Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-106-RC2, 2018

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on “Multi-Model Comparison in the Impact of Lateral Boundary Conditions on Simulated Surface Ozone across the United States Using Chemically Inert Tracers” *by* Peng Liu et al.

Anonymous Referee #2

Received and published: 10 May 2018

Overview:

The paper presents a multi-model comparison of simulated surface values over North America of chemically inert tracers released at different height levels at the boundaries of the region. The tracers represent the boundary conditions for ozone in the PBL (BC1), the free troposphere (BC2) and the upper troposphere/lower stratosphere (BC3). The tracers are subject to dry and wet deposition assuming the specification of ozone. The authors find differences in the BC tracer concentrations at the surface and there relative contribution between the four models. These differences are mainly attributed to differences in the simulation of vertical turbulent mixing by the four models.

General and specific remarks

The title of the paper refers to an interesting scientific question, i.e. the impact of BC for surface ozone. However, the paper does not really provide useful answers to that question. My main concerns are as follow, which are further elaborated below:

- The simulated tracers, which are meant to indicate the impact of BC ozone on surface ozone, are not compared in any way to the simulated surface ozone values of the models.

- The tracer results are not juxtaposed to observations (or model errors) in any way despite the surprising fact that the tracer results are shown only for the location of the ozone observations.

- A BC tracer which is meant to mimic ozone should be subject to an equivalent chemical loss, which is not the case in this study.

- The impact of the top boundary is not discussed despite that fact that stratospheric ozone can influence surface ozone in certain situations.

- The choice of the DMA8 average needs to be better motivated as it excludes night time values, when differences in mixing can be large between different models.

- The difference between the models are discussed in a lot of detail, which is overwhelming for the reader

The title of the study suggest that the paper will give an estimate of the impact of the BC on simulated surface ozone. I did not find any such estimate in the paper. The simulated ozone surface concentrations are not compared to the BC tracer results in anyway. I strongly suggest to change the title of the paper to avoid giving a false impression of its content.

Although artificial tracers cannot be directly evaluated with observations, an analysis of the model errors for ozone in relation to the tracer simulation should have been an

Printer-friendly version

Discussion paper

interesting aspect of the paper. On the other hand, the paper does give the impression that observations are considered because the observations are discussed (figure 1, section 2.3) and all plots of tracer simulation (figures 3-4, 6-10) are provided at the sampling location of the observations. But there is no use of this, which is confusing. Producing continuous maps of the tracers simulations would have been more appropriate. The only figure (11) showing a comparisons with observation is a time series of mean differences between rural and urban observations. This could be an interesting figure but it is just mentioned in passing with no detailed discussion.

Getting a better knowledge of the impact of boundary ozone concentrations, both from a modelling or more factual point of view, on surface ozone over North-America is an interesting question. Using dedicated tracer simulations is an appropriate method to investigate the problem. The authors apply tracers that are only subject to dry and wet deposition but not the chemical ozone loss. It is debatable – as also pointed out by the authors in the conclusion - if this approach does full justice to the problem of ozone because chemical ozone loss could be an important factor for the importance of BC on surface ozone. A quantitative comparison of the contribution of (i) chemical loss, (ii) loss by dry deposition and (iii) by wet deposition to the ozone budget in the study area for the different models would have been needed to demonstrate if the used approach is reasonable or not. (It would probably also show that wet deposition of ozone is of minor importance). Truly inert BC tracers, i.e. also without deposition, give some indication of the transport processes only. They would be not specific to the characteristics of ozone (apart from the BC values) but could still give some information. Hence, a better discussion of the runs without dry deposition could be helpful.

The authors find very little transport from the lateral BC to the surface for tracer BC3 (< 250 hPa). It needs to be discussed in more detail, if this means that stratospheric ozone does not impact surface ozone in the study area at all. However, the top boundary condition would need to be considered for such an investigation. Considering only lateral influx with BC3, given that the models had only 1, 3 or 5 model levels above 250

Printer-friendly version

Discussion paper

hPa, seems not sufficient. A realistic top boundary is required to simulate the impact of upper tropospheric and stratospheric ozone on surface ozone.

A clarification of the setup of the BC tracers would be helpful. Did the values of the ozone BC from the global model agree with observations ? Where the BC placed at exactly the same position for all models or did the location of the model boundaries vary from model to model? This could have had an impact on the results.

The main finding of the authors is that vertical mixing differs between the models. It would be interesting to elaborate on the reasons for this. The authors could provide a confirmation of this finding by studying profiles of primary species emitted over the domain. Also, a statement on which of the different mixing scheme leads to the best results would give useful information to the reader.

DMA8 is the only time aggregation of the discussed BC results. The choice of DMA8 for the given applications needs to be better motivated. As the focus of the study is to identify differences in vertical mixing, night-time values should not be disregarded as it is most likely the case for DMA8 of ozone. It is during night time that different vertical mixing (or the lack thereof) can have the largest impact on surface values. Likewise it would be interesting to check if the time period for the calculation of DM8A was the same for the models. Differences in the diurnal cycle can be an indication of different simulation of vertical mixing.

Most of the paper is dedicated to a detailed comparison between different model versions. This is very detailed and can be tedious for a reader, who is not a developer of one of the discussed models. Given this highly technical aspect of the paper, I recommend publication in another, more suited journal such as GMD.

The colour scale for absolute values could be improved. In my printout the magenta (e.g. 25-30 ppb range, top, Fig 3) looks very similar to the red colours indicating high values.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-106, 2018.

Printer-friendly version

Discussion paper