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Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Estimation of fossil-fuel CO_2 emissions using satellite measurements of "proxy" species" *by* Igor B. Konovalov et al.

Anonymous Referee #2

Received and published: 6 July 2016

The Article *Estimation of fossil-fuel CO_2 emissions using satellite measurements of "proxy" species* by Konovalov et al. is an interesting study about inferring regional anthropogenic CO_2 emission totals from satellite measurements of NO_2 and CO. It is generally well written and of high scientific quality, which is why I recommend it for publication in *Atmospheric Chemistry and Physics* after minor revisions.

In particular, a revised version of the manuscript should address the following points:

- One major issue seems to be the (lack of) distinction between NO_x emissions and NO_2 observations. It seems that the authors use NO_x emission data (reported "as NO_x", "as N", "as NO_2"?) in conjunction with NO_2 measurements. They should explain how the uncertainty in the NO/NO2 partitioning (which can change with season and local time) influences their results, and it should be made clear to the

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reader that the difference between NO_x emissions and NO_2 observations is not problematic in this context (if this is actually the case). This is important, e.g., in the discussion of Eq. 7, and on p.21/l.14.

- The authors should comment on to what extent limiting themselves to measurements over land produces a bias in their estimates, as NO_2 and CO emitted over land and transported over the ocean is not considered (due to the limitation to land pixels) while the emissions are included in the emission totals. The same holds for emissions outside the study area (i.e., Ireland and Eastern Europe), which can be transported to the study area and thus be included in the measurements but not in the emission totals.

- As the present study uses the DOMINO NO_2 product (version 2), the reference to *Bucsela et al., 2013* given on p.5/l. 29 for the AMF uncertainties seems to be off, as that study addresses the NASA OMI NO2 product and not the DOMINO product.

- In the discussion p.10/l.32 and following, it should be stated if the same sampling (coming from cloud and intensity filtering and satellite coverage) is also applied to the CTM values. Also, it should be clearly stated that the satellite retrieval's averaging kernels are applied to all CTM profiles to get the modelled columns (at least I hope that this is the case!). Furthermore, the authors should state how they determined tropospheric columns from the CTM profiles (i.e., use of tropopause information).

- In p.11/l.8 and following, and the corresponding description in Sect. 3.2. it should be noted that the seasonal changes in the "bias" between observations and models strongly points towards systematic errors in the assumed seasonal cycle of emissions and the satellite retrievals (which mostly come from the assumed surface reflectance climatology and the emissions used in the a-priori NO_2 profiles used for the AMF calculations in the DOMINO retrieval).

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- In the context of Eq. 3 (and in general), it would help if the authors clearly stated that their emission estimates are annual totals for the whole study region, divided by sector and species.
- Furthermore, the authors should explain how they derive the Jacobian matrix S_c^s.
- In Eq. 4, it should read "argmin" instead of "agrmin".
- Furthermore, in p.12/l.30, the authors should explain if and how their results are biased towards summer observations, as a result of more available satellite measurements in summer (due to cloud/intensity filtering).
- In the discussion of Eq. 6, it might help the reader if the authors would clearly state that Δ_i^s is the average difference between modelled and observed columns for the month m in which observation i lies. (Or did I understand this wrong? In that case, the authors should clarify their explanation of what exactly they did.)
- In Eq. 10, the authors should explicitly define $\hat{E}_{tot}^s$.
- On p.15/l.17, I believe it should read $\hat{E}_c^{CO2} omb, c$ (missing hat).
- On p.17/l.31 it might be instructive if the authors gave the sample sizes (number of daily values going into the calculations) resulting from the subsampling.
- The authors should discuss to what extent limiting themselves to only one alternative emission inventory (EDGAR and CDIAC for NO_2 and CO) might be problematic – after all, in principle there are more alternatives, and the authors could in principle use an ensemble of alternative inventories.
- On p.20/l.24, the authors should explain how a Cholesky decomposition (of what?) is used to create error samples.

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- In p.21/l.6, it seems that the word "not" is missing in "Note that not only anthropogenic . . . ".
- The authors should explicitly state if they consider the CO_2 intensive cement production as part of the TCO or EHI sector.
- Comparing the "more thatn 60%" in p.28/l.24 to Fig. 10, it seems to me that this is a bit overestimated; from the figure alone it looks more like 50% to me.
- Fig. 2 should explicitly state the units on the y-axis (at least use the word "normalized" in the caption).
- Figs. 10+11 should be more specific in the units on the y-axis: NO_x emissions in Tg NO_x (which NO/NO2 ratio?), or Tg N, or Tg NO_2, . . . ? The same holds for CO and CO_2 emissions.
- The bar for EDGAR in Fig. 11 should be the same color as the bar for EDGAR in Fig. 10.

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