Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-351-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of *CHIMERE* to changes in model resolution and chemistry over the northwestern Iberian Peninsula" *by* Swen Brands et al.

Anonymous Referee #1

Received and published: 19 June 2019

The manuscript of Brands et al. investigates the sensitivity of the CHIMERE model to different horizontal and vertical resolutions and to different chemical mechanisms. The focus is on a region of the northwestern Iberian Peninsula where the CHIMERE model has not been applied so far. The model results are evaluated against observations with a focus on minimum and maximum values.

To my opinion the current version of this manuscript does not fit into the scope of ACP(D) as it is mainly a technical analysis of the model system and has no broader scope or general implications for atmospheric science (see aims and scope of ACP;

'The journal scope is focused on studies with general implications for atmospheric science rather than investigations that are primarily of local or technical interest'). To fit into the scope of ACP(D) a more general geoscientific conclusion/scope of the manuscript would be necessary. However, I think that the technical analysis of the model system is an interesting topic and therefore I propose to revise the manuscript largely (see detailed suggestions below) and hand the manuscript over to the partner journal Geoscientific Model Development (GMD). If the authors wish to publish their manuscript in ACP(D) the scope of the journal should be taken better into account and the manuscript needs major revisions.

General comments:

In p4l6 the authors write '[...]without using downscaling with traffic or population proxies[...]'. Accordingly, also for the finest nest with $0.05° \; x \; 0.04°$ resolution, the authors applied emissions with a resolution of $0.1° \; x \; 0.1°$, right? If this is the case this is an important limitation of the study and needs to be clearly stated. It is well known that the emission resolution influence the results largely. A too coarse resolution of the emissions can also deteriorate the model results (e.g. Markakis et al., 2015). Therefore, I propose to perform additional model runs with a downscaling of the emission as this is a general feature of the CHIMERE model. These additional runs can then be used to quantify uncertainties due to missing downscaling of emissions.

The authors find a poor performance for NO2 of the CHIMERE model and link this poor performance to deficits of the emission inventory. To my opinion this argument needs a more detailed investigation. Several things should be discussed/considered:

- The emission inventory is for the year 2010. What was the emission changes in the last 8 years? What trend do ground-level measurements of NO2 show?

Are there more up to date emission inventories available (for example from TNO, EDGAR etc.)? If so, why don't use them?

- What is the performance of NO2 at the different stations types? How well does the model perform at the 'background' stations? How well at the 'traffic' and 'industry' stations?
- Where are the 'traffic' stations located? Does it make sense to evaluate a model with resolutions of 4 to 5 km and emissions at around 10 km resolution with measurements at the street scale? I guess it makes more sense to evaluate against the measurements at (urban) background stations or average all values of the 'traffic' stations of one particular city.
- How well does WRF reproduce the observed meteorology? To efficient mixing of the boundary layer might cause problems in reproducing the measurements. Please provide at least a basic meteorological evaluation of the used meteorological data.

The authors focus only on daily minimum and maximum values. I agree that especially the maximum values are very important with respect to air quality issues. However, to my opinion it would be very important to investigate also the general ability of the model to represent the hourly variability of the measurements. Therefore, I propose to further perform statistical analysis of the whole time series for each station and not only for minimum and maximum values. Further, the analysis does not take into account that the model concentrations could be shifted geographically (e.g. minima and maxima are misplaced due to coarse resolution of the emissions). Therefore, I propose to provide additionally overlay plots (maybe only in the supplement) combining the geographical distribution of the modelled concentration and the measured concentrations as an example see Fig. 5 of Knote et al, 2011)

I guess that the model runtime (and needed resources) of the different experiments differ heavily. It would therefore be very beneficial to confront the performance of the model with of the different configurations with the model runtime and to give recommendations about the trade-off between model runtime and model performance. This could have important implications for other people using CHIMERE. Further, if the authors (or others) plan to use the CHIMERE model for operational forecasts this trade-off would be very important information.

The description of the performed sensitivity studies (Sect. 2.2) is much too short. Readers familiar with the CHIMERE system might be able to follow the description of the authors; readers from outside the 'CHIMERE world' are lost. Please provide more details about the two different vertical grids (e.g. by a figure showing the different levels). Further, please describe the differences of the two chemical mechanisms in more detail. How do they differ? I know that Mailler et al., 2017 provides some details, but details which are very important for this study should be repeated in the manuscript. Further, Menut et al., 2013 already provide a short comparison of the MELCHIO2 and SAPRC07 mechanisms. How do the findings from the authors compare to the findings of Menut et al., 2013?

Specific comments:
(The line numbers in the manuscript seem to be wrong, at least on page 8). I here refer to the line numbers given in the manuscript)

Abstract: Please provide information about the period consider for this investigation in the abstract (e.g. 20.7.2018-31.8.2018).

Section 2.1: Studies show that ozone on the Iberian Peninsula is heavily influenced

by long range transport (e.g. Pay et al., 2019). Hence, boundary conditions are very important in this region. Therefore, please provide more information about the temporal update frequency of the boundary conditions in Sect. 2.1. Further, the authors mention that chemical boundary conditions stem from different systems (C-IFS, MACC). Further, the meteorological boundary conditions for WRF stem from GFS. Please provide short discussions about the influence of inconsistent chemical (and meteorological) boundary conditions. Further, the authors mention that dust information from C-IFS needed to be scaled. What about the other components? Please provide short information about the quality of the chemical boundary data for the investigated period.

P6l10 The authors mention that for dust there is only a minor benefit when the model top height is increased. For ozone, however transport from the stratosphere is a very important feature which is missing in the applied set-up. Please comment on this issue.
P8l5 How did the authors sample the model data? The authors took the results at the lowest level in the corresponding grid box, right? But did the authors chose instantaneous model results or temporal (e.g. hourly averaged) model output?
P9l21 Especially in complex terrain the height of the lowest model layer and the height of the station might not fit together. Therefore, please provide a comparison of the station height and the height of the model at the lowest layer and check how large the differences are.

Technical corrections:
Figure2: Please fix the legend ('hores')

Bibliography

Knote, C., Brunner, D., Vogel, H., Allan, J., Asmi, A., Aïjalä, M., Carbone, S., van der Gon, H. D., Jimenez, J. L., Kiendler-Scharr, A., Mohr, C., Poulain, L., Prevot, A. S. H., Swietlicki, E., and Vogel, B.: Towards an online-coupled chemistry-climate model: evaluation of trace gases and aerosols in COSMO-ART, Geosci. Model Dev., 4, 1077-1102, https://doi.org/10.5194/gmd-4-1077-2011, 2011.

Mailler, S., Menut, L., Khvorostyanov, D., Valari, M., Couvidat, F., Siour, G., Turquety, S., Briant, R., Tuccella, P., Bessagnet, B., Colette, A., Letinois, L., Markakis, K., and Meleux, F.: CHIMERE-2017: from urban to hemispheric chemistry-transport modeling, Geosci. Model Dev., 10, 2397-2423, https://doi.org/10.5194/gmd-10-2397-2017, 2017.

Markakis, K., Valari, M., Perrussel, O., Sanchez, O., and Honore, C.: Climate-forced air-quality modeling at the urban scale: sensitivity to model resolution, emissions and meteorology, Atmos. Chem. Phys., 15, 7703-7723, https://doi.org/10.5194/acp-15-7703-2015, 2015.

Menut, L., Bessagnet, B., Khvorostyanov, D., Beekmann, M., Blond, N., Colette, A., Coll, I., Curci, G., Foret, G., Hodzic, A., Mailler, S., Meleux, F., Monge, J.-L., Pison, I., Siour, G., Turquety, S., Valari, M., Vautard, R., and Vivanco, M. G.: CHIMERE 2013: a model for regional atmospheric composition modelling, Geosci. Model Dev., 6, 981-1028, https://doi.org/10.5194/gmd-6-981-2013, 2013.

Pay, M. T., Gangoiti, G., Guevara, M., Napelenok, S., Querol, X., Jorba, O., and Perez Garcia-Pando, C.: Ozone source apportionment during peak summer events over southwestern Europe, Atmos. Chem. Phys., 19, 5467-5494, https://doi.org/10.5194/acp-19-5467-2019, 2019.

2019.