Referee#2

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.

The present work presents the differences of two runs using meteorology from two global climate models, set against a reference simulation based on reanalysis meteorology. This paper does not intend to give a full assessment of the performace of RACMO or LOTOS-EUROS, as they can be found elsewhere. RACMO2 has participated in PRUDENCE and ESSENCE model intercomparison studies. The model was performing comparable to other participating models and was not one of the extremes of the ensemble. By driving it with ERA-interim, it will add some small-scale features but we think that, within the scope of this paper, using RACMO-ERA as a reference is justified. LOTOS-EUROS is an operational air quality model, and has participated in several model intercomparison studies showing good performance, albeit with underestimations of the highest ozone concentrations and a significant underestimation of PM10 concentrations. This underestimation is a feature of nearly all air quality models, due to uncertainties in emissions and secondary organic aerosol formation, as stated in the text. The comparison of variability of concentrations with meteorology is for example done in Manders et al 2009 and Mues et al 2012, and is not the subject of this study. There are clear shortcomings with respect to observations, but not worse than other models. Therefore we think that the set-up of the study is sound. We do not intend to give a final answer but demonstrate the impact of uncertainties. These points are stated explicitly in the introduction to clarify the scope of the article.

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?

Horizontal projection of RACMO fields on the LOTOS-EUROS grid is carried out by bi-linear interpolation. Vertical projection of RACMO profiles on the – much coarser -LOTOS-EUROS vertical grid is achieved by mass-weighted averaging of those RACMO model layers that are contained – fully or partially – in each of the LOTOS-EUROS model layers.

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).

The meteorology at a very local scale (city-scale) is indeed not resolved and we do not pretend to give a detailed air quality analysis for these cities. Nevertheless, the cities are interesting areas to study since they are often strong emission sources, and elevated concentrations are indeed modeled even at the relatively coarse scale of 25 km. Therefore, a few of these cities are taken into account in the analysis of the results. We have changed Barcelona into ElsTorms, an EMEP station somewhat further inland, which is used in model assessment studies on a regular base (e.g. Mues et al 2012). Still, we show Madrid and Vredepeel since they show the most extreme behavior. The arguments to present city results are explicitly stated in the text now.

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?)-

RLE_ECHAM and RLE_MIROC are free-running climate models that are not constrained by observations, in contrast to the ERA-Interim reanalysis which represents the observed meteorology as closely as possible. Each global model has, by construction, its own dynamical and physical behavior, depending on which parameterizations and schemes are used. It is exactly the impact of these differences that we want

to illustrate: it matters which global model you take to perform your air quality simulation. The question why one global model shows different circulation patterns than another in a certain area goes beyond the purpose of this study. To illustrate the amplitudes of the difference, results were set against ERA-Interim-driven simulations, which provide a state-of-the-art representation of the observed circulation of the past decades.

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.
The reference to southern Europe is now only made for dust, for SOA references to the literature were added (Gelencser et al 2007, Bergström et al 2012).

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 NOx-limited regions. It is obvious than over large cities (like Madrid or Barcelona, in the Iberian Peninsula or elsewhere) the NOx 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.

In the text it is now made clear that the statements are interpretations and not exact

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.

The beginning of the section is rewritten slightly and some more conclusive sentences have been added to the end of the paragraphs.

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).

It is far beyond the scope of the paper to evaluate LOTOS-EUROS with RACMO using ERA-Interim forcing. ERA-Interim is evaluated elsewhere and was found to represent the observed meteorology for Europe well. RACMO and LOTOS-EUROS are well-validated models that we can use with confidence for the present purpose, as also followed from a small intercomparison of RACMO with ERA-interim forcing with ECMWF analysis that we did ourselves. Furthermore, the results of this study are an illustration of the impact of differences resulting from the choice of the global circulation model for climate simulations, not a quantification of the impact of climate change on air quality.This point is now made more clearly in the introduction.

With respect to other minor aspects:
1. Page 12246, Line 17-19: Please rephrase for the sake of clarity.
Changed
C4445

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

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

References added to articles that were also referred to in the discussion section

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.
Adapted

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”.
Textual corrections were made

11. Please add the units to the captions of all figures (e.g. Fig 7 or Fig 8).
Units added to the figures, captions were improved.

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.
The text was carefully checked again.

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.