correlation coefficient computed from the monthly time series to Table 3.

P4918L11: why is the 95th percentile of hourly O3 chosen while there are alternative indicators that make a consensus in terms of impacts of O3 on ecosystems and human health (AOT, SOMO)?

P4919L1-8: The first paragraph of the discussion is largely irrelevant since only model projection using similar forcing (scenario and target year) should be compared.

P4920L15: The sensitivity of temperature to model resolution, and, in turn, the impact on biogenic emission is not supported by quantitative grounds in the paper and should therefore not appear as one of the findings of the study. Presumably, the underlying biogenic emission model can also play an important role here.

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Minor comments

P4902L23: PM10 should be defined

P4909L14: Is it possible to define what is meant by “aerosol-chemistry-dynamics” modules?

P4915L16: Please add a line with the quadrants on one of the maps.

P4914L18: The winter biases for daily maximum O_3 is not given.

Figure 3&4: It would be more informative to plot the average of the O_3 record at the stations rather than plotting all of them with the same colour.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4901, 2012.

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