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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.

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Reuter et al. investigate the ability to estimate CO2 emissions from localized sources using satellite observations of CO2 from OCO-2, with the help of NO2 data from Sentinel 5P TROPOMI. The scientific significance of this paper is high since it attempts to address a question of importance to the design of the planned European Copernicus Anthropogenic CO2 Monitoring mission constellation and observations from other existing, planned or proposed satellite missions. Although there have been some past theoretical studies on this subject, this is the first study, to my knowledge, to estimate

local-scale CO2 emissions with real satellite observations of both CO2 and NO2.

The methodology applied wisely uses NO2 (with a wider observational field and shorter atmospheric lifetime than CO2) to effectively identify the plume shape, wind direction and potential interfering sources, and thus the approach is independent of any assumptions about NO2:CO2 emission ratios. The figures and general presentation of the manuscript are of high quality. Overall, this work is sufficient to demonstrate the value of coincident NO2 and CO2 satellite observations for estimating emissions, however, some over-simplifications in the method make the actual emission estimates questionable despite their large uncertainties. These issues include assumptions about the effective altitude of emissions, atmospheric stability, treatment of area sources, and reporting in annual units. All of these issues are elaborated on in the specific points below.

Furthermore, some questions remain unanswered such as an assessment of the impact of a temporal offset on the value of the NO2 observations, which occurs when the observations are made from a different satellite (as in this work) or could potentially occur even with a different instrument on the same spacecraft if the scanning approach differed. Despite these limitations, this is a useful study that contributes to our understanding of combining CO2 and NO2 observations for anthropogenic CO2 source estimation and thus I would recommend it for publication in ACP after some revisions.

Specific Points

Page 2, lines 15-17 is a jumble of references to different techniques and different types of measurements (satellites and airborne). Since the rest of the paper is about satellite observations, the reference to the airborne measurements and associated emission estimates of Krings et al. 2011 and 2018 should either be removed or some explanation is needed as to why they are relevant here. It would also be useful to better distinguish between studies that quantified/estimated emissions versus identification. It might also be helpful to point out the studies that took advantage of atmospheric imaging capability, which is crucial to the current work.

P2, L27. Specifying that the S5P launch was in October 2017 would help to clarify for the reader why only observations from 2018 were used in this work.

P3, L8: “eight parallelogram-shaped footprints across track with a spatial resolution at ground of $\leq$1.29 km x 2.25 km”

P3, L13: It would be helpful to add a statement along the lines of “The OCO-2 v9 data set has an improved bias correction approach that results in reduced biases particularly over areas of rough topography.”

P4, L2: It would be helpful to provide the SNR or perhaps a relative precision instead of just the random noise, since most CO2 specialists will not have a good grasp of the magnitude with these units.

P4, L14: “50 minutes” According to the figure labels, the time differences range from 6 minutes (Medupi-Matimba) to 35 minutes (Nanjing). Perhaps it would be more informative to state: “each scene observed by OCO-2 is also observed by S5P with a temporal offset ranging from 6 to 35 minutes”?

P5, L5-10: The method described is interesting and very sensible and is one of the strengths of this work.

P5, L29 – P6 L1: The manual adjustment to wind direction but not windspeed is similar to the approach of Nassar et al. (2017) which would be worth acknowledging.

P6, L7: This constant factor of 1.44 taken from Varon et al. (2018) to treat the vertical dimension is a major oversimplification in this work. Varon et al. (2018) simulated CH4 plumes that might be typical of CH4 leaks from infrastructure, thus they deal with smaller spatial scales and little to no temperature contrast. The effective vertical height of emissions will likely be very different when dealing with smokestacks, urban areas or wildfires, as in the present work. In fact, a new paper (Brunner et al. “Accounting for the vertical distribution of emissions in atmospheric CO2 simulations” Atmos. Chem.

Phys., https://doi.org/10.5194/acp-19-4541-2019) that also has links to the Copernicus candidate CO2 Monitoring mission, describes the relevant factors for the vertical distribution of emissions, where different vertical emission profiles for point sources (i.e. a power plant) or area sources (i.e. an urban area) are discussed. The temperature of the emissions and the season are also shown to be important factors. Although detailed study of plume rise is complex and beyond the scope of this paper, and the use of column data reduces the importance of these issues, surely it must be too simple to use a single factor of 1.44 times the 10 m wind speed to represent plume rise from the diversity of source types and geographic locations studied in the present work. According to equation 3, errors in emission estimates will be approximately proportional to the error in wind speed, so getting a realistic wind speed is important.

P7-11, It would be most useful to have the wind direction adjustments clearly stated for every case either in the text or a table.

P8, L7: “larges” -> “largest”

P8, Sec 3.2: The enhancement near Lipetsk is huge and the fit is very good. Are there other sources in addition to the gas-fired power plant and the steel plant that could be relevant, for example, what about the city of Lipetsk (population $\sim$500,000)?

P9, Figure 1: I assume the hashed/shaded region is the Moscow urban area but I am not sure? Can the authors clarify in the figure caption?

P9, Sec 3.4: The OCO-2 flyby of the Matimba and Medupi power plants used in this work is over 80 km away. Nassar et al. (2017) also estimated the emissions from Matimba using OCO-2 data (but version 7) from a direct overpass in 2014 and a close flyby ($\sim$7 km away) in 2016. Daily emission estimates from Nassar et al. converted to annual values are $\sim$12 MtCO2/a.

P10, Sec 3.5. The Australian wildfires are clearly an example of an area source not a point source. The NO2 data show structure/heterogeneity in the area source. This makes it a poor candidate for the modeling approach applied that represents the plume with a Gaussian function. Furthermore, it makes little sense to report emissions in an annual unit in the case of a wildfire, which lasts on the order of days to weeks and would demonstrate temporal variability even over that limited time scale. For fossil fuel CO2 emissions from power plants or cities, there is also periodic (diurnal, weekly and seasonal variability) and non-periodic (plant shutdowns, heating/cooling linked to weather, etc.) variability, which also makes reporting emission rate estimates for shorter time scales more exact from a single overpass.

P14, Figure 6. Nanjing seems to show two maxima in the NO2 image, which is also problematic to represent with a Gaussian function.

P14, L13: Why is 0.5/MtCO2/a the chosen minimum value?

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