Discussion: Deep learning applied to CO_2 power plant emissions quantification using simulated satellite images

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In the following, the referees comments are in italics and in blue.

Report 1

We would like to thank again Evan D. Sherwin for his comments or requests for clarification, we hope the latest version of this manuscript is even clearer.

The revised manuscript is significantly improved and largely addresses my comments. I still recommend the following relatively minor revisions before acceptance. As a general point, it is helpful to reviewers if the response document reproduces key changes to the text (or relevant pre-existing parts of the text). For each reviewer comment, the authors should generally point to one or the other (or both).

Detailed comments:

- *1.1: The response says "experiments are independent" and that no initial experiment provided implicit info for model tuning. While I am sure this is true, the authors should state that in all cases in this study, there was no independent test set that had not been used by any other models used in this study by the authors. The authors are then welcome to make a plausibility case along the lines they do in this response as to why the approach they took may be justified in this instance.*

  To account for this suggestion, we have added "It is important to note that while the test dataset from one experiment appears in the training dataset of another, each experiment was conducted independently. The model tuning was not influenced by the results obtained with the test datasets" in section "4.3 Geographical separation between the training and test datasets".

- *1.2: Computer vision for remote sensing is indeed a very active field, but if the authors want to make the case that the choice of preprocessing layers is a critical methodological innovation, citing two other papers does not really* *seem like enough. Incorporation of NO2 data is probably relatively novel, but I recommend citing other papers that attempt CO2 remote sensing and do not use NO2 data (they do not need to be computer vision/ML papers). My understanding is that plenty of studies use the sort of poorly-resolved wind reanalysis data used in this study for emissions estimation. Please cite more additional studies than just this one, but here is an example worth mentioning: Kumar et al. 2023, computer vision-based remote sensing of methane: https://openaccess.thecvf.com/content/CVPR2023/html/Kumar_MethaneMapper_Spectral_Absorption_Aware_Hyperspectral_Transformer_for_Methane_Detection_CVPR_2023_paper.html*

NO2 data (for prediction of the position of the plume) or winds data are indeed already used in other papers (cited in our manuscript). What we were referring as "critical methodological innovation" was the data generation process choices and set-up (precisely: the succession of the six steps in section 4.2.1 Description of the preprocessing layers) rather than the choice of the fields used as inputs. In other words, what is critical is the data augmentation set-up to enhance the relatively small dataset.

We actually show in the results section that the addition of NO2 or not has a small impact, and that winds are not critical input fields (although they help in the inversion).

The paper of Kumar et al. 2023 is interesting and we have added a citation in the manuscript. But this is a work on methane (which does not have the low signal-to-noise ratio issue of CO2 satellite images, among other important differences) and at a much higher resolution (1.5m/pixel). We have favored citation of works on CO2, preferably at similar resolutions (CO2M).

We have also added a citation in the introduction to (Santaren et al., 2024) which analyses CO2 plume inversion methods (Cross sectional fluxes, Gaussian, ...) on the SMARTCARB dataset.

Please also note that 4 citations had been added during the first review round (rather than 2).

– *2.1: If the goal of this paper is to train a single CNN for all power plants, I recommend stating this clearly early on in the manuscript. The fact that the authors removed some power plants from the training set to improve model performance in the two cases where the model was not as good a fit should be clearly identified in the manuscript by the authors as a point of caution for future model developers aiming to produce a more generalizable model. It will not be possible to enforce this type of balance in training sets for a model that works for all (or most) power plants and/or cities.*

The goal of this paper is to prove that training a (single) CNN for all power plants is worthwhile: the focus is on the strategy rather than on the final model. We agree with the need for caution, and we have incorporated a statement to this effect in the Discussions and Limitations section: "Secondly, the study emphasises the importance of a balanced dataset, as highlighted by the section .., and the need to be able to identify and address potential overfitting issues."

Furthermore, we have modified a sentence in the introduction to state more clearly the objective: "In this paper, we develop a strategy employing convolutional neural networks (CNN) to estimate the emission fluxes from a plume in a pseudo XCO_2 image. "

Finally, in the last revision, the sentence "The objective behind creating these models is to demonstrate the effectiveness of the architectural framework, laying the groundwork for a universal model based on this architecture and capable of generalising across future PPs." in section "Geographical separation between the training and test datasets" has been added to make the objective clear.

- *2.2: Please add text to the manuscript to address the point on the existence of continuous emissions monitoring systems in the US and probably the EU, or indicate where in the manuscript it is already addressed. I think this point is worth highlighting, since these remote sensing systems will likely add the most value outside the US and EU.*

  It is not clear to us how the paper (Cusworth et al., 2021) (mentioned in the previous review round) provides a continuous emissions monitoring system. The estimations of (Cusworth et al., 2021) are estimations from other types of observations that those obtained with CO2M. Furthermore, in the abstract of (Cusworth et al., 2021), it is mentioned: "We highlight four examples of coal-fired power plants in India, Poland, and South Korea, where we quantify significant carbon dioxide emissions from power plants where limited public emissions data exist" so this paper does not target specifically the US or the EU.

  To cite this interesting paper, we have added the following text in the introduction: "and complement other estimations (Cusworth et al., 2021)."

- *4. Please include language in the manuscript summarizing the points in this response about other CO2-sensing satellites.*

  From our answer in the previous review round, the main points were:

  1. CO2M aims to provide more comprehensive plume imagery than OCO-2 or OCO-3, potentially allowing for more accurate estimations.
  2. The methodology discussed relies specifically on the type of imagery CO2M will provide, which differs from that of OCO-2 or OCO-3.
  3. The research demonstrates that the proposed approach outperforms other methods, like CSF, on idealised images.
  4. OCO-2 focuses more on natural CO2 sources and sinks, while CO2M targets anthropogenic emissions from point sources.
  5. The study primarily focuses on CO2M, which aligns with the scales of interest, unlike other high-resolution over small image areas projects.
  6. Adapting the approach for finer scales would require computationally expensive Large-Eddy Simulation (LES) models and might necessitate reducing the number of simulated cases.

  Points 1-4 are already addressed in the introduction or conclusion. We have added a mention to points 5 and 6 in the "Discussions and Limitations" section: "Finally, the method could be modified to extract CO2 emissions from plume imagery at more detailed scales, necessitating the use of resource-intensive Large-Eddy Simulation (LES) models."

– *7. Please include discussion in the main manuscript along the lines of your response on the absence of zeros in the datasets, the possibility of false positives, and your assessment of the implications of this. Old Figure 2 (different figure number now): Please clarify in the figure caption that these are results from the test dataset. Old L183: Where in the manuscript were these details added? Would be helpful to reproduce the text in the response document. The new organization is now easier to follow. Thank you.*

  In section "Discussions and limitations", we have included: "A last limitation is the absence of zero-emission source in the dataset. However, the inclusion in the training dataset of very low emission power plants and of a plume scaling approach generating near-zero emission plumes indicates that incorporating zero emission cases would likely not markedly change the outcomes."

  Clarifications on figure caption (old figure 2) have been added in the figure caption.

  It was added at line 191 (new). "The chosen model takes 3 to 4 images of 64×64 pixels as input (which correspond to the XCO_2 field and ancillary data such as the winds)."

– *Old Figure 9: Please explain this to the reader in the manuscript. Also, please clarify in this figure that units are in %. Timing of hyperparameter value selection: Please summarize these points explicitly in the main manuscript*

  We are not entirely sure what should be explained from Old figure 9 in the manuscript: the old Figure 9 is now included in the "new" Figure 7. This figure is described and analysed in the manuscript (the whole text of section 5.1 describes Figure 7 and the following table). In Figure 7, only left column is in percents, which is indicated.

  The timing of hyperparameter value selection is now mentioned in section "Geographical separation between the training and test datasets": "(or the hyperparameter selection such as the learning rate that were selected prior to the training of the models)".

– *Old Figure 13: Please mention in the main manuscript that the modeling approach applied requires that CO2 plumes be located at the center of an image, and include a sentence or two of commentary summarizing the points you make in this response.*

  We have added the following text: "In conclusion, the model consistently identifies the target emission plume situated at the image's centre, indicating it implicitly understands the relationship between the plume and targeted emissions. " in section "Gradient-based study of the pixels".

  To indicate that we work on CO2 plumes located at the center of an image, we have modified a sentence in the section "Inversion based on supervised learning" to make it "The inverse problem addressed here is the estimation of the CO_2 emissions accountable for the central hotspot plume observed in a given XCO_2 field image."

Again, many thanks to the Reviewer and the Editor for their time.

Report 2

No additional comments

References

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Santaren, D., Hakkarainen, J., Kuhlmann, G., Koene, E., Chevallier, F., Ialongo, I., Lindqvist, H., Nurmela, J., Tamminen, J., Amoros, L., Brunner, D., and Broquet, G.: Benchmarking data-driven inversion methods for the estimation of local CO_2 emissions from XCO_2 and NO_2 satellite images, Atmospheric Measurement Techniques Discussions, pp. 1–52, https://doi.org/10.5194/amt-2023-241, publisher: Copernicus GmbH, 2024.