

Interactive comment on “The impact of differences in large-scale circulation output from climate models on the regional modeling of ozone and PM” by A. M. M. Manders et al.

Anonymous Referee #2

Received and published: 9 July 2012

The paper “The impact of differences in large-scale circulation output from climate models on the regional modeling of ozone and PM” by A. M. M. Manders et al. aims to compare results from two regional air quality-climate simulations with RACMO2-LOTOS-EUROS driven by ECHAM5 and MIROC with a RACMO2-LOTOS-EUROS simulation forced by ERA-Interim. The results indicate that GCM-driven simulations showed considerable deviations from RLE-ERA in several meteorological magnitudes affecting future air quality.

The paper addresses an interesting and sounding topic related to climate impacts on air quality and therefore it is worth publishing in Atmospheric Chemistry and Physics.

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The figures and tables in the manuscript are also relevant, but the quality of some figures should be improved before final publication.

Although the paper is well organised and detailed, there are some aspects of the paper that require revision before publication. My main objections for the publication of the paper in its present form are detailed below.

Major comments:

1. My main concern about the manuscript is related to the validation of the simulations. The RLE_ERA simulation is taken as reference but the validation of these simulations is very poor. Only six points are taken as comparison, unevenly distributed over Europe. But the key point here is: can we trust the ERA-driven simulations? What are the biases, the correlation coefficients, the errors, etc. . . of the RLE-ERA simulation against observations? Is RLE-ERA correctly reproducing the climatologies? Moreover, can we trust the concentrations of pollutants simulated by RACMO2-LOTOS-EUROS simulation forced by ERA-Interim? How do the air pollution fields reproduce the air quality climatologies? It is the basis to have confidence on the rest of the work. The authors should elaborate on this before the paper can be accepted for publication.

2. In section 2, something must be said about resolution. The resolution used (0.44) by RACMO2 is then interpolated to a 0.5x0.25° regular grid in LOTOS-EUROS. How is this interpolation performed? Also, it seems too coarse to reproduce several patterns at a more local scale. I agree that it is enough to capture the general behaviour of air pollution climatologies at a regional scale, but then the authors discuss about features at cities like Madrid or Paris, which cannot be comprised at this resolution (as stated in the supplementary material).

3. In section 3, some insights are expected (e.g. why is more precipitation simulated in RLE_ECHAM and RLE_MIROC than RLE_ERA? Why are RLE_ECHAM simulations colder than ERA-Interim driven simulations?)-

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4. In the statement at Page 12260, secondary organic aerosol is pointed out as one of the most important components of PM over southern Europe. However, the authors are neglecting their contribution over northern Europe and specially over Western Europe. This should be reelaborated in the corrected version of the manuscript.

5. The discussion in Page 12263 about the chemical regimes governing ozone formation on the Netherlands and the Iberian Peninsula is somehow arguable. Over the entire Europe, the ozone regimes are extremely complicated, with strong transitions between VOC-limited and NO_x-limited regions. It is obvious than over large cities (like Madrid or Barcelona, in the Iberian Peninsula or elsewhere) the NO_x titration dominates the destruction of ozone over the city, but what happens in downwind areas, where O₃ is formed because and the maximum ozone concentrations are measured (and simulated). More references on this very topic should be provided (or indicate it is just an hypothesis). The authors should be extremely careful in this discussion, since I would dare to say that their discussion is not very exact in this very aspect.

6. Conclusions are not very conclusive. I would expect a deeper analysis of the implications of this study for the state of the art and not a summary of what I've read before in the manuscript. The authors should elaborate on this a little bit more.

7. Also, it is somehow confusing that, in the last section (Discussion and Conclusions) the authors refer to "biases" in RLE_ECHAM and RLE_MIROC simulations with respect to RLE_ERA, and talk about "overestimation" and "underestimation" throughout the entire document. I would expect these biases to refer to compare to observations. How do authors know that RLE_ERA simulations are closer to reality, so you can talk about "overestimation" or "underestimation"? Authors should be very careful about that, and they could only use these terms after a complete evaluation of the results is performed (see my comment 1).

With respect to other minor aspects:

1. Page 12246, Line 17-19: Please rephrase for the sake of clarity.

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2. Page 12247, Line 16: Not only warmer conditions are expected, but also important variations on other climatological fields conditioning air pollution.

3. Page 12248, Line 10-11: Some references are needed here.

4. Pag. 12255., Line 5 (and elsewhere): Please harmonize the reference to the RLE_ECHAM simulation throughout all the manuscript. Are you naming it RLE_ECHAM5 or RLE_ECHAM? Please be consistent.

5. Page 12260, Line 19-20: Please rephrase for the sake of clarity.

6. Page 12261, Line 22: Replace "amount of increase" by "increase".

7. Page 12261, Line 28: Replace "nex section" by "next section".

8. Page 12263, Line 12: Replace "The reason is thats" by "The reason is that".

9. Page 12266, Line 23: Replace "fot" by "for".

10. Page 12268, Line 14: Replace "between of meteorology on anthropogenic emissions" by "between meteorology and anthropogenic emissions".

11. Please add the units to the captions of all figures (e.g. Fig 7 or Fig 8).

12. Other comments are related with the writing of the manuscript. A revision of minor aspects related to English should be addressed by a native speaker.

Because of all of the above comments, I recommend this manuscript to be published in ACP when all the aforementioned modifications are taken into account.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12245, 2012.

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