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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-source SO_2 emissions retrievals and consistency of satellite and surface measurements with reported emissions" *by* Vitali Fioletov et al.

Anonymous Referee #2

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The study links SO2 emissions as well as surface measurements to column measurements from satellite by a simple dispersion model. While previous studies on this topic focus on individual point sources, this study uses a generalized model function which allows to derive emission estimates for a list of sources (even close to each other) at once. By establishing the link between emissions and columns, even "reconstructed" SO2 columns were generated for the time before actual satellite measurements are available.

The paper is well written. Results are impressive and convincing, and the method is innovative. It should be published in ACP after dealing with the following issues:

General comments:

1. OMI SO2 Bias

The good results are only reached after removing a somehow mysterious "retrieval bias". When reading Page 7, Line 3, I was thinking about some constant, or weakly latitude dependent bias. But in fact, the bias has systematic spatial structure and considerable spatial gradients. The authors argue that the enhanced OMI signal at the East coast is not reflecting true SO2, and in particular the comparison to OMPS is convincing. However, the reasons for this OMI "bias" remain unclear. I don't find the given reasons (O3 interference, stray light) convincing at all.

I see the need for the high degree of Polynomials fitted to remove the unexplained spatial features. However, I would not call it a "bias" (which I would associate with something like a constant offset).

In addition, the authors should

- extend the description of the characteristics of the bias in the paper and point out the spatial pattern (US Eastcoast) in the main text,

- extend the discussion of possible reasons (in paper or supplement),

- be aware that the high degree of the fitted polynomial actually removes any unexpected signal (by adding degrees of freedom, anything can be fitted), thus the good fit results are not that surprising,

- discuss how far the bias removal might affect the emission estimate, in particular for the study on wind dependency (see next point).

2. Dependency on wind speed

The application of the model fit for different wind speed bins is quite interesting. However, the authors do not provide the resulting emission estimates. The authors claim that VCDs are not good proxies for emissions as they depend on wind speed (Page

10, Line 17). But from Figure 4, I have the impression that not only the local, but also the integrated VCD depends on wind speed, which should not be the case according to the model function. Is this the case? Please provide the emission estimates for the 3 wind speed bins. If they are different, discuss possible reasons. Could the difference be related to the fitted Polynomial? Please provide maps of the fitted bias for each wind speed bin in the supplement.

Detailed comments:

Page 4 Line 29: "...do not vary much" - have you checked this? How would the reconstructed VCDs look like if e.g. the wind data from 2006 would be used instead?

Page 6 Line 17: "prescribed SO2 decay time" - please provide details here and give the numbers used for tau for the different seasons.

Page 7 Line 3: "change slowly": this would apply for a polynomial of degree 2, but not for degree 6!

Page 7 Line 11: "artifact from the retrieval": please extend the discussion of the artifact and possible reasons (here or in the supplement).

Page 8: I understand the reason for the structure of Figures 1&2, but the order of the text is a bit confusing: it first refers to Fig. 2b, then Fig. 1, then Fig. 2a, and in the following to particular columns of Fig. 1. Please try to make the text plus references to Figures more smooth. It would also help a lot to have the columns of Fig. 1 labelled (a to e or I to V) to avoid references like 'Figure 1 (the "VCD from emissions" column)'.

Page 8 Line 26: "Figure 1" -> Figure 1 (e)" (or 1 V).

Page 9 Line 32: For the correlation of reconstructed VCDs with OMI (bias removed and emission-related signal extracted), the same model is assumed for both datasets, and any non-matching measurement is removed from the OMI data (by bias removal). Thus, the good correlation is not that surprising.

Page 11 Line 15: "reached *until* 2014".

Page 12 Line 26 end of sentence: dot missing.

Page 13 Line 6: 0.91 is reached after bias removal, as stated in brackets, but these are NOT the "actual OMI measurements" any more!

Page 13 Line 18: I agree in general, but the requirements on spatial resolution and quality of emission inventories would be high, and sources from power stations, industry and traffic are often close to each other. The authors should add a statement that emission inventories with good spatial resolution would be required.

Page 15 Equation A1: The division by wind actually converts the decay rate from time to space reference system. It would be helpful to indicate this by adding a subscript "t" to lambda, and replace "lambda_1" by "lambda_x"

Page 16 Line 4: For y>0, sigma_1 is just sigma, so how far does "sigma_1 increased with the distance from the source"?

Page 16 Line 12: "calculates" -> "calculated"

Figures 1 and 5:

- add lat/lon coordinates.

- add column numbers (a to e or I to V)

Figure 2:

- shift a, b, c to top left corner of panel or even above the panel

- "d" is missing

Figure 8: Why is 1980-1982 included when there have been no measurements?

Figure 9: place labels a-d above panels.

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