Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-295-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "European NO_x emissions in WRF-Chem derived from OMI: impacts on summertime surface ozone" *by* Auke J. Visser et al.

Anonymous Referee #3

Received and published: 21 May 2019

I found the manuscript very well written and clear. All details of the methods seem to be explained in order to assure reproducibility and the results are logically and clearly illustrated. I think the manuscript is basically ready for publication, but I have only two comments/suggestions that the authors may evaluate for a minor revision:

- attribution to soil NOx emissions: the authors make a first-order estimate of the contribution of soil NOx emissions to increased total NOx emissions, after ingestion of satellite NO2 column data, using "anthropogenic" grid cells to estimate the contribution to NOx emissions from sources other than soils. This sounds to be reasonable, also considering the diffuse nature of the NOx emission change. A further relatively simple

Printer-friendly version

Discussion paper

test to confirm the hypothesis would be to run an additional simulation with increased bottom-up soil NOx emissions only by an x%, and see if the changes are consistent with the simulations using top-down emissions, both in terms of spatial distribution and magnitude.

- one interesting area is the Po Valley, which is the one showing the highest NO2 and O3 levels in the observations. The top-down correction of NOx emissions, however, does not seem to be effective enough in this area to fill the gap with observations. This point is sparsely discussed in the manuscript, but it would be useful to have some slightly further comment. For example, Figure 1 in the supplement shows that low values of beta (proportional to NO2 lifetime, from my understanding) are calculated upon main urban settlements (e.g. Milan), but the gamma factor (accounting for changes in the "shape" of the NO2 profile after update of emissions) is the lowest in Europe and pretty flat over the valley. Why is that and could this be a cause for the persistent underestimation of NOx emissions and O3 levels in the area? One rough idea is that the model possibly simulates a quite uniform PBL (thus a low gamma, from my understanding), even if this could be quite vertically inhomogeneous, due to recirculation of air in the valley (see e.g. Zhang and Rao (1999), J. Appl. Meteorol., 38, 1674–1691, doi:10.1175/1520-0450(1999)038<1674:TROVMI>2.0.CO;2; Ordonez et al. (2006) J. Geophys. Res., 111, D05310, doi:10.1029/2005JD006305; Curci et al. (2015) Atmos. Chem. Phys., 15, 2629-2649, https://doi.org/10.5194/acp-15-2629-2015). A further inspection in the vertical profiles over Po Valley, perhaps compared to other polluted regions such as Benelux would be instructive.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-295, 2019.