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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 1:

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,

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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.

Authors answer:

The point raised is very relevant and deserves careful consideration. If the model biases are sensitive to the chemical regime (and there is no evidence that they would not be), the relevance of a correction developed for a given regime can be questioned if the regime changes in the future.

This issue relates to the problem of the stationarity of the correction discussed in Michelangeli et al. and other publications on statistical downscaling techniques. Therefore it does not apply exclusively to the downscaling of photochemical models.

The shortcoming is exactly the same when it comes to correcting for instance total precipitation without considering potential changes in the fraction of convective and stratiform precipitations. In both cases the total magnitude of the response is corrected without taking into account the underlying processes that are being captured by the physical or chemical model being implemented before the correction.

There is ongoing research on the development of conditional corrections (see Vrac et al. 2007 for an example of conditional downscaling). But the application of such more sophisticated approach to photochemical modelling is hampered by the lack of observational data. The correction is used independently at each station, hence in order to develop a conditional correction, one would need to have training periods

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covering both regimes at a given station. Testing a correction developed at one station in another location is not an option because the correction includes implicitly a number of factors (beyond chemical regimes) that are location-dependant.

Unfortunately there is no evidence of a change in photochemical regime at a given station over the past. Regimes are well documented by looking at different stations, and projections show that there is scope for future regime change, but such behaviour is not documented yet in any long record of in situ measurements at a single location.

Therefore, we regret it is challenging to quantitatively validate the robustness of the downscaling and fully address this very relevant question with the data available for the time being. Nevertheless we considered that despite the fact that we implement a suite of CTMs that are designed to capture changes in photochemical regimes, known biases over the historical period could not be ignored. Any bias correction would have suffered from the above mentioned shortcoming, and CDF-t offered a promising refinement in taking into account the whole distribution (and not just the mean bias) and the possible change in the distribution in the future.

A paragraph has been added to the paper to elaborate on theses potential limitations: **"The main underlying assumption is that the transformation remains stationary in time, which is not granted if model biases change in the future. This limitation raises specific concern for photochemical modelling since ozone formation regimes shall change in the future (switching from NOx saturated to NOx limited) and therefore a bias correction developed over a past period might prove less efficient in the future. Note that any bias correction technique would carry the same underlying hypothesis. An alternative in the field of climate downscaling consists of accounting for a conditionality depending on the weather regime (Vrac et al., 2007). However, this is not an option here as one would need to build upon observations at a single location where both regimes occur. For the same reason it is not possible to quantify the uncertainty. Such an assessment should be the priority for future work when long time series exhibiting photochemical**

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regime changes become available. In the meantime, we considered that it was worth doing our utmost to minimize model biases by exploring existing statistical correction techniques that constitute a significant refinement compared to basic bias correction approaches."

M. Vrac, M. Stein, K. Hayhoe. 'Statistical downscaling of precipitation through nonhomogeneous stochastic weather typing', Climate Research, 34: 169-184, doi: 10.3354/cr00696, 2007

Reviewer 1:

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?

Authors answer:

In both scenarios, most of the burden of emission reduction is carried by urban areas (assuming there is more scope for improvement in densely populated areas). In other words, the distribution of the additional emission reduction in the sustainable scenario is identical to the reference scenario. It is difficult to provide a quantitative estimate of the impact on the results of the study without investigating a duplicate of these scenarios that would rely on a homogeneous spatial distribution methodology. However one can refer to the recent paper by Butler et al. 2012, who documented the impact of the same urban parameterisation in the RCP8.5 and found indeed a very significant impact.

The following text has been added to address this comment: **"These exposure-driven trends are designed by means of comparison with emission trends over the re-**

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cent past in order to capture the fact that there is more scope for emission abatement in densely populated area. (Butler et al., 2012) documented the significance of this redistribution algorithm in the case of the RCP8.5 by comparison with a scenario where the reductions of emissions are homogeneous."

Butler, T. M., Stock, Z. S., Russo, M. R., Denier van der Gon, H. A. C., and Lawrence, M. G.: Megacity ozone air quality under four alternative future scenarios, Atmos. Chem. Phys., 12, 4413-4428, doi:10.5194/acp-12-4413-2012, 2012.

Reviewer 1:

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.

Authors answer:

More details on the boundary conditions have been added to the text of 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 1:

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

Authors answer:

A sentence has been added to the text to explicit how the assessment of past trends allow us to better understand the uncertainties in the projections: "Because of their relatively coarse resolution, the CTMs involved in the hindcast were more successful in capturing the geographical patterns of the trends observed at rural than urban stations. **Whereas increasing trends in urban areas were widespread in the observations,**

the models could only capture this behaviour over the main NOx saturated area of the larger Benelux region. The joint analysis of the present projection and the published hindcast allows us to conclude that the projected increase of O3 modelled over the greater Benelux area could actually apply to urban areas beyond the Benelux region in Europe, even if it is not resolved by the models implemented here."

Reviewer 1:

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.

Authors answer:

Thanks to the CDF-t downscaling, the corrected values in the model are very close to the reported values, as seen for instance in Fig 6b. Nevertheless for clarity, we have added these estimates in the revised version of Table 3.

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

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