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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future air quality in Europe: a multi-model assessment of projected exposure to ozone" *by* A. Colette et al.

A. Colette et al.

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Reviewer 2:

P 14777, L25. ".. we can use present-day meteorological conditions to drive the CTMs". Could you please clarify? To my understanding, the decade of 2030s was simulated in the current work, using the two GEA emission scenarios (reference and sustainable) projected for the 2030s, but what about the meteorology? If not a relevant time slice (2030s) from a downscaled GCM was used for all models, which present day conditions were used? There is not so direct information in the text, therefore, you necessarily, need to elaborate a lot more on the meteorology used for the simulation and put forward a well argued case to justify your choice to use meteorology of different time-slices – if this is the case.

Please, make sure you consider the following points: The Ozone chemistry and thus the calculated exposure metrics are sensitive to the external meteorological forcing, therefore it would be nice to know about the range of variation of meteorology that forces the ensemble models. Preferably, a paragraph entitled meteorology, should be added in the current manuscript, to describe the meteorology used. You mention that "The 2030 time period has also the advantage for air quality modeling that the climate signal is relatively weak, so that we can use present-day meteorological conditions to drive the CTMs". This is true, however, there is a climate change signal, in some cases appearing to be statistically significant, and it does definitely affect surface air quality, even to a small extent. This direct impact of climate change on air quality is lost, if not a relevant meteorological time-slice is used (2030s). The latter is acceptable, however, it must be pointed out that you investigate the impact of only future emission changes on air quality. This could be an addition to the already published work investigating solely the climate change impact on air quality, leaving emissions unchanged. You could discuss findings of both studies and comment on their findings, which signal is the stronger, whether they mask or add to each other depending on the region.

Authors answer:

In the present work, we use exclusively meteorological fields representative of present conditions. The statement P14777L25 was meant to support this choice. The rationale is: given that the magnitude of the climate signal is relatively limited at the 2030 horizon, we can use meteorological fields of the early 21st century. In the submitted version, brief details on the choice of meteorological fields were provided later in Section 3.1. As mentioned in the revised version, for 5 out of the 6 models in the ensemble, the meteorological forcing are identical to Colette et al. (2011), the remaining model being forcing by a historical simulation of a free evolving climate model. Since we did not attempt to include a variety of meteorological forcing, we did not consider relevant to perform a dedicated analysis of meteorological variability. Similarly we did not investigate the relative importance of climate and anthropogenic forcing and left it for future

work as mentioned in the perspective P14796L7 of the ACPD version. Therefore we referred to recently published work (Langner et al. 2012, Katragkou et al, 2011) to support the statement on the anticipated limited climate signal.

We apologize if the formulation of the submitted version was misleading. The revised version includes a cross reference to Section 3.1 in Section 2 and further details on meteorological fields used in the ensemble: **"All the groups simulated 10 meteorological years corresponding to the early 21st century for each of the three emission scenarios described above. Five models used the same reanalyses of historical years (downscaled with a mesoscale model for the regional tools) as in the C2011 paper, and the remaining model used downscaled control climate simulations representative of the early 21st century."**

Reviewer 2:

It would be nice to even mention shortly the chemical boundaries used in the models and their variability, so that the reader has a direct access to information.

Authors answer:

We added this information in Section 3.1: **"The boundary conditions for the regional models are identical to C2011 and therefore also representative of early 21st century (LMDzINCA fields for CHIMERE and BOLCHEM and observation-based O3 climatology for EMEP and EURAD)."**

Reviewer 2:

P 14781, L20-22. The evaluation performed in the CS2011 is certainly indicative of the robustness of the modeling systems used, however, the current modeling set up is somewhat different. Therefore, it is strongly suggested that you add some more material with respect to evaluation. This could be a table with simple but representative statistics of model performance e.g. error, bias, correlation coefficient. Since model resolution is 0.5 to 0.22 only rural stations could be used for the evaluation metrics.

Authors answer:

A table presenting the scores of the models at Airbase stations was added to the paper as well as the following text: **"The model performances are further documented in Table 2 that provides a comparison between each model and observed values reported at AIRBASE stations (the public air quality database maintained by the European Environmental Agency http://air-climate.eionet.europa.eu/databases/AIRBASE/). The mean bias, root mean square error and correlation of the daily maximum ozone over the June-July-August months are provided for all models reporting hourly data (BOLCHEM, CHIMERE, EMEP, EURAD, and OsloCTM2). For EMEP only the mean bias is given: since the CTM relies on meteorological fields from a climate free-run for this experiment, there was no scope for a synchronous comparison with observations. The low bias of BOLCHEM mentioned before appears on the median score as well as the high bias of CHIMERE, only compensated by a high correlation to achieve an average root mean square error. Whereas EMEP reported a similar behaviour than CHIMERE in the C2011 study, it exhibits here a negative bias attributed to the different choice of meteorological forcing."**

Reviewer 2:

P14784, L3. Differences in O3 concentrations can be also seen over the Mediterranean.How do you account for that?

Authors answer:

Given that we use identical meteorological forcing, this discrepancy over a remote area is attributed to differences in the total mass of precursor exposed to long range transport and leading to the build-up of ozone over the Eastern part of the Mediterranean documented e.g. in the review of Kanakidou et al., AE, 2011: "Megacities as hotspots of air pollution in the East Mediterranean". A sentence has been added to include this discussion and reference: **"Important differences are also found over the Mediter-**

ranean, despite the use of identical meteorological forcing ruling out possible changes in incoming solar radiation or deposition fluxes. These differences in remote areas are thus also attributed to differences in the total mass of precursor over Western Europe that builds up as ozone after having undergone long range transport over the sea (Kanakidou et al., 2011).”

Reviewer 2:

P14784, L20. What exactly is meant by “marginal”? The increase of O3 in the Benelux area for the sustainable scenario seems to be more than 5 ppb, which is a lot.

Authors answer:

We removed this sentence that was wrong in its English formulation (around was originally meant to refer to the outskirts of the Benelux region) and not very informative.

Reviewer 2:

P14787, L6. Could you explain how this choice has been made? Based on previous experience, or that simply gives best results? (Referring to the choice of a different EMEP model level than the surface, if I get that correctly).

Authors answer:

The first layer of the EMEP model is centred at 45m above ground level, which is high to estimate exposure of vegetation and population. A specific downscaling methodology assuming constant yet landuse-dependant vertical deposition flux density for O3 was thus derived for the EMEP model in order to better estimate near-surface levels. This technique is described in Simpson et al. (2012): **“[...] except for the EMEP model for which a downscaled 3m concentration was provided because of the thickness of the first model layer (with centre at ca. 45m), the downscaling methodology is described in (Simpson et al., 2012).“**

Reviewer 2:

14792, L6. Does ‘significantly’ mean that you perform a statistical test? If not, better not using this word.

Authors answer:

The word “significantly” has been removed since no statistical test has been performed here.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14771, 2012.