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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 #1

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The manuscript has presented the simulations of surface ozone concentrations over Europe in the regional air quality model (WRF-Chem). Its main focus is to analyse the changes in summertime surface ozone over Europe when replacing the bottom-up NOx emission inventories with top-down NOx emission estimates derived from the latest OMI NO2 product. The results show that OMI-constrained European NOx emissions are 56% higher than the bottom-up estimates, and that the increases can be largely attributed to large underestimates of agricultural soil emissions in the model. Model results with the top-down emissions significantly improve the comparison with surface in-situ NO2 measurements and moderately improve the comparison with surface ozone measurements as well.

Overall the manuscript is well organised and written, the methodology is sound. I recommend publish on ACP after the following comments been addressed.

Specific Comments:

(1) Page 1, Line 20 in the abstract:
What does "-48%" mean? Reduced by or to this value? Please clarify.

(2) Page 4, Line 17:
The study assumes 97% of NOx is emitted as NO and 3% as NO2. Can the model simulation of NO2 column be sensitive to this partitioning? Please discuss.

(3) Page 4, Line 31:
Here "+/-40%" should be "40%".

(4) Page 10, Line 17-20:
The sentence is confusing. Why the model underestimates of NO2 column would reflect emissions from power plants being too strong? Please clarify.

(5) Page 10, Line 25-28:
This statement did not explain why there was a larger model underestimate of surface NO2 concentration than that of NO2 column. Can you explain further? Would it reflect biases in model vertical transport or any measurement bias?

(6) Page 12, Line 20-22:
The sentence stated that model "underestimates the highest monthly averaged NO2 observations", but Figure 9 showed the opposite. Model results appeared to be slightly biases high for over high NOx emission regions.
And should hear 0.86 be 0.89?

Interactive comment

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Discussion paper

(7) Page 14, Line 1:
Should 0.40 be 0.41 as seen from Table 1?

(8) Page 13, Section 6.2:
The improvement on surface ozone simulation with the top-down NOx emissions appears to be small. Can you also comment on some other metrics, such as time series of ozone levels at representative sites, or their diurnal cycles?

(9) Page 28, Figure 10: The right panel of Figure 10 is misleading by showing all values including negative values in red. Can you change the color table, e.g., use red for positive values, white for near-zero values, and blue for negative values?

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