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Interactive comment on "A methodology to constrain carbon dioxide emissions from coal-fired power plants using satellite observations of co-emitted nitrogen dioxide" *by* Fei Liu et al.

Anonymous Referee #2

Received and published: 5 August 2019

Liu et al. describe a method to estimate CO_2 emissions from power plants using satellite observations of tropospheric NO_2 columns. The method involves the estimation of NO_x emissions using a top-down approach previously developed by the authors and estimation of CO_2 emissions by applying a NO_x/CO_2 emission ratio derived from direct stack emission measurements of both gases. The topic of the manuscript is important and relevant in the context of the ongoing development of the global emission monitoring system intended to support the elaboration of climate control and mitigation strategies. Although the idea to use satellite NO_2 measurements to constrain CO_2

emissions from fossil fuel burning is not new, application of this approach to specifically power plant emissions is a step forward. Another new point of the study is the analysis of the relationship between NO_x and CO_2 emissions from different types of coal-fired power plants in the US. That said, I keep wondering whether and how the method proposed in this manuscript can be proven useful in any scientific or practical applications. The weak points of the manuscript and my suggestions to the authors are outlined in my comments below.

Major comment

I find that the manuscript lacks clear logic in presenting the ideas and results of the authors. Specifically, while the main focus in Section 2 ("Method") is given to the analysis of the CEMS stack measurements in the US in the period from 2005 to 2017, it is not explained and justified how the outcome of this analysis can be used for applications outside of the US. Such possible applications are illustrated in the manuscript (in Sect. 3.3) by the example of only one power plant (Matimba), for which the authors use the NO_x/CO_2 emission ratio estimated only for 2005 and even argue that this estimate (based on the US data) is not directly applicable to the Matimba plant. Furthermore, if the "regressed" estimates of the NO_x/CO_2 emission ratio are not directly applicable to power plants outside of the US, the application of these approximate estimates to the selected 8 power plants inside of the US (presumably to test the method) seems to be pointless, as the CEMS measurements provide accurate direct estimates of the NO_x/CO_2 emission ratio for any power plant in the US. As for the Matimba power plant, a reasonable alternative to using the CEMS measurements would be to get a corresponding estimate of the NO_x/CO_2 emission ratio from the ODIAC inventory. Therefore, in the present form, the discussion and evaluation of the method is very confusing and, to some extent, misleading. In this respect, I recommend that the authors illustrate the potential of their method and the usefulness of the analysis of the US CEMS data by considering a few more power plants outside of the US (e.g., in China), paying special attention to the accuracy of the estimates of the NO_x/CO_2 emission ratio

based on the US CEMS data versus the accuracy of corresponding estimates that can be obtained directly from available data of global and regional emission inventories.

Specific comments

p.2, l.16-18: I believe that the narrow swath of the OCO-2 sensor is not the main reason for the limitations of the novel and promising method proposed by Reuter et al. (2019). I suggest that the authors provide a more extensive and accurate discussion (not necessarily in Introduction) of the advantages and disadvantages of their approach with respect to that of Reuter et al. (2019).

p.2, l.37: I recommend that the authors avoid boasting about the "novel" method here and elsewhere. Actually, the only significant new point of their method is that it is focused on a particular source of CO2 emissions (as noted above). A very similar method to constrain CO2 emissions is described in previous papers (cited in this manuscript) focused on estimating fossil fuel burning CO2 emissions in China and in Europe. Certainly, there are differences concerning the ways to estimate the NOx emissions and NOx/CO2 emission ratio in the different studies, but these differences are mostly of technical nature. Furthermore, the method which was used to estimate NOx emissions in this study is identical to that presented by the same authors in their previous papers.

p.3, l.7-12: It would be useful to explain briefly why a special approximation procedure is needed to estimate a NOx/CO2 emission ratio while using the CMES data (i.e. why the NOx/CO2 emission ratio for any given power plant in the US could not be directly evaluated using the corresponding CMES measurements).

P3. l.21. It is quite unusual and inconvenient that the first figure ever mentioned in the manuscript is Figure 5 (instead of Figure 1). The order of the figures should be corrected.

p.3, l.29-32: The authors should explain the origin and significance of the value "1.32". Would their estimates be less accurate if they assumed that the NOx/NO2 ratio equals,

say, to 1.3? Further, do the authors imply that if one had a way to measure the NO/NO2 ratio around any power plant anywhere in the world with a spatial resolution of 13 km×24 km, then the measured NOx/NO2 ratio would be exactly 1.32? Wouldn't the NOx/NO2 ratio actually strongly vary from site to site and would depend on the ozone level (which is frequently not determined by local pollution sources) and the age of the plume? Doesn't the fact that the estimates of the NOx lifetime inferred from satellite measurements vary across the 8 power plants within almost a factor of 2 (according to Table 2) mean that OH (and therefore O3) levels are quite different in plumes from different power plants? Overall, I believe that the uncertainty associated with the estimation of the NOx/NO2 ratio should be carefully discussed and evaluated (perhaps, using a chemistry transport model). A brief and superficial discussion of this important point in Liu et al. (2016) is certainly insufficient.

Table S1: The authors provided some useful supplementary information for Sect. 2.1 in Table S1, but this table is not mentioned and discussed anywhere in the manuscript.

p.4, l.3-33: I suggest the authors provide an additional figure illustrating the NO2 plume from the Rockport power plant along with a corresponding Gaussian fit.

p.5, l.5: It would be helpful if the authors explained here what is the purpose of creating "continuous and consistent records of ratio_CEMS...". Are these records supposed to be helpful for estimating CO2 emissions inside of the US (although accurate estimates of the NOx/CO2 ration are already provided by CEMS for each power plant) or outside of the US (although the applicability of the CEMS data outside of the US is very questionable)?

Sect. 3.2: In my opinion, the uncertainties of the emission estimates inferred from the OMI measurements are well characterized by the standard deviations reported in Table 3. However, these "data-based" uncertainty estimates are not discussed in the manuscript. The present discussion of the uncertainties, however, looks very superficial. I suggest the authors provide a separate table (e.g., in the Supporting information)

reporting the uncertainties associated with each power plant and with each individual factor contributing to the total uncertainty. Also, I wonder how a reader acquainted with the basic knowledge of the mathematical statistics is supposed to interpret the values of the uncertainty reported in this section. Do these values represent the standard deviation (that is, the confidence interval corresponding to the 68.3 percentile)? If so, does the fact that the uncertainty estimates range from 62%–96% mean that there is a significant chance that a true value of the emissions can be below zero (assuming that the error distribution is Gaussian)? My suggestion is to consider reporting the so huge uncertainties in terms of the geometric standard deviation (thus assuming that the error distribution is log-normal).

p.7, l.19,20: If the authors believe that the NOx/CO2 emission ratio at Matimba is on the upper end of the US values, then perhaps they should have used a maximum value of the NOx/CO2 emission ratios among all of the US power plants without NOx emission control. Anyway, it is not clear how the standard deviation of ratio_regressed was evaluated? Is it the standard deviation of the slope of a linear fit or the standard deviation of the original NOx/CO2 emission ratios from the CMES data?

p.7, l.29-31: According to Reuter et al. (2019), the CO2 emission estimates for the Matiba power plant are available also from the ODIAC inventory. The authors could consider using the corresponding estimates for comparison.

Conclusions: This section looks unusually short for ACP. Furthermore, instead of providing a clear and logical summary of the major findings of the study, the authors preferred to speculate about possible future developments of their method. Accordingly, I believe this section needs to be re-written and significantly extended.

Figure 2: Do the emissions shown in this figure correspond to the ozone season only? If so, this should be indicated in the figure caption. The regression coefficients could be reported only with one or two digits after the point. Is there a reason for showing a linear regression with the intercept term in the panel (c) and without the intercept in

other panels?

Figure 8: The meaning of a shaded band should be clearly explained in the figure caption. I suggest also to supply the emission estimates inferred from the OMI observations with the error bars corresponding to the mean of the standard deviations reported in Table 3.