Review of the manuscript:

Estimation of diurnal emissions of CO2 from thermal power plants using spaceborne IPDA lidar

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Synopsis:

This paper presents a comprehensive analysis of XCO2 observations obtained in the vicinity of coal-fired power plants using the ACDL spaceborne instruments on board the DQ-1 satellite. Leveraging the integrated differential absorption (IPDA) lidar technique, the work utilizes these data to derive emission fluxes and compare them against existing inventories.

The IPDA method offers a significant advantage over passive remote sensing technologies by enabling measurements at night and in high-latitude regions, where traditional methods are often limited or ineffective. By employing a Gaussian plume model to interpret the observed XCO2 signatures, this study aims to infer CO2 emissions from coal-fired power plants.

General Review:

In my opinion, this contribution provides a substantial step forward to infer CO2 emission from satellite-borne instruments. As the first of its kind CO2 lidar in space, the work provides new data and thus is significant for the scope of Atmospheric Chemistry and Physics.

The scientific approach and methodologies employed are clearly presented, and the results are discussed in a largely satisfactory manner. However, to strengthen the paper's credibility, I recommend adding references to relevant existing studies. The results themselves are presented concisely and effectively, with conclusions drawn by the authors that are well-supported.

The scientific approach and the methods applied are well presented. The results are mostly discussed appropriately, however, several references to related work need to be added. The results are presented in a clear and concise way. The authors developed their conclusion well. Some of the figures should be improved for the sake of readability and, language-wise, some expressions need improvement.

To estimate CO2 emissions from power plants, the authors apply a Gaussian-shaped plume model derived from theoretical considerations.
In order to estimate the CO2 emissions of power plants the authors fit the results to a Gaussian-shaped plume derived from model. It is suggested that the authors provide justification for choosing this approach over alternative methods, such as budgeting (Riemann sum) approaches e.g. https://doi.org/10.1364/AO.56.005182 .

The most significant deficiency of the paper is its failure to discuss potential implications of turbulence on CO2 emission estimates. Turbulence can significantly impact the validity of applying a Gaussian plume approximation and may lead to biases in deriving emission rates, especially considering the expected differences between night-time and daylight measurements. This topic requires a more detailed discussion, given its complexity.

This is more detailed in the specific review below.

Specific Review:

Abstract and Introduction:

- The authors' conclusion in the abstract that their method is adequate for monitoring diurnal variations in power plant emissions strikes a note of caution, given that the satellite only overpasses the sources once per night and once per day, which does not provide better time resolution than what the method can account for.

- The authors provide a comprehensive review of studies utilizing passive remote sensing techniques. Notably, they accurately highlight the benefits of active over passive remote sensing methods. Nevertheless, it would be beneficial for them to extend this thoroughness to lidar-related references, which appear to be incomplete.
  At least the following publications need to be cited:

  – Ehret et al.: https://doi.org/10.1007/s00340-007-2892-3

  – Menzies et al., https://doi.org/10.1175/JTECH-D-13-00128.1

  – Wolff et al.: https://doi.org/10.5194/amt-14-2717-2021
  This paper does not only present airborne IPDA measurement of CO2 plumes from power plants, but also discusses the mass balance and Gaussian plume approximation using lidar cross sections. Moreover, it discusses turbulence and its impact on IPDA measurements. Thus, it is of high relevance to this paper.

  – Kiemle et al.: https://doi.org/10.3390/rs9111137

Even for results for passive remote sensing (eg. w.r.t EM27/SUN) a few important publications are missing and should be cited:

– Luther et. al: https://doi.org/10.5194/amt-12-5217-2019
– Ye et al.: https://doi.org/10.1029/2019JD030528
– Wu et al.: https://doi.org/10.5194/gmd-11-4843-2018

Concerning plume shapes and results from different models, Brunner et al. need to be cited.
https://doi.org/10.5194/acp-23-2699-2023

- The number of power plants/ observations seems to be inconsistent within the abstract. It becomes not so clear how many individual power plants have been observed (10?), how many of those have been overflown more than once, how many overflights were considered in total (38?) and how many overflights took place at night (15?) and day (23?). Please clarify. Possibly, table 2 can be improved with this regard. It would also be helpful to know about the percentage of “good” overflights versus all overflights.

- Be more specific in what is improved versus the Han et al. paper.

Data and Methodology

Line 113: Explain why water vapour column information is needed.

Para 2.1.3: Can you comment on the accuracy of the Climate Trace Data and specifically about the emissions of those power plants considered in this study.

Line 125: Bovensmann et al. is certainly not the original reference for the Gaussian plume approximation. The authors should probably look for a better suited reference.

Line 135: "gravitational acceleration" (rather than acceleration due to …)

Figure 1: Please improve the graphs such that the labelling of axes can be read.

Line 148: Please describe why you use a 30-km distance as the limit.

Line 153: Please describe why you use a 25° angle as the limit. It would also be interesting to know how many overflights had to be discarded due to wind directions outside of this criterion, i.e. the percentage of "good" overpasses versus all overpasses.

Line 159: Please describe how cloud cover and solar altitude are considered in the study of atmospheric stability. What about solar altitude and night-time measurements?

Line 160: Over which distance smoothing was applied. In this context, the authors should be more precise. At some point the precision of the measurements (for the applied averaging interval) should be given.

Para 2.2.: The authors should better distinguish between stack height and plume heights. To do so, a paper by Brunner et al. is considered to be important and must be cited: https://doi.org/10.5194/acp-19-4541-2019

Results and Discussion

Line 186: Here, a radius of 50 km is given. This is inconsistent with the 30-km criterion given above. Correct or explain!

Line 188: Please be quantitative in specifying the filter criteria. For example, what minimum optical depth of "thick" clouds leads to exclusion?

Line 199: Pleas include (e.g. in Table 1 or 2) for which power plants the stack height is known to the authors. Moreover, the stack height is not uncertain, it is just not known to the authors.

Line 200: What is meant by: "OCO-2 satellites have no valid Gaussian plume data?

Table 2: Please provide also the year of the respective overpass.

Figure 2: The source of the maps should be given. It would be nice to provide a map insert providing a better information about where the GRES power plant is located. Please increase the axis labels on the left figure.

Figure 3: Please increase the figures and labels for better readability. It might be more instructive to show the overpasses in the same map cutout and scale. Is there a significance in the size of the wind arrows? I suggest to provide approximate wind speed information and local time of overpass. Probably is makes no sense to apply the same scale to all graphs due to different background values, but at least the same spread (2 ppm?) should be applied to all figures.

Line 216: How is the lift height derived from stack height and atmospheric stability. Does the temperature of the gas play a role?

Line 253: Provide a number for the linearly varying background.

Figure 7: Climate Trace (not Change?); The reader may think that this is continuous data, but the lines consist of 7? individual daytime and 9? nighttime observations. Please plot the data points!

Line 298: Which power plants provide stack heights? Include this information in one of the Tables.

Line 300: The authors must comment on the single pulse-pair precision that is used to fit the Gaussian plume. On short spatial scales, this information is more important than the accuracy of the IPDA measurement since the background is subtracted.

Table 1/2: Swap the order of the tables. Table 1 should contain the basic information about the power plants and Table 2 the uncertainties.

Figure 8: See comment above about the error of the data base.

Line 325: Without robust evidence on the diurnal (day-night) and seasonal (winter-summer) variations in power plant emissions, any claims about these differences are speculative and should be clearly labeled as such.

Line 333: Quantify the higher emission rates vs. Carbon Brief and Climate TRACE.

Conclusions:

In the conclusions, I would like to see suggestions for potential improvements in emission estimate accuracy, such as utilizing more advanced models or those with higher spatial resolution. For instance, the authors briefly mention WRF (Weather Research and Forecasting) model in the introduction. However, it would be helpful to provide a more detailed discussion on what enhancements can be expected from using this model instead of a Gaussian plume model, as well as any challenges that might arise.

Data availability:

Please give a hint about the availability of ACDL data, since this is the major data source used in this work.

Summary:

To summarize, the manuscript presents valuable findings based on new data, but also exhibits some shortcomings, including missing citations and a critical oversight regarding the impact of turbulence on the results. Notably, this omission is particularly concerning in light of potential differences between daytime and nighttime measurements. In order to ensure the manuscript's credibility and thoroughness, I believe it is essential to address these issues before publication.