

## ***Interactive comment on “European NO<sub>x</sub> emissions in WRF-Chem derived from OMI: impacts on summertime surface ozone” by Auke J. Visser et al.***

### **Anonymous Referee #3**

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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 NO<sub>x</sub> emissions: the authors make a first-order estimate of the contribution of soil NO<sub>x</sub> emissions to increased total NO<sub>x</sub> emissions, after ingestion of satellite NO<sub>2</sub> column data, using "anthropogenic" grid cells to estimate the contribution to NO<sub>x</sub> emissions from sources other than soils. This sounds to be reasonable, also considering the diffuse nature of the NO<sub>x</sub> emission change. A further relatively simple

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test to confirm the hypothesis would be to run an additional simulation with increased bottom-up soil NO<sub>x</sub> 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 NO<sub>2</sub> and O<sub>3</sub> levels in the observations. The top-down correction of NO<sub>x</sub> 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 NO<sub>2</sub> 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 NO<sub>2</sub> 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 NO<sub>x</sub> emissions and O<sub>3</sub> 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.

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