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Comment on egusphere-2022-1490

Gerrit Kuhlmann (Referee)

Referee comment on "Monitoring and quantifying CO_2 emissions of isolated power plants from space" by Xiaojuan Lin et al., EGUsphere,
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The study estimates CO2 emission of power plant using OCO-2 and OCO-3 observations using the Gaussian plume inversion and cross-sectional flux method with different input parameters. The methods are tested for U.S. power plant for which bottom-up reports of hourly emissions are available and afterwards applied globally. The paper well written but some aspects on the method are unclear. I would recommend publication following a revision based on the general and specific comments below:

- **Background**: For Gaussian plume model method please describe already in L153ff how you calculate the background. In L200, you write that the 90th percentile was used, which seems to be based on tests with different percentiles with the aim to minimize the difference between estimated and reported emissions (L220, Figure S2). The choice of the percentile (60-99th) will mostly result in a bias in the estimated emissions, which might be caused by the background, but can also be the result of other systematic errors in other parameters (e.g. effective wind speed). Therefore, how does this choice of the background agree with the background that you compute with the cross sectional flux method (L175)? Would a different background affect your conclusions on the best approach for computing the effective wind speed?
- **Wind**: The evaluation of the different wind products in your study is inconsistent. You use winds from ERA-5 (0.25°), MERRA-2 (0.5°x0.625°) and high-resolution ECWMF forecast (resolution not mentioned). You use the wind speed at the center of the PBL for the ECMWF forecast. However, for ERA-5 and MERRA-2, you take 10-m winds multiplied by the empirical scaling factor of 1.4 from Varon et al. (2018). When you compare the impact of the different wind products on the estimated emissions, it is not possible to identify if the different performances are caused by differences between the products or the different computation of the final product (scaling factor or wind at half PBL height). To analyse this better, I suggest comparing all datasets using both the 1.4-factor and the wind at half the height of the PBL. Note that the scaling factor of 1.4 is derived for CH4 plumes measured by high-resolution satellites, which are inherently different to CO2 plumes from power plants measured by OCO-2. Therefore, using the value might be not the best approach, even it is true that it has been used in previous studies "for convenience" (Reuter et al. 2019).
- **Uncertainties:** You seem to compute the uncertainties using an ensemble approach with a rather small number of members (3 for wind and 4 for background) for computing reasonable statistics (see also previous comment on the wind). How large are the uncertainties of wind speed in m/s and background in ppm? How do these uncertainty estimates compare to estimated uncertainties in previous studies? How large is the uncertainty of the fitting parameters for the background in Equation 3?

Finally, how large would be the uncertainties from the assumption and simplification of your methods such as the assumption of steady state conditions?

Specific comments

L171ff: You write here that you fit equation 3 to obtain parameters, k, b, A and sigma. Then, you write that you subtract the background to compute the line density. However, your parameter A is already the line density, so I don't understand why you need to calculate it again.

L182: It is not clear to me how you compute the wind here. Do you rotate the wind vector so that it points from the source location to the maximum in the OCO swath?

L235: You write that the peak is well described by a Gaussian [curve] in Figure 1b. However, no curve is shown in the figure.

L240ff: You partly repeat the description of your method here, which seems unnecessary.

L261: Please discuss why WPBL provides better results than the other two options.

L265: Do you use the arithmetic average or the weighted average considering the uncertainty of the estimates?

L268/Figure 2: It surprises me that r^2 is so much higher for summing compared to averaging? Can you explain why this is the case?

L279: This relates back to my previous comment how you do calculate the normal wind for both methods. Are the difference between estimates and wind speed used in both method correlated? Another reason for deviations can be the method for computing the background.

Figure 5: The red line is somewhat misleading, because without reading the caption one would assume that you could estimate emissions for 8% of all tracks near power plant, while in truths it is only 0.05%. I would strongly suggest removing it to avoid confusion.

L340ff: Does this number of 1522 Mt/a correctly accounts for observing the same power plant in different years or does this never happens? I am asking because the percentage numbers for the individual years add up exactly to 17, which would not happen if you estimate for the same power plant more than once.

Figure 7b: It is difficult to see the bars for most countries. Maybe the figure can use a logarithmic scale on the y-axis.

L367f: The conclusion on the difference between cross-sectional flux and Gaussian plume method needs more explanations (see previous comment).

L374f: Unfortunately, GeoCarb was recently canceled. CO2M is developed by ESA and EU with involvement from EUMETSAT and ECMWF. It is probably easiest to call it a "European mission". The Japanese GOSAT-GW should be mention here, as well.

Supplement: The resolution of the figures in the supplement is very low making it difficult to read the labels. In some cases, labels and units are missing (e.g., S9).