Response to Reviewers' comments

We would like to thank the editor and reviewers for the in-depth comments that contributed to improving the presentation of our paper. Both reviewers pointed out the lack of quantitative results for validation and asked for statistics (error and correlation), which we agree and have included in the revised manuscript. Besides, the main work has been carried out to address the main concerns of all the reviewers:

1. Ensemble simulation of October 16th to see the spread and mean of flux profile in a series of turbulence realisations. For results and corresponding discussion, please see the new section 4.5: Ensemble simulation of a representative validation case;
2. Grid sensitivity study on an ideal convective boundary layer case over a reduced 2km by 2km domain in the study area. We hope the additional results can improve the rigorousness of the study. For results and corresponding discussion, please see the new section 4.4: Grid sensitivity study;
3. The new section 3.4 Diagnostic flux averaging configuration, section 3.5 BT Tower Eddy-Covariance Measurements and Source Area Characteristics and section 3.6 Evaluation of biogenic module, have been added in the revised version;
4. Additional Appendices: A-Sectors and subsectors in emission inventory, B-Results for all months, C- Additional analysis for July 16th, D- EC flux processing procedure, E-Additional information on grid sensitivity study and F- VPRM parameters. G -Diurnal cycles of surface CO2 concentration

We removed "PALM-CO2(v01)" from the title due to a suggestion from one PALM developer. They recommended us to merge our implementation to the main PALM version and not keep a stand-alone version. We think this is a good idea! However, for the purpose of this paper, the name PALM-CO2 is kept in the main text. We will add a note to the Zenodo link when the merging is finished.

We address the reviewers' comments point by point in the following document.

RC1: 'Comment on egusphere-2026-970', Anonymous Referee #1, 07 Apr 2026

General comment:

RC1.1 Thank you to the authors for this important work. The implementation of a model of biogenic fluxes in PALM is truly of high value as it opens the possibility for a lot of interesting LES studies that would not be possible without. The additional inclusion of anthropogenic fluxes based on inventories enables the application to urban areas. The implementation itself seems physically consistent and user-friendly as the application is well-designed and can be easily adapted for use in other studies, depending on data availability. The PALM is a free open-source model and the code extension for PALM-CO2 is published on Zenodo so that everyone can access it.

Overall, the manuscript is well structured and easy to follow. However, I'm missing a subchapter on the measurements used for validation in the methods section and the results and discussion section could be a little bit more detailed. There is still significant room for improvement in the validation. I do not find the choice of station for validating biogenic fluxes in the urban LES to be particularly well-suited; however, the authors have made an effort to improve comparability by analyzing the input variables. Nevertheless, the comparison is rather qualitative and a more detailed statistical analysis would be desirable. Furthermore, I think the use ensemble simulations would enhance the quality of the validation and I would like to see a more critical examination of the grid spacing used.

All in all, I strongly support the publication of this manuscript! I hope my comments will help improve it, and I wish the authors every success in revising the manuscript.

Thanks for your comments.

Specific comments:

RC1.2 Line 2-3: It says here that the model produces anthropogenic and biogenic carbon emissions and is based on a biogenic carbon emission module (VPRM). This leaves me wondering how anthropogenic carbon emissions were included.

Response: The sentence has been broken into three sentences to avoid confusion.

"*The PALM-CO2 model is implemented within the open-source urban-flow large-eddy simulation (LES) model PALM. Anthropogenic CO2 emissions are prescribed using an external emission inventory, while biogenic CO2 fluxes are 5 computed online using the Vegetation Photosynthesis and Respiration Model (VPRM). In addition, custom output modules are developed to diagnose carbon fluxes.*"

RC1.3 Line 5-7: Was a grid sensitivity study performed to show that a 10 m resolution is sufficient? I wonder if it actually is, as 10 m does not resolve trees or even smaller vegetation, and also street canyons and buildings are probably also not fully resolved (Camden has a lot of small streets with 2-storey houses that are not even 10 m tall).

Response: To address this issue, a grid sensitivity study and corresponding discussion below are included the section 4.4 in this revision (Page 25, line 542ff). The grid sensitivity study used an ideal convective boundary layer case. 10-m and 5-m resolution simulation are conducted. Vertical eddy-covariance fluxes are compared. It showed that under convective boundary-layer conditions, a 5 m grid resolution provides a more accurate representation of surface heterogeneity and turbulent transport within the lower urban canopy and roughness sublayer (up to approximately three roughness heights). Above this region, however, turbulent transport of passive scalars is already well resolved at a 10 m grid resolution. Additional details of the grid-sensitivity experiment and model configuration are provided in the Appendix D.

RC1.4 Line 11: "building-induced turbulence": I assumed this sentence refers to the forest simulations in Czech Republic. Were there buildings in that forest? Or is this the conclusion for both validation cases? Was the resolution of the Czech forest simulation fine enough to resolve building-induced turbulence?

Response: We apologize for the ambiguity. No simulations were conducted for the Czech forest site. "*The biogenic flux module is evaluated independently using observed monthly diurnal profiles of biogenic CO_2 fluxes from a deciduous forest site in the Czech Republic*." - This has been clarified in the abstract and introduction.

RC1.5 Line 75-82: If I understand correctly, the anthropogenic fluxes are included as prescribed fluxes based on emission inventories. How is this method different from / does it overcome the problems highlighted in lines 34 to 41? Is the novelty concerning anthropogenic carbon fluxes mainly combining those inventories with a high-resolution LES? It could be briefly highlighted here what the novelty is. I believe including spatially and temporally resolved scalar surface fluxes is something that is not possible by default in PALM. This is worth mentioning here.

Response: We thank the reviewer for this comment. We agree that anthropogenic CO_2 emissions are prescribed as surface scalar flux boundary conditions in PALM, which is a standard approach and does not, by itself, represent a methodological novelty. The challenge highlighted in Lines 34–41 is the lack of anthropogenic emission inventories at high-spatial resolutions compatible with large-eddy simulations (LES), which limits the representation of fine-scale urban emission heterogeneity. The novelty of this study lies in overcoming this limitation through the development of a new 10-m anthropogenic CO_2 emission inventory derived from an existing 1-km inventory using multiple spatial proxies. This high-resolution inventory is then combined with high-resolution biogenic CO_2 fluxes and implemented as spatially and temporally varying surface scalar fluxes within PALM LES. The resulting framework enables the representation of neighbourhood-scale variability in both anthropogenic emissions and biogenic uptake, which cannot be achieved using coarse-resolution inventories alone. We have clarified the novelty in the revised version (line 85 - 89).

RC1.6 Line 121/Figure 1: Is the anthropogenic flux prescribed at the surface or also within the boundary layer? Also, is it an extra input file? Why is it not added to the dynamic driver?

Response: The anthropogenic flux is prescribed at the surface in a chemistry input file. This is an additional input file along with the dynamic driver. This information has been added in Figure 1.

RC1.7 Line 123-125: Could you please include the equations for P_scale and W_scale?

Response: These have been added in this revision – see Eqs. (3) and (5).

RC1.8 Line 147: Equation 9, claims that spatially filtered quantities are identical with quantities at infinitely small points in space. I think this assumption may be true in certain cases, depending on the time interval for averaging and grid spacing, but it is not generally the case. Please include the information under which circumstances this assumption holds.

Response: The assumptions have been stated in the revised manuscript (lines 166- 176):

"A major assumption is the omission of the sub-grid scale turbulent flux, which requires the grid size to be small enough to resolve the total turbulent flux. It is shown in the grid sensitivity study that the resolved flux at medium height boundary layer (higher than three roughness-layer heights) takes over 96% of the total turbulent flux. Thus, the assumption is valid. A minor assumption is that the spatial filtering and time averaging commute and that the filter preserves the mean, such as $\langle\cdot\rangle = \langle\bar{\cdot}\rangle$*. This assumption requires the averaging window is long enough to converge the statistics at all spatial locations. A computation of eddy turnover time (about 450 s for the grid sensitivity study case) shows that a half-hour average window (1800 s) covers about four eddy turnover periods, validating this minor assumption. The temporal averaging of* $\overline{w}\,\overline{s}$ *is added to the code via a user-defined procedure. Its rolling sum is computed during time-stepping in the code, and its temporal average* $\langle ws\rangle$ *is computed and reported after each averaging time window* τ*.* $\langle w\rangle$ *and* $\langle s\rangle$ *can be easily recorded using the default output settings in PALM. The time window for averaging is set as 0.5 hour."*

RC1.9 Line 149-152: I appreciate the effort, but isn't that the same as ws_product_av in the default PALM 3D output quantities?

Response: To our knowledge, the default output for ws_product_av is not defined for the chemistry module in PALM (product of w and kc_<component_name>) where we implement the CO_2 components (emission inputs and biogenic emission module). It might be possible to implement everything as an independent passive scalar to use the ws_product option, but we didn't do that.

RC1.10 Line 179-181: Does the VPRM also consider water availability (soil moisture relative to soil characteristics such as wilting point) and VPD? In line 125, it says that W_scale accounts for the water content in vegetation. Is this coupled with the soil moisture development in PALM or does it rely solely on input data? Considering Figure 1, I assume the latter is true. In that case, where would one get the necessary information on a sufficient temporal and spatial resolution?

Response: The water availability is solely determined by the LSWI computed from the satellite multi-spectral image dataset. There is no diurnal variation of water availability in the current implementation. It is a very good idea to couple the biogenic emission module with the soil moisture computed in PALM. We can consider this in the future.

RC1.11 Line 225: Which are the sectors that contribute to less than 10% of the emissions? Isn't it possible that those contributions are small because they are only very local/short term?

Response: The (sub-sub-)sectors that contribute to less than 10% of the total emissions are:
- Domestic Oil/Coal combustion
- Domestic Household and Garden NRMM
- Industrial Oil/Coal combustion
- Industrial Construction NRMM
- Industrial Natural Gas Leakage
- Industrial Process NRMM
- Industrial Process Part A2 / B (small industries)
- Agriculture
- Transport – Aviation
- Rail transport
- River transport

Most of these sectors should be well spread over space, such as Domestic Oil/Coal combustion and Garden NRMM, Aviation, Rail and River transport. While some of these sectors might be local/short term (such as those emissions associated with industrial activities), it is difficult to find additional spatial proxies to further disaggregate them.

A table listing the sectors and their emission proportions is included in Appendix A for reference.

RC1.12 Line 225-231: Is the 10% threshold applied to the sectors or sub-sectors? This is not fully clear in lines 225-228, please specify.

Response: It is applied to sub-subsectors. The sub-subsectors have been processed to merge small sub-subsectors together and to remove those with zero emissions. A list of the sub-subsectors and their emission proportions is included in Appendix A.

RC1.13 Line 239: Here, it says “sectors” again, but I assume it refers to the sub-sectors mentioned earlier. Please use the words “sectors” and “sub-sectors” consistently throughout the manuscript to avoid confusion.

Response: We have corrected the wordings to always use “sub-subsectors” when appropriate. Hopefully with the additional table in Appendix A, it would be clear.

RC1.14 Line 279: Is this supposed to be 10% (because it says similarly as in spatial disaggregation) or is the threshold different from the spatial disaggregation? Why was a different threshold chosen here?

Response: A different threshold 1% is selected for temporal disaggregation. The only additional sub-subsectors (between 1% and 10%) are: Industrial Oil/Coal Combustion, Industrial Construction NRMM, and Transport – Aviation. There are temporal profiles for these sub-subsectors in CAMS-TEMPO and EDGAR, so we decide to include them in temporal disaggregation as well.

This information has been added to main text Section 3.2.2 (lines 314-317).

RC1.15 Line 283-291: I believe this approach is acceptable for the phenology, but water stress is much more variable in time. One possibility to overcome the lack of high temporal resolution data would be to couple the VPRM also with the soil moisture water vapor mixing ratio (to calculate VPD) in PALM and not only with radiation and air temperature. Have you considered that?

Response: Thanks again for the suggestion. We didn’t consider that in the current development. We think it is totally possible to access soil moisture in the biogenic emission module and use it to compute water stress. It requires more validation though. It could be our future work.

RC1.16 Line 299-300: Please see my comment on line149-152.

Response: Please see our response for comments **RC1.8** and **RC1.9**.

RC1.17 Line 305: It would also be possible to prescribe leaf area density and thus resolve the canopy (at least trees). Of course, with a resolution of 10 m^3, this is not possible. However, as pointed out in my comment on line 5-7, I wonder whether the resolution in this study is sufficient to resolve urban flow structures, as trees and buildings (at least in some areas) are not properly resolved. Why was this resolution chosen? Was a sensitivity study performed?

Response: It is a good idea to include leaf area density as a more detailed parameter for the biogenic emission module. It could be our future work to further utilise the parametrisations of urban processes already developed in PALM.

For grid resolution and sensitivity study, please see our response for comment **RC1.3**.

RC1.18 Line 342-349: this part belongs in the methods section. A short sub-section on the EC measurements could be included in chapter 3. It should also include a short description of the station location and its surroundings. What types of buildings/parks/roads are within the footprint of the station? How tall is the tower in comparison to the buildings in the area? The location of the EC station could also be marked in Figure 2b.

Response: This paragraph has been moved to *Section 3.5 BT Tower Eddy-Covariance Measurements and Source Area Characteristics* (line 358 – 364). Additional description of the station location and its surrounding land-use and building heights is included (line 365 – 382). The site location is marked in Figure 2(b).

"*The measurement site is in Camden, London. An eddy-covariance flux tower was set up on the BT tower in Camden by previous researchers (Wood et al., 2010; Helfter et al., 2011). It collected the vertical CO2 flux (Helfter et al., 2011), turbulent flow quantities (Wood et al., 2010) and other meteorological and greenhouse gas variables. We used the measurements in 2019 to validate our CO2 model. Specifically, the wind speed and the eddy-covariance CO2 flux were used in the validation. The temporal interval of measurements was half an hour. The CO2 flux is computed via the procedure described in Section 2.5. In the following, we provide some essential information on the measurement site and its surrounding areas. For more details, the reader is referred to Helfter et al. (2016).*

The eddy-covariance (EC) measurements of CO2 were conducted from the BT Tower in central London (51◦31'17.4'' N, 0◦08'20.0'' W). The EC system was installed on a lattice structure mounted on the roof of the telecommunications tower, providing an effective measurement height of approximately 190 m above street level. The system consisted of a three-dimensional sonic anemometer and a closed-path cavity ring-down spectrometer measuring CO2, CH4 and H2O, with air sampled through a 45 m inlet line. Owing to the unusually large measurement height, the footprint of the observations extended over several kilometres and integrated emissions from a substantial portion of central London.

The BT Tower is located within a densely urbanized area of central London characterized by a mixture of commercial, residential and transport infrastructure. Within a radius of approximately 10 km, the mean building height is 8.8 ± 3.0 m, while typical suburban building heights are 5.6 ± 1.8 m. Consequently, the 190 m measurement height is more than an order of magnitude greater than the average surrounding building height, placing the EC sensors well above the urban roughness sublayer and enabling observation of integrated fluxes from a large urban source area.

Footprint analyses indicate that the source area varies seasonally from a few kilometres under convective conditions to several tens of kilometres during wintertime stable conditions. The footprint encompasses a diverse range of land uses. To the southwest and northwest, the source area includes two major urban parks, Hyde Park (142 ha) and Regent's Park (197 ha), respectively. Northern sectors are dominated by suburban residential neighbourhoods, whereas eastern and southern sectors contain a mixture of densely built residential and commercial districts. The southeastern footprint also includes part of the River Thames. As a result, the measured CO2 fluxes represent the integrated influence of emissions from road traffic, residential and commercial energy use, and human activities across a heterogeneous urban landscape."

RC1.19 Line 350-366: The analysis of CO2 fluxes and wind speed is limited to four days, only, but simulations were performed for each month. Were the results of the other 8 days similar or different? Why weren't they included in the plots (or at least similar plots in the appendix?). Also, have you performed some sort of statistical analysis, e.g., looked at the correlation between field measurements and LES output across all simulated days?

Response: Months 1, 4, 7, and 10 were selected to represent winter, spring, summer, and autumn conditions, respectively. These months illustrate the range of model performance observed throughout the year, including both high-skill and low-skill cases. Performance metrics for all available months are provided in Table B1.

Texts for validation are tweaked to include the newly added statistics.

Additional simulations and corresponding statistical analysis have been conducted and added in Appendix B. Specifically, we did the following simulations in this revision:

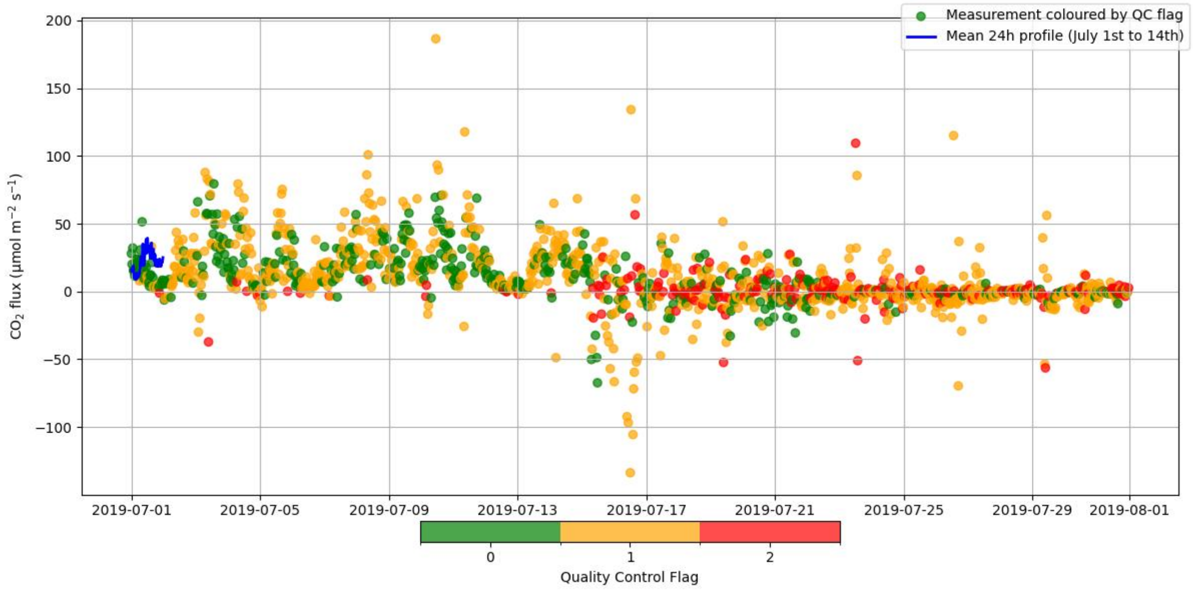
- To consolidate results, we reran all the days with virtual measurement at the same location as BT tower measurement and activated synthetic turbulence generator. For details, see our response for comments **RC1.21** (1).
- To explain the over-estimation of EC flux in the simulation for July 16th, we selected another day in July (20th) to run the model. For details, see our response below for comments on **RC1.20**).
- To see the spread of different turbulence realisations, we ran October 16th for ten more times as an ensemble (see our response for RC1.21 (4)).

We include the following new materials in this revision:

- Statistics (RMSE, bias and correlation coefficient) for EC flux, wind speed, and wind direction (selected months in main text Fig. 5 subtitles, all months in Appendix B Table B1).
- Plots of EC flux, wind speed and wind direction's 24h profiles for all months in Appendix B Figure B1 to B3.
- A comparison of EC flux computed with Equation (12) and with virtual measurement in Appendix C Figure C1.

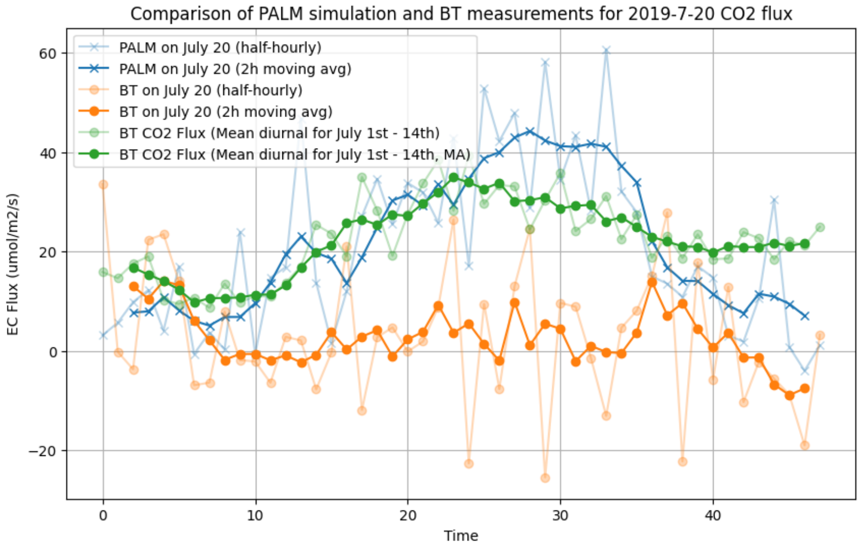
RC1.20 Line 356-358: Why are you so sure that this discrepancy is due to the observation data (=EC measurements?) and not because of the LES? To my understanding, it could also be that this day in reality was different from the average day and the temporal disaggregation of the LES input data doesn't capture this variability, in which case it would be a limitation of the approach. Carefully checking the quality of the EC measurements on that day and also checking how well the meteorological conditions of that day match between field measurements and LES could provide more insight here. It could also be, e.g., a mismatch of footprints due to non-matching wind directions or so. Also, domestic gas consumption is the largest contributor to CO2 emissions according to figure 4. Shouldn't the CO2 emissions be lower in summer due to reduced heating demand? The CO2 fluxes produced by the LES are higher in July than in January or October.

Response: The processed EC flux data in the BT tower dataset have gone through quality control. The quality control checks several criteria including the magnitude of turbulence intensity, number of valid instantaneous measurements, out-of-range velocity ratios and range of heat fluxes. A quality control flag (qc) is provided in the dataset (Helfter et al. 2016). A qc value less than 2 is deemed good data. The following plot shows the BT measurement EC flux in 2019-07 and their individual qc flag. It can be seen that from 07-16, quality of data deteriorated and more data are with qc flag = 2. The plot also shows the average 24hr EC flux profile for the first two weeks of July (1st to 14th) as a blue line in the first day. This average profile is very different to that on July 16th. Therefore, it is likely the measurements on July 16th are unfortunately not representative of the real situation.

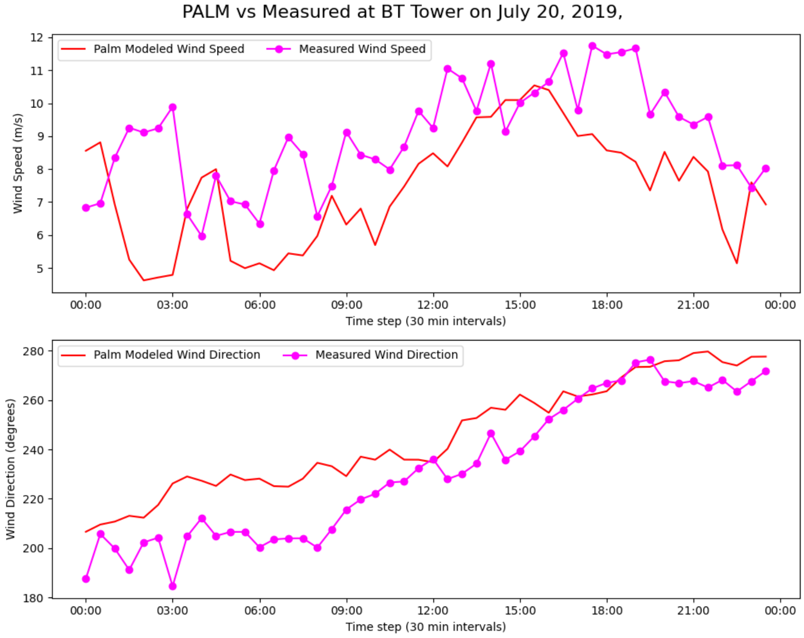


This doesn't explain why the July simulation is larger than that in January or October though – as anthropogenic emission is indeed smaller in July. We think that the wind speed on July 16th contributed to this over-estimation. As can be seen in Figure 6 in main text, wind speed from 8:00 to 18:00 is very low even at 190m height. At lower ground the wind speed would be even smaller. The worse convection condition will likely cause the build-up of flux in the domain that would otherwise be transported horizontally out of the domain. Eventually this caused an over-estimation.

To further prove this point, we select another day in July (the 20th) with moderate wind magnitude. This day has similar anthropogenic emission to July 16th. However, the simulated EC flux (blue lines in figure) is smaller, and smaller than that in January and October. Comparison with observation is unreliable for July 20th due to bad data quality (orange lines in figure). However, if the average 24hr profile for the first two weeks of July (green lines in figure) is used in the comparison, the July 20th simulation is in good agreement with measurements (RMSE =8.28 umol m^{-2} s^{-1}, r = 0.819).



The wind speed and direction for July 20th is plotted below:



RC1.21 Figure 5: I have multiple questions/comments on how the data for this figure was processed. It may be good to have a sub-chapter in the methods section on the post-processing of LES and EC data and the validation approach. (1) How was the moving mean calculated from PALM, if only the 30-min averaged values of CO2 and w were output? (2) Did you have the high-resolution data from the EC station to calculate the running mean for the measurements? (3) Is the LES data extracted from one grid box in the same location as the EC station in the field? (4) This is the most important one: the figure shows one realization of each day from the measurements and one realization of the day from the LES. For the measurements, this is obvious, it's simply how these particular days developed. However, turbulence is chaotic in nature and even a slight change to the environment or initial conditions can change the outcome severely. It is impossible to precisely reproduce the exact same day, so it cannot be expected that the LES and field observations match perfectly. However, performing ensemble simulations with slightly changed turbulence using different random numbers provides multiple realizations in the LES. This can result in a better understanding of what range of values may result from the LES and whether the field measurements fall within that range. Performing ensemble simulations for the validations would strongly improve the quality of this manuscript.

Response:

(1) The moving average is computed by taking four half-hourly EC fluxes and compute their mean. The timestamp for the moving average is at the end of the 2-hr window. EC fluxes are always computed using 30-min averaging intervals.
To further validate the temporal approach, we also include EC fluxes computed with time series in this revision (Appendix C). A description of two methods to obtain half-hourly EC fluxes from simulation and the 2-hr moving average used in computing the metrics (RSME and correlation coefficient) is added to a new Subsection 3.4 (Line 333ff).

(2) Unfortunately, we don't have the high-resolution time series from EC station. The EC fluxes have been provided at half-hourly averaging window.

(3) Yes. The EC flux in LES is extracted from one grid box in the same location as the EC station in the field.

(4) We agree that LES is only one realisation of the turbulent flow. We thank the reviewer for pointing out the difficulty in precisely reproducing the exact flow and EC flux profiles. To show the spread and mean of the simulation, we did additional ensemble simulation for October 16th. The additional results are included in a new section 4.5 in this revision. Overall, we found that the ensemble mean captures the main diurnal EC flux variation similar to that of individual realisations. At the same time, differences remain between the modelled and observed fluxes. The residual mismatches could be due to uncertainties in the emission profiles of the particular day.
Due to limitation of computational resources, we didn't do ensemble simulation for all months.

RC1.22 Line 361-366: To validate the overall comparability of the simulation and the real situation, more variables could be considered in addition to wind speed. E.g., wind direction, temperature, humidity.

Response: Additional comparison on wind direction is included in this revision in Figure 7 and Figure B3.

RC1.23 Line 367ff: If I understand it correctly, the carbon fluxes from Regent Park forest areas (LES) are compared to field measurements from a forest site in Czech Republic. I think this approach is questionable and not described in enough detail. Why was this station (CZ-Lnz) chosen for comparison? Why is it expected that forests in a London park behave similar to a forest in Czech Republic? Those questions should be answered in the methods section. CZ-Lnz is installed in a floodplain forest in a rural area in south-eastern Czech Republic. London has an oceanic climate with roughly 700 mm annual precipitation and an average temperature of roughly 11 °C. According to the ICOS website, CZ-Lnz has an annual precipitation of 520 mm and average temperature of 10 °C, and it has a continental climate with much lower winter temperatures and higher summer temperatures and also a different precipitation distribution. The soil is likely also very different. I wouldn't consider this a benchmark for an urban biogenic CO2 flux model in London. Even though it requires more computational resources, I think it is necessary to perform an extra set of LES for a vegetated site where measurements are available. It would not be necessary for this to be an urban site.

Response: We thank the reviewer for raising this point. We agree that the rationale for including CZ-Lnz was not sufficiently explained and have revised the manuscript accordingly. We have tweaked the first paragraph of this subsection to tone down the purpose of this comparison (validation -> evaluation):

"*4.2 Evaluation of biogenic fluxes*
Comparison with the BT tower measurements provides the primary validation of the modelling framework in the study area. However, CO2 fluxes measured at the BT tower are dominated by anthropogenic emissions, making it difficult to directly assess the realism of the vegetation component of the model. Here, we compare biogenic flux with two independent sources in the literature to evaluate the biogenic fluxes in PALM-CO2. To provide an independent evaluation of the implemented VPRM scheme, we first compare the simulated biogenic fluxes against eddy-covariance observations from a European forest site. The purpose of this comparison is not to perform a sitespecific validation of vegetation in Regent's Park, but rather to assess whether the model produces realistic magnitudes, seasonal variations, and diurnal cycles of ecosystem carbon exchange."

We also included a new subsection “*3.6 Evaluation of biogenic module*” to describe the purpose and method of this comparison.

Our primary model validation is performed using observations from the London site, which directly represents the urban environment targeted in this study. The comparison with CZ-Lnz was included for a different purpose. Because CO_2 fluxes at the London site are dominated by anthropogenic emissions, they provide limited information on whether the vegetation parameterization produces realistic biogenic fluxes. The CZ-Lnz comparison was therefore intended as an independent assessment of the simulated vegetation-driven carbon exchange, allowing us to evaluate whether the magnitude and seasonal evolution of the modelled biogenic fluxes are physically reasonable when compared with eddy-covariance measurements from a well-characterized forest ecosystem.

We agree that CZ-Lnz is not climatically or ecologically equivalent to Regent's Park and do not intend it to serve as a site-specific validation of urban vegetation in London. We have revised the manuscript to make this distinction explicit and to discuss the limitations associated with this comparison. The purpose of the analysis is to provide confidence that the biogenic flux module produces realistic carbon fluxes, rather than to demonstrate direct agreement between two ecologically similar sites.

Regarding the suggestion to perform an additional LES simulation for a vegetated site with available measurements, we believe this would constitute a substantial extension of the present study. The objective of this work is to evaluate urban CO_2 fluxes and transport processes, for which the London site provides the primary validation dataset. Since the CZ-Lnz analysis is included only as an auxiliary evaluation of the biogenic flux component, we consider that clarifying its purpose and limitations is a more appropriate revision than introducing a new LES case study.

RC1.24 Line 374: To my knowledge, ICOS has 48 class 1 sites and a lot more class 2 and associated sites.

Response: We have removed this description.

RC1.25 Line 274: I’m not sure what “situated in a naturally-surrounding town” means. It is clearly not located in a town, the distance to the next village is over 4 km.

Response: This has been corrected. A different description of the site is included in this revision: (line 392 – 395)

“*CZ-Lnz site is located in a deciduous floodplain forest and its eddy-covariance measurements are primarily influenced by local biogenic carbon exchange. Although the climatic and ecological conditions differ from those in London, CZ-Lnz provides a long-term, high-quality record of net ecosystem exchange (NEE) from a deciduous forest ecosystem, allowing a qualitative assessment of the VPRM representation of forest carbon uptake and respiration*.”

RC1.26 Line 376-378: So here, the surface flux provided by the VPRM is compared to field measurements at a height of ca. 50 m? And also, one single day of LES is compared to a monthly average of field measurements? I think this makes the comparison even more difficult and it is is not considered in the discussion of results. Figure 7 could at least show a standard deviation or min/max values for the field measurements to give an idea of the variability within each month.

Response: We rephrased and emphasized the purpose of this comparison – to serve as a qualitative assessment of the VPRM representation of forest carbon uptake and respiration. We removed strong claim on the results of the comparison to avoid confusion.

We have added the confidence level of CZ-Lnz site measurement in Figure 8 (corresponding to the old Figure 7) in this revision.

RC1.27 Line 386-391: I can follow the argumentation for the differences in spring. However, in autumn, the EVI in Camden decreases but in August and September, it is still higher than at Cz-Lnz, so how does this explain the smaller carbon uptake?

Response: Thank you for pointing out this. We have removed the analysis using EVI for late summer and early Autumn and replace with the explanation below. Admittedly, your previous comments on coupling the biogenic model with soil moisture and canopy modelling could be the next step of improving and validating the biogenic module.

"*The stronger uptake simulated during spring is consistent with the rapid increase in EVI and LSWI at the Regent's Park location. However, the differences observed during late summer and early autumn cannot be explained by EVI alone. Although EVI remains relatively high during August and September, NEE is influenced by additional environmental controls in VPRM, including temperature and moisture stress, as well as site-specific ecosystem characteristics. Furthermore, the comparison involves two ecologically distinct systems: an urban park in London and a rural floodplain forest in the Czech Republic. Differences in species composition, soil moisture conditions, forest structure, and phenological timing may therefore lead to different seasonal trajectories of carbon uptake despite similar vegetation index values. Consequently, the late-summer discrepancy is likely attributable to site-specific ecological differences rather than changes in canopy greenness alone.*"

Additional comparison to ICOS-VPRM also shows lower net NEE for August and September (Figure 10), consolidating our implementation while confirming the difficulty of this cross-site comparison.

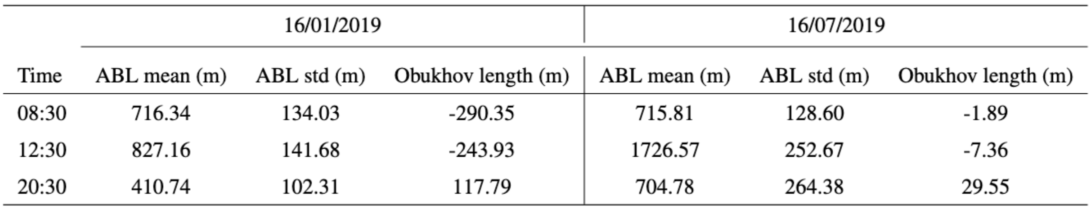
RC1.28 Line 393: Does this show LES output? Please specify here and in the description of Figure 9.

Response: Yes. This has been added to the figure subtitle.

RC1.29 Line 393ff: This section discusses the results in the context of the ABL height. However, ABL height is not characterized at all. Also, in line 400ff, the influence of strong/weak turbulence is discussed. However, this is not quantified, either. Instead, three instantaneous snapshots for two days in January and July are considered and described qualitatively. Please provide the ABL height and a measure of stability (i.e., Obukhov length L or z/L) for these selected situations to show what situations are truly reflected by those snap shots.

Response: The ABL height and Obukhov length are added to Table 3.

Table 3. ABL height statistics and Obukhov length for 16 January 2019 and 16 July 2019.



	16/01/2019			16/07/2019		
Time	ABL mean (m)	ABL std (m)	Obukhov length (m)	ABL mean (m)	ABL std (m)	Obukhov length (m)
08:30	716.34	134.03	-290.35	715.81	128.60	-1.89
12:30	827.16	141.68	-243.93	1726.57	252.67	-7.36
20:30	410.74	102.31	117.79	704.78	264.38	29.55

Additional description of ABL heights and stability are added to the analysis: (line 528 to 534)

"*The January cases are characterized by a relatively shallow boundary layer (411 – 827 m) and near-neutral to weakly unstable/stable conditions (|L| ≈100 – 300 m). In contrast, the July cases exhibit substantially stronger instability during daytime (L = -1.9 to -7.4 m), leading to enhanced turbulent mixing and the development of a deep convective boundary layer reaching 1.7 km at midday. Following sunset, both periods transition to stable stratification (L > 0), accompanied by a reduction in boundary-layer height, although the summer boundary layer remains considerably deeper than the winter boundary layer due to stronger daytime heating and residual turbulence*."

RC1.30 Line 408-410: While describing Figure 10, ABL heights are given as "high" and "low". However, it is not possible to see this in Figure 10. It could by shown by including a line in each subplot and as pointed out in the previous comment, (e.g., spatially averaged +- standard deviation) ABL heights for each snap shot should be provided. Also, it says that "in general, during morning and night, the boundary layer is shallow, trapping CO2 below approximately 300-400 m" while the ABL height is likely much lower in January than in July.

Response: Mean and standard deviation of ABL heights are listed in Table 3. You are right about ABL height difference in January and in July. We have changed the description. The ABL heights are quantified in the description.

RC1.31 Line 418: I've mentioned it before, but I still want to highlight it once more: I think this is not a fine-scale modelling of the urban topography, as a lot of buildings and street canyons in the domain cannot be resolved with this resolution and there have been urban PALM simulations with a much higher resolution of 1 m^3.

Response: We thank the reviewer for this comment. We agree that the term "fine-scale" may be misleading in the context of urban LES, where simulations with grid spacings of approximately 1 m have been used to resolve individual buildings and street-canyon processes in greater detail. Our simulation uses a 10 m grid spacing and therefore does not resolve all buildings and street canyons within the domain. To avoid overstating the model resolution, we have revised the text and replaced "fine-scale modelling" with a more precise description of the LES representation of urban morphology and spatially heterogeneous emissions. We emphasize that our conclusion concerns the benefits of explicitly representing urban-scale flow structures and emission heterogeneity rather than fully resolving all microscale urban features. Line 546 – 548:

"*The above pattern is revealed through LES that explicitly represents urban topography and spatially heterogeneous anthropogenic and biogenic emissions at a 10 m resolution. The resulting complex CO_2 dispersion pattern highlights the importance of resolving urban-scale flow structures and emission heterogeneity when simulating urban carbon transport*."

RC1.32 Line 419: Despite the previous comment, I fully agree with the authors that fine-resolution models for simulating CO2 transport in urban areas is necessary and technically, the presented implementation in PALM is capable of this. So even if I am not fully convinced by the resolution that was used in this study, the study certainly provides a proof of concept and promising first test of the model. A higher resolution would not change the boundary information provided by the VPRM and the downscaling of anthropogenic fluxes that are prescribed. It would, however, resolve turbulent transport within the complex urban canopy.

Response: We thank the reviewer for this constructive assessment. We agree that increasing the grid resolution beyond 10 m would improve the representation of turbulent transport within the urban canopy and enable a more detailed resolution of building-scale and street-canyon processes. As noted by the reviewer, however, the spatial resolution of the prescribed anthropogenic and biogenic fluxes would remain unchanged, such that the benefits of further refinement would primarily arise from a more realistic representation of transport and mixing processes.

The objective of this study was to provide a first implementation and proof-of-concept of urban CO_2 transport modelling in PALM using currently available flux products. We therefore selected a 10 m grid spacing as a compromise between resolving major urban morphological features and maintaining computational feasibility for city-scale simulations. We agree that future studies should investigate the sensitivity of simulated CO$_2$ fields to grid resolution and assess the added value of building-resolving simulations when higher-resolution emission and biosphere flux datasets become available. We have added text to the manuscript to clarify this point.

We hope the additional grid sensitivity study would provide some insights on impact of better resolved transport given input fluxes at the same resolutions.

RC1.33 Line 420: Could you please repeat the argument of Brunner et al. (2019) here?

Response: This has been added: Line 549-550

"*This conclusion echoes the argument in Brunner et al. (2019) that vertical distribution of carbon fluxes greatly affects the transport process*."

RC1.34 Line 421: To be precise, PALM is not an urban LES code, but an LES code for all kinds of applications, not only urban simulations. And as far as I understand, PALM-CO2 could also be applied outside of urban areas by setting the anthropogenic CO2 fluxes to zero and only applying the VPRM. If that is the case, I would drop the limitation to urban areas and instead mention the variety of applications for this approach. While the possibility to include anthropogenic fluxes is of course necessary and therefore highly valuable for urban simulations, it's not limited to those. It has a great potential!

Response: We agree that the LES capability would model carbon transport at better accuracy. We removed the "urban" in this sentence. The benefits of applying this to a non-urban scenario are somehow out of stretch of the current paper, as the main case study is located in a heterogeneous urban site. We decide not to expand the conclusion to avoid claiming too much.

RC1.35 Line 447-448: Since the idea PALM-CO2 is to provide a model that can resolve the urban distribution of CO2 (fluxes), it would improve the quality of the manuscript to show that this truly works. Yes, Figures 9 and 10 show a very inhomogeneous distribution of CO2 that seems reasonable, but in order to estimate how well the model performs, wouldn't it be necessary to compare at least the CO2 concentrations to measurements at multiple sites within the domain that have different characteristics and expected CO2 concentrations?

Response: This is true. Unfortunately, the BT tower site data we obtained didn't have CO2 concentration data before 2021. During our work, the most recent emission inventory data we were able to obtain was for 2019.

Technical corrections:

RC1.36 Line 17: "the carbon cycle"

Response: Done.

RC 1.37 Line 19: "the global carbon cycle"

Response: Done.

RC1.38 Line 22: "uptake" instead of "update"? (2x!)

Response: Done. Sorry.

RC1.39 Line 26: "the urban environment" or "urban environments"

Response: Done.

RC1.40 Line 27: "the energy sector produces"

Response: Done.

RC1.41 Line 37: "fine scale emission maps"

Response: Done.

RC1.42 Line 117: "the WRF-Chem module"

Response: Done.

RC1.43 Line 132: T_air is not used in equations 3 or 4. I assume this refers to T in equations 3 and 4. Please standardize.

Response: Yes. This has been corrected.

RC1.44 Line 142,151: T is here used here and in equation 7 as the time window for averaging but was used in line 127 and equations 3 and 4 as air temperature. Please make sure not to use every symbol only once. Even if T_air was used for air temperature, I think using T for something else is not ideal as T clearly refers to temperature and the subscript only further specifies it.

Response: We agree. We have changed time window to τ.

RC1.45 Line 211: "the London case study"

Response: Done.

RC1.46 Figure 2: I think it would be easier to see the domains in both subplots if the lines were different from the coordinate grids (different color or thicker lines).

Response: This has been changed. The lines for domains are thicker now.

RC1.47 Line 223: “comprehensive” instead of “comprehend”?

Response: Done.

RC1.48 Line 244, 256,257: the abbreviations LSOA and MSOA are reintroduced here, which is unnecessary as they were introduced in line 240 already.

Response: The short descriptions of LSOA and MSOA have been moved to the beginning of this subsection: Line 272 – 276

“*Each LSOA is a statistical area covering 400 to 1,200 households and has 1,000 to 3,000 residents, while each MSOA contains around 5,000-15,000 residents or 2,000-6,000 households.*”

After that only abbreviations are used.

RC1.49 Line 275: “a waste water processing factory” or “waste water processing factories”?

Response: We have corrected it to “wastewater processing factories”.

RC1.50 Line 314: “we activated the following surface modules. An urban surface module” (otherwise, urban surface module is a sub-category of urban surface modules)

Response: Yes, we agree. We have corrected this.

RC1.51 Line 319: “the ECMWF reanalysis dataset”

Response: Done.

RC1.52 Line 406: “vertical cross-section”

Response: Done.

RC1.53 Line 407: “ABL height/structure/characteristics”?

Response: We changed it to ABL height and stability and added ABL height and stability analysis to the start of this paragraph (line 528 – 534).

RC1.54 Figure 10: The panels and all labels are very small. Stacking all panels vertically (1 column, 6 rows) would improve this.

Response: This is changed in this revision.

RC1.55 Line 440: remove “shows”

Response: Done.

Citation: https://doi.org/10.5194/egusphere-2026-970-RC1

RC2: 'Comment on egusphere-2026-970', Anonymous Referee #2, 02 May 2026

General Comments

RC2.1 This paper by Li et al., titled "PALM-CO2 (v01): A High-Resolution Urban CO_2 Transport Model with Anthropogenic and Biogenic Fluxes," addresses a highly relevant and timely topic. High-resolution simulations are essential for improving our understanding of complex and heterogeneous urban environments, where spatial variability in emissions and processes plays a crucial role.

However, several important aspects need improvement before the manuscript can be considered for publication. In particular, some key information is missing, and the authors often make statements without supporting statistical analyses, relying solely on qualitative interpretation of figures. The quality and clarity of several figures should also be improved. Additionally, the manuscript contains typos and lacks sufficient bibliographic references in multiple sections, which weakens the scientific rigor of the work.

Overall, I support the publication of this manuscript, provided that the authors address the points raised below through a thorough revision.

Response: Thanks for your comments.

Specific Comments

RC2.2 I recommend standardizing the terminology used throughout the manuscript when referring to anthropogenic emissions and biogenic fluxes. Avoid using the term emissions when referring to biogenic processes; use fluxes consistently for clarity.

Response: We agree. This has been corrected. All instances where biogenic fluxes are referred to have been changed to fluxes in this revision.

RC2.3 Line 3: The sentence "...a biogenic carbon emissions module (VPRM) and customised output modules for carbon fluxes..." is confusing. The description should clarify that the biogenic flux module (VPRM) is implemented online, while anthropogenic emissions are prescribed as customized inputs.

Response: This has been corrected as follows: (line 3 – 4)

> *"Anthropogenic CO_2 emissions are prescribed using an external emission inventory, while biogenic CO_2 fluxes are computed online using the Vegetation Photosynthesis and Respiration Model (VPRM)."*

RC2.4 The authors make several assertions without supporting references (e.g., Lines 16, 42, 63). Please include appropriate citations.

Response: Relevant references have been added to support the argument on Lines 16, 42, 63:

Line 16 (now line 20): Intergovernmental Panel on Climate Change: Climate Change 2023: Synthesis Report. Contribution of Working Groups I, II and III to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change, https://doi.org/10.59327/IPCC/AR6-9789291691647, 2023

Line 42 (now line 48): Friedlingstein et al. Global Carbon Budget 2023, Earth System Science Data, 15, 5301–5369, https://doi.org/10.5194/essd-15-5301-2023, 2023.

Line 63 (now line 70):

Stroh, E., Harrie, L., and Gustafsson, S.: A study of spatial resolution in pollution exposure modelling, International journal of health geographics, 6, 19, 2007.

Li, Y., Henze, D. K., Jack, D., and Kinney, P. L.: The influence of air quality model resolution on health impact assessment for fine particulate matter and its components, Air Quality, Atmosphere & Health, 9, 51–68, 2016.

Fenech, S., Doherty, R. M., Heaviside, C., Vardoulakis, S., Macintyre, H. L., and O'Connor, F. M.: The influence of model spatial resolution on simulated ozone and fine particulate matter for Europe: implications for health impact assessments, Atmospheric Chemistry and Physics, 18, 5765–5784, 2018.

RC2.5 Section 2.2 would be better titled "Biogenic Fluxes Model" rather than "Biogenic Emission Model,".

Response: We agree. This has been corrected.

RC2.6 In this section, the authors should:

- Include and explain the equations for Wscale and Pscale.

Response: Equations for W_scale and P_scale have been added in this revision (Eqs. (3) – (5)).

RC2.7

- Provide a table summarizing the VPRM input parameters for each vegetation type used in the domain.

Response: Parameters for VPRM as well as temperature parameters for photosynthesis (Tmin, Topt, Tmax) has been added in Appendix E Table E1.

RC2.8

- Include the temperature parameters used for photosynthesis (Tmin, Topt, Tmax).

Response: Parameters for VPRM as well as temperature parameters for photosynthesis (Tmin, Topt, Tmax) has been added in Appendix E Table E1.

RC2.9 Section 2.4 describes the methodology for disaggregating anthropogenic emissions. Was this approach based on previous studies or established methodologies? If so, please cite them.

Response: The methodology is not novel and has been used in many studies. We have added references. The work itself, a 10-m resolution grid map for greater London, has not been done before. Line 187 – 191:

> *"The method for processing emission inputs for chemistry transport models has been implemented in several existing tools, including the online emission module in COSMO (Jähn et al., 2020) and the stand-alone processors HERMES (Guevara et al., 2019), EMIPS (Chen et al., 2023), and FUME (Belda et al., 2024). However, these tools are primarily designed for global and regional applications and directly use emission inventories at those scales; therefore, they are not readily applicable to our study."*

RC2.10 Line 179: Provide a clearer link or reference back to the section where the VPRM model is described.

Response: A reference is added to the sentence: (line 211)

> *"VPRM (described in Section 2.2) is implemented in PALM as part of an online chemistry module, where biogenic fluxes are computed prior to solving the scalar transport equation using near-surface air temperature and short-wave radiation retrieved from PALM runtime variables."*

RC2.11 Figure 2: Improve the resolution and overall clarity. The figure should be more informative and include the locations of observational sites used for model validation.

Response: Figure 2 (map for both domains) has been replaced. The new version shows larger markers for geographic references as well as BT tower site location.

RC2.12 Figure 3: Improve resolution and include the location of the eddy covariance tower used to validate CO2 fluxes and wind.

Response: As the map data is from OpenStreetMap, we couldn't obtain a higher resolution background map. The figure here is to show the complex topography of the study area where individual buildings and street canyons contribute to the heterogeneity of the surface.

The location of the eddy-covariance tower site has been added to the figure.

RC2.13 Figure 4: Use a color scheme that makes different sectors easier to distinguish.

Response: A different colour scheme is selected for Figure 4.

RC2.14 Table 2: Consider organizing it into sections corresponding to each stage of the model workflow, as presented in Figure 1.

Response: A new leading column is added to Table 2 showing the stage of the model workflow that the data is used for.

RC2.15 Section 3.5.2: Were alternative datasets tested for initial and boundary conditions? If so, explain why the EGG4 product was selected.

Response: We used the CAMS global greenhouse gas reanalysis (EGG4) for CO_2 boundary conditions because it provides spatially and temporally consistent global fields of atmospheric CO_2 mixing ratios suitable for use in regional transport modelling. Compared to other datasets such as CarbonTracker, it would be more compatible with the meteorology driver ERA5 that we have selected. In contrast to surface-only observational datasets, EGG4 is generated using a data assimilation system that combines atmospheric transport modelling with a wide range of observations, ensuring physical consistency and complete global coverage.

We have tweaked the section to include this justification: (line 422 – 424)

“*This information comes from CAMS global greenhouse gas reanalysis (EGG4) (Copernicus Atmosphere Monitoring Service, 2021), due to its spatially and temporally consistent global coverage and suitability for boundary forcing in regional atmospheric transport simulations*.”

RC2.16 Line 335: Again, the terminology referring to anthropogenic emissions and biogenic fluxes is unclear. Please revise for consistency.

Response: This has been corrected to: “*the input anthropogenic emissions and online-computed biogenic fluxes*” (line 435).

RC2.17 In Section 4.1, the model performance is evaluated only qualitatively through figures. No statistical metrics are presented. The authors should include quantitative evaluation (e.g., RMSE, bias, correlation).

Response: The RMSE, bias and correlation coefficient has been added to Figure 5, 6, and 7. Following summary sentences are included in corresponding paragraphs:

“When July is excluded, the average RMSE and correlation coefficient are 16.74 µmol m-2 s-1 and 0.74, respectively.” (line 447)

“The average RMSEs for wind speed and wind direction were 2.30 m s-1 and 37.50◦, respectively, while the corresponding average correlation coefficients were 0.50 and 0.68.” (line 462)

RC2.18

Additionally:

- The manuscript claims anthropogenic contributions, but was there a clear separation between anthropogenic and biogenic components?

Response: Anthropogenic emissions and biogenic fluxes for the study area (both N01 and N02 domains) are computed and listed in Table 3 for comparison.

RC2.19

- If so, include this analysis to quantify their respective contributions.

Response: Quantitative description is added to the end of this section together with its implication: (line 507 – 511)

“To show the ratio of biogenic fluxes and anthropogenic emissions, Tab. 3 also lists the total biogenic fluxes in the N02 domain (8 by 8 km2) as well as the N01 domain. For the N02 domain, biogenic flux to anthropogenic flux ratio is between -4.72% to 2.29%; for the N01 domain the ratio is between -78.2% and 8.6%. This shows that in the densely populated Camden borough biogenic fluxes have very limited offset to anthropogenic emissions.”

RC2.20 Lines 380–390: The authors do not report actual flux values nor compare results with previous studies. Given the scarcity of observations, such comparisons are essential for validation.

Response: Additional Table 3 and Figure 10 are included to enrich the evaluation of the biogenic module. The biogenic flux is compared with the ICOS-VPRM product. Figure 8 has been updated to include the standard deviation of site observations. We have to admit that the purpose of the analysis is to provide confidence that the biogenic flux module produces realistic carbon fluxes. The assessment of VPRM parameters and even different biogenic flux modules and the corresponding validation can be our future work. For example, Reviewer 1 mentioned the possibility of coupling the soil moisture or canopy modules in PALM with the biogenic flux module.

RC2.21

Please:

- Include quantitative values.

Response: Quantitative values are included in Table 3. In the main text, the following texts are included in this revision: (Section 4.2, line 505 – 507)

"*Using the NEE on the 16th day of each month to represent monthly flux, average biogenic flux in 2019 is -3.95 kton day−1 in our model while ICOS-VPRM reports average NEE -4.92 kton day−1.*"

RC2.22

- Compare with existing literature.

Response: Table 3 and Figure 10 compare our model with ICOS-VPRM product. We have added the following description in Section 4.2: (line 502 – 507)

"*Second, the sum of the total biogenic flux in the N01 domain (60 by 60 km2) is compared with the ICOS-VPRM product (Gerbig and Koch, 2021). Figure 10 shows the comparison. The annual trend of biogenic flux computed in PALM agrees with that in ICOS-VPRM in general. A positive net flux during winter, late summer and autumn seasons appeared in both computations, while spring and early summer show negative net flux. The values are listed in Tab. 3. Using the NEE on the 16th day of each month to represent monthly flux, average biogenic flux in 2019 is -3.95 kton day−1 in our model while ICOS-VPRM reports average NEE -4.92 kton day−1. Correlation coefficient between them is 0.97.*"

RC2.23

- Add a figure showing the diurnal cycle of simulated surface CO2 concentrations to complement the ABL analysis.

Response: A figure showing the diurnal cycle of simulated mean CO2 concentration is included in Appendix G. In the main text, we included additional information on ABL heights and stability to complement the ABL analysis.

RC2.24 Lines 436–438: The manuscript states that “the model is validated against eddy-covariance flux and wind measurements from multiple sites.” However, it appears validation was conducted for only one site. Please clarify.

Response: This is our fault. To correct this, validation and evaluation are described separately now. Line 648 – 659:

“*The model is validated against eddy-covariance flux and wind measurements from measurement site at BT tower. Within the urban case study domain, simulated turbulent CO2 fluxes and horizontal wind speeds show good agreement with observations from the BT Tower, where anthropogenic emissions dominate the measured signal. …*

To further assess the biogenic component, PALM-CO2 is evaluated against flux measurements from the Lanžhot site in the Czech Republic, a deciduous forest ecosystem comparable to the green spaces in the Regent’s Park in the study area. …

The annual average biogenic flux of the Greater London Area is -3.95 kton day−1 in the model, close to the number in the ICOS-VPRM product -4.92 kton day−1. …”

RC2.25 Furthermore, no statistical analysis is presented to support the claim of “good agreement,” which is also stated in the conclusions.

Response: Statistics of validation and evaluation results are inserted to the paragraph to support the argument. Line 650 - 658

“*When July is excluded, the average RMSE and correlation coefficient for EC fluxes are 16.74 µmol m−2 s−1 and 0.74, respectively. The average RMSEs for wind speed and wind direction were 2.30 m s−1 and 37.50°, respectively, while the corresponding average correlation coefficients were 0.50 and 0.68. …*

The annual average biogenic flux of the Greater London Area is -3.95 kton day−1 in the model, close to the number in the ICOS-VPRM product -4.92 kton day−1. Annual cycle of biogenic flux agrees with the ICOS-VPRM product, with a correlation coefficient 0.97.”

Technical Corrections

- Line 22: “uptake” instead of “update”.

Response: Corrected.

- Line 32: “by” instead of “bay”.

Response: This refers to the San Francisco Bay Area. It is capitalised in this revision to avoid confusion.

- Line 58: Consider “land and vegetation types” instead of “land types and vegetation”.

Response: This has been corrected.

- Line 63: "...air pollution studies...", add appropriate citations.

Response: The following citations are included to support the argument:

Stroh, E., Harrie, L., and Gustafsson, S.: A study of spatial resolution in pollution exposure modelling, International journal of health geographics, 6, 19, 2007.

Li, Y., Henze, D. K., Jack, D., and Kinney, P. L.: The influence of air quality model resolution on health impact assessment for fine particulate matter and its components, Air Quality, Atmosphere & Health, 9, 51–68, 2016.

Fenech, S., Doherty, R. M., Heaviside, C., Vardoulakis, S., Macintyre, H. L., and O'Connor, F. M.: The influence of model spatial resolution on simulated ozone and fine particulate matter for Europe: implications for health impact assessments, Atmospheric Chemistry and Physics, 18, 5765–5784, 2018.

- Line 75: Replace "external forcing" with "external inputs".

Response: This has been corrected.

- Figure 1: Improve legend clarity.

Response: We improved the clarity of Figure 1 by revising the caption to clarify the categories of inputs, model components and outputs. Font size of the figure has been increased.

- Figure 2: Improve figure and caption quality.

Response: Figure 2 (map for both domains) has been replaced. The new version shows larger markers for geographic references as well as BT tower site location. The caption has been organised for each panel.

- Figures 5 and 6: Split into panels (A, B, C, D) and describe each in the caption.

Response: This has changed.

- Figure 10: Clearly indicate the locations of the regions used in the cross-section within the domain.

Response: The location of the vertical cross-section is shown in Figure 2(b). A reference is added to Figure 10.

Citation: https://doi.org/10.5194/egusphere-2026-970-RC2