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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Towards monitoring localized CO_2 emissions from space: co-located regional CO_2 and NO_2 enhancements observed by the OCO-2 and S5P satellites" *by* Maximilian Reuter et al.

Anonymous Referee #2

Received and published: 12 April 2019

General comments

The authors estimate CO2 emissions of cities, power plants and a wild fire from OCO-2 XCO2 observations using a mass-balance approach. They include Tropomi NO2 (slant) columns to improve the emission estimation and present six examples for which emissions were estimated. The paper describes relevant and new ideas and is generally well structured, but more results should be included to support the conclusions better.

Therefore, I recommend a major revision of the manuscript which should address the comments below:

* The number of cases studied is very small. Although 20 promising scenes were identified only six are shown in the manuscript. The authors should include the remaining cases, not as additional examples (except maybe in the supplement), but in order to have more cases to discuss and compare the results.

* The manuscript lacks a detailed comparison of the examples and discussion of the result. A broad summary is given in the conclusions, but this should be moved to a designated section. Furthermore, the results of the flux estimates without including NO2 fields in the fit should be shown in the results and not only briefly mentioned in the conclusions (P15, L6ff).

* The connection between this study and the proposed CO2M mission, which is emphasized in the abstract and the conclusions, is not well presented. The authors used the NO2 fields to identify the location of the source outside the OCO-2 swath and to screen for potential sources upstream. Both will not be possible with the CO2M mission, if CO2 and NO2 instrument would have the same swath as currently proposed. In addition, it might also not be necessary for CO2M to use NO2 to identify the source outside the swath, because CO2M's swath will be significantly wider than OCO-2's swath. A major advantage of the NO2 observations is likely the potential for improving the fit of the Gaussian (see previous comment), which should be presented more prominently.

Specific comments

P2, L5ff: Consider re-formulating, e.g.: "... to halve [...] emissions each decade after reaching peak emissions in 2020"

P2, L21-23: Consider to remove, because the detailed chemistry seems not relevant for the study.

P2, L27: Add the resolution of the NO2 instrument.

P2, L29: Clarify the term “localized small scale”.

P3, L10f: Consider changing “The data set. . .” to “The product. . .” to make it clear that the filtering is part of the OCO-2 L2 Lite product.

P4, L1: Please explain the term “viewing angle correct”. Are these geometric air mass factors?

P4, L4-9: The paragraph might be easier readable, if it first explains the approach used in this study and briefly contrasts it the “normal” approach.

P4, L13: It would be useful to have the time difference between OCO-2 and Tropomi for the six examples presented in the manuscript.

P4, L22: Please specify if three times lower resolution is temporal, spatial or both and, if spatial resolution, if it is grid cell area or length. Maybe it’s better to just write the resolution of the product.

P5, L7-8: This is not a constraint on the width of the CO2 Gaussian function, if CO2 and NO2 values are fitted simultaneously, but using the same width for fitting both CO2 and NO2 Gaussian function. It would be a constraint if the NO2 plume is fitted first and afterwards the CO2 plume is fitted using the coefficient obtained from the NO2 fit.

P5, L13: Please clarify if a Level-2 or Level-3 product is used for the fit.

P5, L22: The equation would be more readable without the unit conversions. The equation could be split in two.

P5, L27: Does the OCO-2 product have not air columns that could be used instead?

P6, L3: Please add how sensitive the factor (i.e. 0.53) is to surface pressure, to provide a range in which this approximation might be used.

P6, L24: Please state typical values for the uncertainty of the wind speed in the ERA5 data.

P7, L5: Are the 100 co-locations for Level-2 or Level-3?

P15, L1ff: The major advantage of the NO2 instrument here would be the wider swath and thus having CO2 instrument with a wider swath should bring the same advantage without the need for an additional NO2 instrument. The authors should consider discussing if (or why not) having a CO2 instrument with a wide swath is (not) an option.

P15, L22: Please explain the term “steady state conditions” in the paper.

Figure 1-6: The corrected wind arrow looks very subjectively. It might help to draw the arrow centered on the XCO2 swath where the arrow should be a perpendicular to the plume.

Table 1: Mention that the single sounding uncertainty (0.4-0.7 ppm) provided the OCO-2 product was used for computing the flux uncertainty, because evaluation with TCCON found a significant larger value (1.3 ppm).

Table 1: Add to the columns that estimated cross sections are instantaneous; EDGAR and ODIAC are annual or monthly emissions.

Technical corrections

P1, L10: XCO2 was already defined in line 3

P5, L5: Consider changing “. . .first degree polynomial (i.e. a linear polynomial). . .” to “. . .a linear polynomial. . .”.

P5, L6: Consider adding commas before “accounting” and after “values”.

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