Atmos. Chem. Phys. Discuss., 12, C4523–C4532, 2012
www.atmos-chem-phys-discuss.net/12/C4523/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "European summer surface ozone 1990–2100" *by* J. Langner et al.

J. Langner et al.

joakim.langner@smhi.se

Received and published: 10 July 2012

We are very grateful for the valuable comments and questions posed by Anonymous Referee #2 that we address below.

General comment:

The paper describes a modeling study which goal is to diagnose the relative contribution of climate change and changing emission precursors on future European summer surface ozone. This is a relevant topic as ozone is a key compound for air quality and climate. The paper is relatively well written and interesting but additional and more detailed discussions about the model results and the limitations of the study are missing in the current version of the paper (as further detailed in the following), which prevents publication at that stage in my opinion. The Authors should also provide more detailed information about the extent to which the results presented in this paper are different from existing work dealing with a similar topic.

Response - The general comment regarding limitations of the study and request for more details are addressed in connection with the specific comments below.

Specific comments:

Abstract: Please define what is a “sufficiently long period”. Indicate the main limitations of the study (see also details below), and also what is really new in the paper.

Response - By a “sufficiently long period” we mean in this context a time period that is sufficiently long to enable detection of statistically significant changes in the model results.

In our view what is new and original with the paper is that the study tries to simultaneously assess the different contributions from regional climate changes, precursor emission changes and changes in hemispheric background for summer surface ozone in Europe. In addition long transient model runs are performed to demonstrate the importance of decadal variability in meteorology for the simulated changes. Although these variations may not look very large due to the averaging applied they are still substantial and we are not aware that similar results have been presented before. Main limitations of the study are discussed in connection to the comment on the conclusions of the paper further down in this response.

Section 2.1, page 7708, lines 6-7: The Authors report that the evaluation of both current and future climate simulations were discussed in two papers. Could they briefly summarize the findings of these papers and in particular the known deficiencies (if any) of the model? They should also provide a short but quantitative description of how future climate look like in their simulations.

Response - The simulation of past climate using ERA40 as forcing on the boundaries show improvements in the climate simulated by RCA3 compared to previous versions of the model. Primary model biases that remain are underestimates of the diurnal temperature range over northern Europe in summer and overestimate of precipitation and surface evaporation in northern Europe in the summer (Samuelsson et al. 2011). When using GCM input to drive the simulations for the control period (1961-1990) the HadCM3 shows lowest bias compared to observations while the simulation using ECHAM5 is generally too cold and too wet in northern Europe in summer (Kjellström et al. 2011).

Both climate projections used show an increase in summer precipitation in large parts of northern Europe extending also to south-east over Poland and Belarus between the periods 1990-2009 and 2040-2959 while precipitation generally decrease in southern and south-western Europe. Increase in precipitation in this period is stronger in the downscaling of HadCM3, more than 10% in large areas, than in ECHAM5, 0-10%. Connected to this is also a summer total cloudiness increase in northern Europe and decrease in southern Europe with the same spatial pattern as for precipitation.

Section 2.1, page 7708, line 8. The GCM simulations were performed with an emission scenario that differs from the emission scenario used for air pollutants in the regional chemistry transport model. There are good reasons for this, however the Authors should provide more details about these different scenarios and actually discuss to what extent those different scenarios are consistent (or not) in terms of world development. In particular, what are the assumptions about air quality regulations that are used in these scenarios? Otherwise this section just looks a bit like "a scenario acronym soup".

Response - Global emissions of CO2 and methane for 2050 are 45 and 37% larger respectively in SRES A1B than in the RCP4.5 emission scenario. Global emissions of NOx and NMVOC for 2050 are 65 and 46% larger respectively in SRES A1B than in RCP4.5. SRES A1B therefore corresponds to a world with substantially larger emissions of both long-lived greenhouse gases and air pollutants. For the climate change signal in the climate projections this is not so important for the first half of the 21st century where already committed emissions of long-lived greenhouse gases dominate the

ACPD
12, C4523–C4532, 2012

radiative forcing while towards the end of the century the climate change signal under the RCP4.5 scenario would be expected to be smaller than for SRES A1B. For Europe emission reductions of air pollutants in the RCP4.5 until 2020 are comparable to e.g. the PRIMES base line scenario by IIASA for NOx but smaller for VOC (Amann et al. 2010).

Section 2.3, page 7710, lines 1-2: Are emissions changing homogeneously throughout Europe?

Response - Emissions are not changing homogeneously over Europe in the RCP4.5 scenario. Looking specifically at the changes between 2000 and 2020 we find that emissions decrease by 50-60% for NOx in e.g. Germany, Italy, France, Spain and UK, while emissions decrease by more than 80% in Russia and Ukraine.

Section 2.4, page 7710, lines 15-16: Could a reference be provided for the statement regarding the stabilization of the increase in ozone background? This point should be discussed in a more quantitative manner to provide justifications for the values chosen afterwards for the increasing ozone background in the simulations.

Response - Isaksen et al. (2010) is a good reference for this statement. We will include it in a revised version of the manuscript.

Section 2.4: I assume that the Authors mean "lateral boundary conditions" by boundary conditions? What are the assumptions made at the model top?

Response - The treatment of chemical boundary conditions, both lateral and top, are described in detail in Andersson et al. (2007). For ozone, lateral boundary conditions are based on back-trajectory analyzed measurement data from the EMEP network for year 1999 and for the top boundary on ozone sonde data from Ireland, UK and Norway for the period 1996-2001.

Section 2.4, page 7710, lines 22-25: Is this consistent to consider a steady increase in ozone background at the border of the domain throughout the entire period and at

ACPD

12, C4523–C4532, 2012

the same time to consider an increase followed by a decrease in methane concentrations? Part of the change in background ozone could be due to a changing methane concentration.

Response - We agree that it may not be consistent to assume linearly increasing background ozone concentrations at the same time as methane concentrations are decreasing. However, we do not fully understand all the processes driving the background ozone concentration. Rather than trying to "model" future background ozone based only on methane we choose to use a simple increasing boundary case to demonstrate the potential importance of future increasing ozone concentrations. We state clearly in the paper that the lack of a link to a global CTM for providing background ozone boundary conditions is a limitation in the study.

Section 2.5, page 7711, lines 11-22: Please be quantitative when you say "Evaluation of MATCH driven by meteorology data constrained by observations shows better correlation". What "better" means here? How do the results differ between the simulations driven by the different GCMs? The Authors say "Emission data also impacts the results". That is certainly true but not particularly insightful. Could they provide any quantitative statement with respect to the validity of the emission inventory they used in that study in comparison to some emissions they have used previously? Also about lines 19-22: I would think that a too cold and wet climate could induce a bias in the simulated ozone but maybe not such a low correlation. Again, how does that look like in the HadCM3-driven simulation (assuming that this model does not suffer from such bias)?

Response - Evaluation of a 7-year simulation with MATCH for the period 1997-2003 using ERA40 as meteorological input has been presented by Andersson et al. (2009). The correlation for the whole domain was in this case 0.61 for mean values and 0.87 for daily max values which are better than what we obtained in the present study which are 0.55 and 0.71.

ACPD
12, C4523–C4532, 2012

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

The results from the statistical evaluation of the model simulations using the two different climate projections differ somewhat. The simulation using ECHAM5 shows higher correlation than the simulation using HadCM3 while the bias is smaller using HadCM3 compared to ECHAM5. Numbers for the simulation using HadCM3 will be added in a revised version of the paper.

Section 3, page 77121, lines 18-19: Which quantities exactly are changing in the climate change cases? How are BVOC (isoprene) emissions changing? How are cloud cover changing and what are the implications on the photolysis rates for example (if any)? How are dry deposition velocities changing throughout the domain? In my opinion, addition analyses of the simulations are really needed so that the paper includes a substantial added value upon previous papers addressing a similar topic.

Response - In the paper we state that the main drivers for changing surface ozone in a future climate are changing isoprene emissions and changing dry deposition of ozone. This has been analyzed and discussed in detail by Andersson and Engardt (2010) using the same model and a similar model setup and we don't think it is useful to repeat that analysis here. In the response to the next comment we expand on the causes for decreasing ozone concentrations in northern Europe in the simulations and this discussion should be included in a revised version of the paper.

Section 3, page 7712, line 18 until page 7713, line 10: Why does a changing climate result in decreasing surface ozone in Northern Europe and an increase in Southern Europe?

Response - The most important factors contributing to increase in surface ozone in southern Europe are given in the paper and include increased biogenic emissions and reduced dry deposition due to dryer conditions. The reasons for reduced concentrations in northern Europe have not been disentangeled in detail yet but both cloudiness and precipitation increase in northern Europe in the climate projections used leading to increased scavenging of ozone precursors and less solar radiation for driving the

ACPD

12, C4523–C4532, 2012

Interactive Comment

photochemistry.

Section 3, page 7714, lines 6-14: Do the distributions change in the climate-only changing simulations?

Response - When considering only climate change (keeping emissions and lateral boundaries at their ca. 2000 values), the trends in the frequency distributions are quite different. See Fig 1. The peaks of the distributions remain unchanged in both Northern and Southern Europe. For southern Europe, the high end tail of the distribution is, however clearly increased during the period 2040-2059 compared to 1990-2009. This feature is also seen in northern Europe, but to a lesser extent. The implication is that climate change increases the peak values of surface ozone.

Section 3, page 7715, lines 12-29: What do the Authors mean by "Under a SRES A2 scenario surface O3 concentrations in 2030 could increase by 4–6 ppbv around Europe, in line with our Increasing boundary case, and would then continue to increase until 2100 (Prather et al., 2003)." How does the SRES scenario compare with the scenario used in this study? It is not too interesting to compare changes in future surface summer ozone if the assumptions underlying the emission scenario are different.

Response - We agree with the referee regarding this part of the paper and suggest deleting these lines in a revised manuscript.

Conclusion: The authors say "A drawback with this model setup is that assumptions have to be made about trends in the concentrations of chemical components on the model boundaries." In my opinion, there are many additional drawbacks that are not discussed. For example, to what extent future changes in the stratosphere-troposphere exchange may affect future surface ozone? In general in the conclusion, the Authors should clearly state: - what is new/original in their paper in terms of future summer surface ozone and the respective role of changing climate versus changing emissions? - what are the main limitations of their study? Are there any missing processes/feedbacks in their model that would affect their results?

ACPD
12, C4523–C4532, 2012

Response - We agree that the study has additional limitations such as neglect of changes in stratosphere-troposphere exchange. Additional potentially important missing processes/feedbacks include change in vegetation/land use, change in wild fire emissions. We also note that previous evaluations of the MATCH model for surface ozone has indicated problems in simulating the highest observed ozone concentrations. A statement along these lines will be added in a revised manuscript.

In our view what is new and original with the paper is that the study tries to simultaneously assess the different contributions from regional climate changes, precursor emission changes and changes in hemispheric background for summer surface ozone in Europe. In addition long transient model runs are performed to demonstrate the importance of decadal variability in meteorology for the simulated changes in surface ozone. Although these variations may not look very large due to the averaging applied they are still substantial and we are not aware that similar results have been presented before.

In addition, I think it is not correct to state that "the MATCH CTM simulations using climate model output are able to capture major features of the observed distribution of surface ozone over Europe" when the correlation is below 0.1 in at least one case.

Please rephrase.

Response - This will be rephrased in a revised version of the manuscript.

References

Amann, M., B. Imrich, J. Cofala, C. Heyes, Z. Klimont, P. Rafaj, W. Schöpp, F. Wagner, 2010. Scope for further environmental improvements in 2020 beyond the baseline projections. Background paper for the 47th Session of the Working Group on Strategies and Review of the Convention on Long-range Transboundary Air Pollution Geneva, August 30 – September 3 2010 Centre for Integrated Assessment Modelling (CIAM) International Institute for Applied Systems Analysis (IIASA) CIAM Report 1/2010.

Andersson, C., Bergström, R. and Johansson, C. "Population exposure and mortality due to regional background PM in Europe – long-term simulations of source-region and shipping contributions." Atmospheric Environment 43, 3614-3620, 2009. Andersson, C., and Engardt, M.: European ozone in a future climate – The importance of changes in dry deposition and isoprene emissions, J. Geophys. Res., 115, D02303, doi:10.1029/2008JD011690, 2010.

Andersson, C., Langner, J., and Bergström, R.: Interannual variation and trends in air pollution over Europe due to climate variability during 1958-2001 simulated with a regional CTM coupled to the ERA40 reanalysis, Tellus B, 59, 77-98, 2007.

Isaksen, I.S.A., Granier, C., Myhre, G., Berntsen, T.K., Dalsøren, S.B., Gauss, M., Klimont, Z., Benestad, R., Bousquet, P., Collins, W., Cox, T., Eyring, V., Fowler, D., Fuzzi, S., Jöckel, P., Laj, P., Lohmann, U., Maione, M., Monks, P., Prevot, A.S.H., Raes, F., Richter, A., Rognerud, B., Schulz, M., Shindell, D., Stevenson, D.S., Storelvmo, T., Wang, W.-C., van Weele, M., Wild, M., and Wuebbles, D.: Atmospheric Composition Change: Climate-Chemistry interactions, Atmos. Environ., 43, 5138-5192, 2009.

Kjellström, E., Nikulin, G., Hansson, U., Strandberg, G., and Ullerstig, A.: 21st century changes in the European climate: uncertainties derived from an ensemble of regional climate model simulations, Tellus 63A, 24–40, doi:10.1111/j.1600-0870.2010.00475.x, 2011.

Samuelsson, P., Jones, C. G. Willén, U. Ullerstig, A. Gollvik, S. Hansson, U. Jansson, C. Kjellström, E. Nikulin G., and Wyser, K.: The Rossby Centre Regional Climate model RCA3: Model description and performance, Tellus A, 63, 4-23, doi:10.1111/j.1600-0870.2010.00478.x, 2011.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7705, 2012.

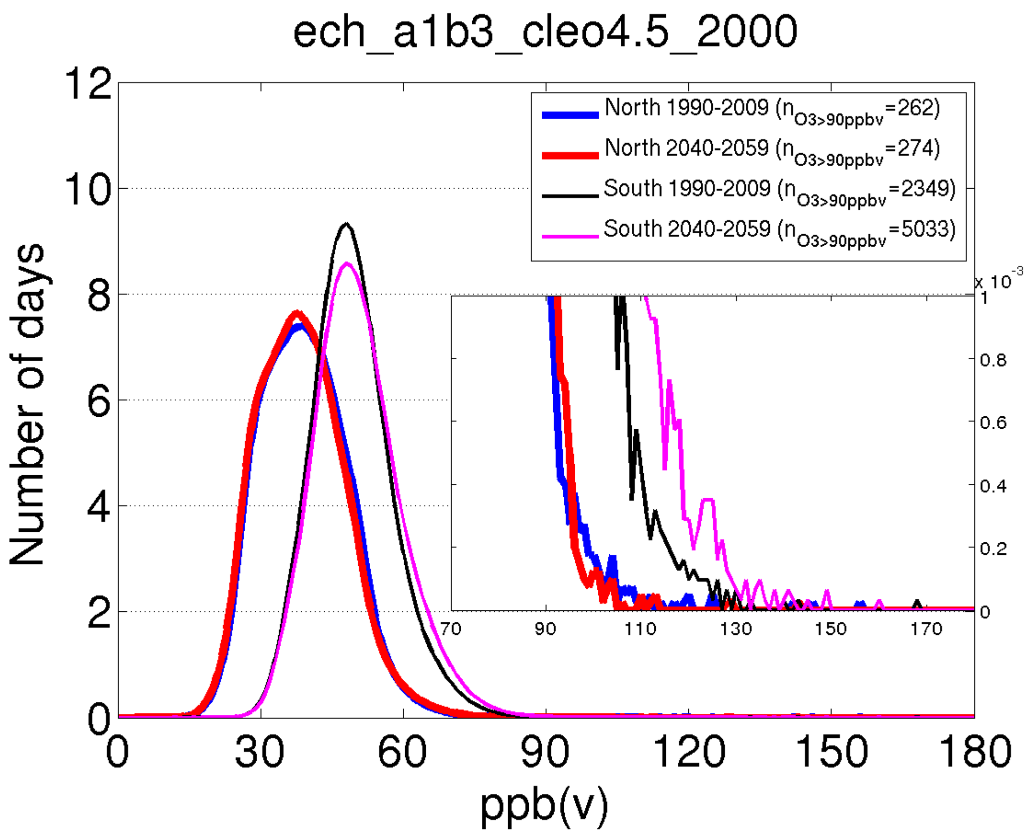

Fig. 1. Frequency distributions of daily max O3 concentrations in North and South Europe in the climate sensitivity simulation with meteorological data downscaled from the ECHAM5 global model.

ACPD
12, C4523–C4532, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper