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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.

Anonymous Referee #1

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Colette et al. use an ensemble of models to explore the implications for European ozone pollution of two 2030 emissions scenarios: one in which emissions are projected to decrease in line with expected changes in air quality legislation (a "reference" scenario); and another in which additional decreases in ozone precursor emissions result from measures designed to decrease greenhouse gas emissions (a "sustainable" scenario). Decreases in emissions of ozone precursor species result in widespread decreases in ozone over the European domain, consistent with both historical experience of emissions control measures, and other modelling- based future projections. Similarly, Colette et al. also note an increase of ozone over the Benelux and English regions due to these regions becoming less saturated in NOx. Again, this is consistent with historical observations and the existing modelling literature.

Various ozone exposure metrics are calculated from the ensemble output, and in all cases an improvement is expected by 2030. An additional reduction in ozone exposure relative to the reference scenario is expected in the sustainable scenario, which is to be expected given the stronger reduction of ozone precursor emissions under this scenario. Analysis of the spread of results within the model ensemble lends weight to these conclusions.

An interesting feature of Colette et al. is the use of a probabilistic downscaling methodology (CDF-t) for the correction of biases in the model simulations hourly ozone concentration at monitoring stations. Corrections of the model biases based on comparison of historical model simulations with observed ozone concentrations are used to modify the projected ozone concentrations for 2030. Unfortunately the manuscript lacks any justification for the choice of method used, and has no discussion of its applicability to regional ozone modelling. The reference given for the CDF-t method (Michelangeli et al. 2009) describes its development and application for climate variables such as wind speed, but there is no indication given, either by Michelangeli et al. (2009) or by Colette et al. that this method is applicable for correcting model ozone biases.

For example, how well does the probabilistic downscaling method cope with the presence of a step change in the ozone photochemical regime? It is well known that ozone photochemistry can exist in two different states: NOx saturated (also known as VOC limited); or NOx limited. If a photochemical model incorrectly simulates NOx limited chemistry when reality is actually NOx saturated, then the model biases are potentially quite different than if the same model were to correctly simulate the chemical regime. If, as a result of changes in emissions under future scenarios, the simulated chemical regime also changes, it is not clear to me that the bias correction calculated based on comparison with historical measurements is still applicable. It is asserted on P14789, L19-23 that the bias correction uses the change between the control and projected distributions to scale the bias correction in the future, but I do not see how this applies to photochemical regimes. This potential issue is especially important given the transition of large parts of the European domain from NOx saturated to NOx limited, as the authors themselves note.

Before this manuscript can be published in ACP the authors must clearly show how this probabilistic downscaling methodology is applicable to their ozone simulations, or at least provide a discussion of its potential limitations.

As well as the above major criticism, there are also a couple of other minor issues which should be addressed in a revised manuscript.

In Section 2 it is described how the emissions decreases from the present to the future are spatially distributed according to exposure. Isn't this exposure-driven distribution algorithm only applicable for the reference scenario, where the effects of air quality legislation are being accounted for? How are the additional emission reductions in the sustainable scenario spatially distributed? And how do these assumptions affect the results of the study?

In Section 3.1 it would be useful to briefly describe the chemical boundary conditions used for the regional models, as this is potentially directly relevant for the simulation of background ozone.

On P14785 L10-14, please explain the reasoning behind this conclusion.

In Table 3 it would be useful to include the exposure indicators as calculated from the station data. This would be useful for comparison with the model-calculated values. A few words in the text describing such a comparison could also be included.

Despite the above criticisms, I find the manuscript to be generally well laid out, with appropriate used of figures and tables. The text is also quite clearly written, despite the presence of a number of grammatical errors, a sample of which is included here.

At various places in the text: "consists in" should be "consists of".

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Interactive Comment

P14778 L19: should be "same ensemble of CTMs *as* the present paper".

P14782 L6: should be "...this local *minimum* is captured...".

P14783 L7: "21th" should be "21st".

P14789 L2: "documenting" should be "documentation of".

The author list of this paper includes at least one native English speaker. I recommend that the manuscript be proofread by the authors and revised for correct English usage before resubmission.

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

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Interactive Comment