Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Impact of spatial proxies on the representation of bottom-up emission inventories: A satellite-based analysis" *by* Guannan Geng et al.

Anonymous Referee #1

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Title: Impact of spatial proxies on the representation of bottom-up emission inventories: a satellite-based analysis Author(s): Guannan Geng et al. MS No.: acp-2016-905

General Comments: The authors explore the sensitivity of NOx VCD to spatial distribution of NOx emissions. Generally, the manuscript presents a sound analysis of the influence of the selected spatial surrogates. It is well-written with clear connections between the objectives and results. There are a few key places the analysis could be improved or should at least be addressed in the narrative. (1) The review of spatial surrogates used by other studies raises questions about the sectors and scenarios used in this study. (2) Further, the scenarios applied do not distinguish between critical sector-based allocation uncertainties. (3) The spatial allocation to the road network is

not sufficientlly discussed. (4) The impact of the prior vertical distribution should be discussed further with respect to artifacts introduced by increasing or decreasing urban/rural gradients in the AMF calculation. (5) The AMF application needs to be more clearly connected to results in Fig 4-6. (6) The OMI NO2 threshold appears to be applied for only some of the analyses (Fig 6) – to what extent would this change bias calculations in previous sections. (7) Lastly, how would you expect the systematic 10% low-bias to affect the outcome of this study? More discussion of each point is provided below.

(1) The review of spatial surrogates shows that previous studies have use more sectors than the MEIC and use better spatial allocation than S1-S3. For example, none of the reviewed inventories uses total population to allocate large power plants, on-road transportation, or biofuels. This raises questions about the reasonability of S1 and S3. The authors should clearly state the value of including "non standard-of-practice" references in the evaluation. The authors should also specifically address the lack of residential biofuel and Table 2 should include a distinction between off-road vs on-road transportation emissions. The S4 and S5 simulations seem most consistent with previous work, and should be the focus of the paper's contribution. Particularly in the conclusions, it should be noted that what the standard of practice is, so that the results are not incorrectly extrapolated.

(2) The five scenarios do not distinguish between the uncertainty in transportation and industrial spatial allocation. The authors correctly point out that the DCW road network is outdated and may be an important source of uncertainty. They then do not quantify the effect of the road network, instead they combine industrial and road network allocations (S4 to S5). Especially because these changes are most relevant to the reviewed inventories (Table 1), the effect of each uncertainty would be very valuable. Can the authors add a simulation or infer contributions from existing data?

(3) The description of the road network allocation lacks specificity and is inconsistent with other parts of the manuscript. The manuscript simply says that the S4 "uses DCW

road networks to allocate on-road transportation sectors." First, Table 2 does not distinguish between on-road and off-road so the effect of allocation is not clear. Second, spatial allocation to a road network can benefit from distinctions between road classes and capacities. The manuscript does not discuss how/if primary or secondary roads are treated differently. Improving the characterization of this process would improve the clarity and reproducibility of results.

(4) The manuscript explains the use of the GC model results as a prior, but does not distinguish between the role of the prior and the change in simulated VCD. Recent work has suggested that higher resolution priors improve the model bias. This work does not distinguish between the role of the prior and the change in simulated concentration. Can the authors clarify the relative roles?

(5) The discussion of AMF in section 2.4 is difficult to connect to results in Figures 4-6. If the AMF is dependent on the GC profile, then the AMF should be scenario specific (S1-S5). In the cited Lamsal et al. (2010), the retrieval was modified to to create a satellite product consistent with the GEOS-Chem run (DP_GC). In this manuscript, Pg 7 ln 18-19 says that the AMF was revised using the profile "from the nested GC model described above," which could be interpreted as inclusive of S1-S5. Figure 4, however, has only one OMI product panel. In Fig 5 and 6, the individual scenarios may be compared to scenario-specific VCD. Please clarify.

(6) The low OMI NO2 values are removed from the PDF in Fig 6 because according to the authors their priors are uncertain. To what extent was the analysis characterized in Fig 5 influenced by grid cells with uncertain priors? The "other" counties, due to the large number, appear to dominate the NMB calculations. The "other" counties are more frequently below 3e15, so the sensitivity to this threshold seems relevant. From the scatter plots, I doubt it will affect your conclusions. Please discuss the reason between using different results.

(7) Evaluation of trends is common with OMI due to the known biases. For this work,

the bias is important to the evaluation of the model. The uncertainties section (3.2) addresses some the uncertainties. Specifically, how does the source of the systematic low-bias (pg 10, 6) possibly affect the outcome of the study? If the satellite retrievals are high-biased, how might the conclusions have been different?

Specific Comments:

- general, 4 municipalities are discussed, but never defined. People not familiar with the region won't know what they are. - general, use specific vertical or slant column terminology throughout. (e.g., section 3.1 Results; Fig 4 caption; etc) - pg 2, 27 - This statement seems to suggest a previous over-reliance on population and, yet, the studies cited here all use other spatial surrogates including road networks, and GDP. Zhang et al. (2009) use Streets 2003, which in Table 1 and the paper uses more than population density as a surrogate. Lu et al. (2011) uses road networks and GDP. Further, this paper does not evaluate the errors introduced by using population for residential or sub-province industry. - pg 5, 28-29 - Will correlation at the provincial level be a good surrogate for sub-province (i.e., county) allocation? Or could emissions be more related to sector than IGDP? Still, seems a good improvement. - pg 6, 4 - Details on the Gompertz formulation would be appropriate here because the allocation of transportation emissions becomes a central outcome. - pg 6, circa 4 - Details on the road network allocation are critical. Are all roads weighted by length density (km/km2) or is road capacity addressed (primary vs secondary roads)? - pg 6, 28 - Which version of EDGAR was used? - pg 7, 19 - The new product resolution is unclear to me. Can you clarify the original resolution and the new resolution? - pg 7, 16-21 - Am I correctly interpreting that OMI NO2 VCD in Figure 5 uses a AMF consistent with that emission inventory? - pg 8, 27 - more details of how the counties are assigned would be useful. - pg 10, 11 - Rephrase. It is not clear which error you are referring to. - pg 11, 6-7 - This is most interesting for the S4 vs S5 differences. The use of newer road networks seems like an obvious best-practice. The IGDP seems like a less obvious improvement, which is why seeing the contribution of each would be nice.

Technical Corrections:

- pg 1, 26 - use IGDP here since that is how it is subsequently used or remove acronym, which is not used in the abstract or elsewhere without redefinition. - Table 1 - citations in table should, but do not appear in bibliography. - Fig 6 - How did your PDF bandwidth affect the shape of S1 and the tails of S2-S3?

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