

Response to RC2:

This study demonstrates how CO₂ EC flux partitioning into anthropogenic and biogenic components can be done without depending on additional measurements by developing an ecological parameter-migration framework between two cities in Guangzhou, China. The two cities shared similar climate conditions but had different land cover distributions, with the donor city having mostly vegetative land cover and the target city being dominated by impervious (or built-up) land cover. The authors clearly explained most of their methodology and provided sensitivity and uncertainty calculations that supported their choices, and the results indicated that the framework they proposed worked relatively well. It would be interesting to have a bit more discussion in the paper on how feasible the authors think it would be to reproduce this at other sites or other areas of the world.

Response:

We sincerely thank the reviewer for the positive assessment of our framework, methodological description, and sensitivity analyses. We agree that the feasibility of applying the framework at other sites and in other regions requires more explicit discussion. **We have therefore added Sect. 3.6, “Transferability and implications for urban carbon monitoring,” (Pages 46–48, Lines 1241–1290) and revised the corresponding statement in the Conclusions to clarify the practical requirements and limitations of extending the approach beyond the CH–PY site pair within Guangzhou (Page 50, Lines 1334–1335).**

The new discussion explains that transferability depends on ecological comparability between donor and target sites. Suitable donor sites should have broadly comparable climatic and vegetation backgrounds and sufficiently weak anthropogenic disturbance for ecological responses to be identifiable. The framework transfers vegetation-normalized ecological response parameters, rather than absolute donor-site GPP or ecosystem respiration fluxes. Target-site biogenic fluxes are then reconstructed using local meteorological forcing, dynamic footprints, and footprint-weighted vegetation information. Application elsewhere therefore requires appropriate local

donor selection and target-site data, rather than direct reuse of the CH–PY parameter relationships.

We also discuss how differences in vegetation functional types, phenology, irrigation, management, and soil conditions can increase transfer uncertainty, particularly for ecosystem respiration, which is less directly constrained by vegetation indices than photosynthetic uptake. Where a single donor cannot adequately represent the ecological heterogeneity of the target footprint, multiple donors, vegetation-specific parameterizations, or additional local ecological observations may be needed. The framework can potentially be implemented using paired EC observations from suitable donor and urban target sites, meteorological data, footprint modelling, land-cover information, and satellite vegetation indices without continuous isotope or multi-species flux measurements. We now explicitly state that the framework has so far been evaluated only for the CH–PY site pair, and that its performance in other climatic and ecological settings requires local evaluation.

The revised discussion also outlines the framework’s potential for interpreting anthropogenic signals and biological masking and evaluating inventory spatial allocation. Sustained observations across urban EC networks could further help track multi-year trends in anthropogenic emissions and net biogenic uptake and support assessment of emission-reduction and urban-greening measures. We also clarify that individual towers provide footprint-scale constraints and that the framework does not independently resolve anthropogenic sectors. Assessing city-wide emission trends or mitigation outcomes would require integration with broader observation networks and atmospheric transport or inversion modelling.

To support reproducibility, we have also expanded the descriptions of footprint-model implementation and spatial weighting, inventory processing and temporal matching, and the observational basis and construction of auxiliary diurnal profiles. These revisions are detailed in the point-by-point responses below.

The paper is well-written and detailed in most aspects, but as highlighted below, there are a few areas that require additional information in order to improve comprehension and reproducibility, specifically regarding footprint development and emission inventory/diurnal profile implementation. Once this and a few minor comments (listed below) are addressed, I recommend this paper for publication.

Response:

We appreciate the reviewer's encouraging comments on the clarity and level of detail of the manuscript, and the recommendation for publication following the requested revisions. The specific suggestions have helped us identify where additional explanation was needed to improve comprehension and reproducibility.

In response, we have added a dedicated subsection describing footprint-model inputs, applicability screening, normalization, and spatial weighting; expanded the inventory description to specify source coverage, spatial and temporal resolutions, original units, conversion procedures, and correspondence with the observation years; and clarified the measurement sources and processing steps used to construct the auxiliary diurnal profiles. We have also addressed the remaining comments by updating relevant figures and captions, clarifying temporal coverage and interpretive limitations, and correcting terminology and notation. Below, we provide point-by-point responses and indicate the corresponding locations of the revisions in the main text and Supplement. The revised text has been highlighted in the manuscript for ease of reference.

Abstract

Line 34: I would replace 'footprint-aligned' with 'footprint-weighted'.

Response:

Thank you for this helpful suggestion. We have replaced "footprint-aligned" with "footprint-weighted" in the Abstract to more accurately describe the weighting procedure (**Page 1, Line 34**).

Introduction

Lines 71-94: Somewhere in this paragraph, it would be good to cite a study that came out a couple of months ago (Molinier et al 2026, <https://doi.org/10.1021/acsestair.6c00148>) which shows another way to try to partition urban EC flux measurements into anthropogenic and biogenic components by combining footprints, emission inventories, and VPRM outputs rather than adding more measurements.

Response:

We thank the reviewer for pointing out this relevant recent study. We have added Molinier et al. (2026) to the third paragraph of the Introduction (**Page 3, Lines 74–78**) and described their approach, which combines dynamic flux footprints, sector-resolved emission inventories, and Vegetation Photosynthesis and Respiration Model (VPRM) outputs to estimate anthropogenic and biogenic CO₂ contributions and evaluate their sum against urban EC observations. This addition broadens our overview of existing approaches by acknowledging the complementary role of inventories and ecosystem modeling alongside measurement-based partitioning methods.

In the following paragraph (**Page 4, Lines 101–104**), we now acknowledge that inventory–model approaches depend on the availability and representativeness of spatially and temporally resolved emission inventories and biogenic flux estimates. These revisions provide a more balanced description of the available approaches and their respective data requirements.

Data and methods

Lines 193 and 196: It would help to already know the measurement heights at this stage, rather than waiting until the next section.

Response:

Thank you for this helpful comment. We have moved the EC measurement heights (48 m at PY and 40 m at CH) to the initial site descriptions in Sect. 2.1 (**Page 7, Lines 198 and 201**), so that this information is available when the two sites are first introduced. To avoid repetition, the measurement-height values were removed from the

instrument descriptions in Sect. 2.2, while the comparison between the EC measurement heights (Z_c , m) and the mean roughness-element heights (Z_H , m) was retained because it provides the methodological basis for evaluating flux representativeness.

We have also clarified the calculation of the zero-plane displacement height (d_m , m) and the effective measurement height (z_m , m) above the displacement plane (**Page 9, Lines 242–247**). Specifically, displacement height was approximated as $d_m = 0.7Z_H$ following Grimmond and Oke (1999), and effective measurement height was calculated as $z_m = Z_c - d_m$, where Z_c is the EC measurement height above ground. These calculations yielded effective measurement heights of 38.8 m at PY and 29.4 m at CH, explicitly distinguishing the heights used in footprint modelling from the measurement heights above ground (**Page 10, Lines 284–285**).

Lines 197-198: What is the study area that is considered by the footprint model (in m² or km²)?

Response:

We thank the reviewer for requesting this clarification. We have specified the tower-centered computational domains in Sect. 2.1: –2200 to +2200 m in both horizontal directions at PY (4.4 × 4.4 km; 19.36 km²) and –2000 to +2000 m at CH (4.0 × 4.0 km; 16.00 km²). We also clarify that these dimensions define the computational domains rather than fixed EC source areas, which varied dynamically among individual 30 min periods (**Page 7, Lines 203–213**). The Fig. 1 caption has been updated accordingly to state the domain dimensions and the 10 m grid resolution (**Page 8, Lines 222–223**).

Figure 1: Is it possible to make the cumulative footprint contours (30%-70%) more distinctive? They're difficult to see even after zooming in.

Response:

Thank you for this helpful suggestion. We have revised Fig. 1 to improve the visibility of the cumulative footprint contours against the land-cover background. All

contours are now shown in black, with increased line widths for the 30 %, 50 %, and 70 % contours. We have also removed the “Tower” text from both map panels to reduce obstruction of the inner contours, retained the yellow triangular markers indicating tower locations, and added the corresponding tower symbol to the right-hand legend. These changes improve contour visibility in both panels while keeping the tower locations clearly identified (**revised Fig. 1, Page 8, Line 218**). The revisions concern graphical presentation only; the footprint calculations and contour locations remain unchanged.

Line 256: The authors allude to ‘footprint calculations’ here and have mentioned using ‘footprint-weighted’ parameters throughout the main text and SI without actually explaining how they implemented the model, what inputs they used, quality flagging of the footprints themselves, etc. A short paragraph or section on this topic would strengthen the paper as it would help with reproducibility.

Response:

We thank the reviewer for this helpful suggestion. We agree that the original manuscript did not sufficiently describe the footprint-model implementation. **We have therefore added a dedicated subsection, Sect. 2.3, “Flux-footprint modelling and spatial weighting,”** to improve methodological transparency and reproducibility (**Pages 10–11, Lines 278–304**).

The new subsection describes the application of the Kljun et al. (2015) FFP parameterization to individual 30 min EC records, including the required inputs and site-specific effective measurement heights. It clarifies that wind and turbulence variables were obtained from observations or derived during EC processing, aerodynamic roughness length was estimated from observed mean wind speed and turbulence-derived friction velocity and Obukhov length using the wind-profile relationship, and boundary-layer height was obtained from ERA5 reanalysis. It also specifies temporal matching, computational domains, the 10 m spatial resolution, and FFP-based screening principles, including $u^* > 0.1 \text{ m s}^{-1}$ and an effective measurement height within the atmospheric boundary layer (**Page 10, Lines 278–295**).

We further explain the normalization of footprint fields within the computational domains, their use in deriving footprint-weighted land-cover fractions and EVI, and their monthly aggregation for inventory weighting (**Pages 10–11, Lines 296–304**). To reduce repetition, we have streamlined the corresponding description in the parameter-migration subsection (**Page 15, Lines 426–429, Lines 436–437**) and revised the opening of Sect. S5 to distinguish footprint calculation and screening from the subsequent spatial registration, EVI processing, valid-pixel renormalization, and inventory-processing procedures (**Supplement: Sect. S5, Page 6, Lines 132–140**).

Line 347: For clarification, does ‘11 d subsets’ refer to subsets of 11 days?

Response:

Thank you for pointing out this ambiguity. Yes, “11 d subsets” refers to data subsets spanning 11 d that were used for ecological parameter fitting. We have replaced this phrase with “data subsets spanning 11 d” and clarified that the fitted parameters were subsequently smoothed with an 11 d moving average (**Page 14, Lines 392–393**).

Results and discussion

Line 463: So the 2025 observations were not considered? If that is the case, the authors should specify that earlier when they mention the duration of observations.

Response:

Thank you for this comment. We agree that the analysis period should be stated explicitly when the observation period is first introduced. The January–April 2025 observations were not included in the main EC analyses presented in this study, which used observations from 1 January 2023 to 31 December 2024. We have therefore clarified this distinction in Sect. 2.2, immediately after introducing the overall observation period from 1 January 2023 to 30 April 2025 (**Page 9, Lines 251–253**).

Lines 833-834: I might have missed this, but can the authors clarify where the diurnal patterns of these species are coming from? Measurements of NO_x, CO, and PM_{2.5} (or any other tracer, for that matter) are not mentioned in the methods section, so are these well-established profiles or are they observation-based? While there is a brief reference to observations of these species in the SI, more detail is needed on how the diurnal profiles were developed for use in this study.

Response:

We appreciate the reviewer's request to clarify the observational basis and processing of these profiles. The tracer concentration profiles shown in Fig. 7 are observation-based, rather than prescribed profiles or patterns taken from the literature. They were constructed from observations collected from 1 January 2023 to 31 December 2024, concurrent with the main EC analysis period. NO_x and PM_{2.5} concentrations were obtained from the Higher Education Mega Center atmospheric-environment monitoring station, approximately 2 km northeast of the PY tower, whereas CO and CO₂ concentrations were measured at the PY site.

For each species, observations were first aggregated to hourly means. Available hourly means were then grouped by local hour of day (00:00–23:00 LT), and values within each group were averaged across the study period, producing a 24-point mean diurnal concentration profile. The half-hourly F_{ff} estimates were similarly aggregated to hourly means and then averaged by local hour of day to construct the corresponding mean diurnal flux profile.

Each resulting 24-point profile was subsequently z-score standardized using the mean and standard deviation of its own 24 values. Pearson correlations between the F_{ff} profile and each concentration profile were calculated across the 24 standardized hourly values.

We have added Sect. 2.7, “Auxiliary tracer observations and diurnal-profile analysis,” to introduce the observation sources, analysis period, and processing steps before these profiles are used in the results (**Pages 16–17, Lines 463–474**). Sect. S7 now provides the detailed profile-construction procedure, standardization formula, and correlation calculation (**Supplement: Sect. S7, Pages 13–14, Lines 301–320**). We have

also revised the opening of Sect. 3.4.1 (**Page 33, Lines 894–897**) and the Fig. 7 caption to identify the observational basis and analysis period, clarify the construction of the mean diurnal profiles, and specify that the measurements were made “at and near the PY site” (**Page 34, Lines 915–922**).

In addition, we have revised the wording in the first paragraph of Sect. 3.4.1 to distinguish the factors influencing CO₂ concentration from the additional processes affecting PM_{2.5}, avoiding a combined description that could incorrectly associate secondary aerosol formation with CO₂ (**Page 33, Lines 908–910**).

Lines 944-946: It would be helpful to include the spatial and temporal resolutions of the inventories here in addition to the SI.

Response:

Thank you for this helpful comment. We agree that the spatial and temporal resolutions should be stated directly where the inventories are introduced. We have therefore revised Sect. 3.4.3 to specify that all three inventories were used at monthly temporal resolution, with native spatial resolutions of 1 km × 1 km for ODIAC and 0.1° × 0.1° for EDGAR and MEIC (**Page 38, Lines 1018–1021**). We have also clarified in Sect. S5 that monthly products were used for all three inventories, ensuring consistency between the main text and the Supplement (**Supplement: Sect. S5, Page 8, Lines 184–189**).

Supplement Information

Section S3: Out of curiosity, do the human respiration emissions calculations account for whether humans are indoors or outdoors? Is this what is meant by ‘activity level’ or ‘activity intensity’? Also, just under equation S2, the term ‘Agrid’ is not formatted with subscripts as the other variables in the text.

Response:

Thank you for this helpful comment. We agree that the treatment of indoor and outdoor populations and the meaning of “activity level” and “activity intensity” should be clarified. Indoor and outdoor populations were not treated separately in the human-

respiration calculation. Here, “activity level” and “activity intensity” refer to physical activity and its associated per-capita metabolic CO₂ production, rather than indoor or outdoor occupancy. In our simplified parameterization, the hourly per-capita respiration rate was linearly scaled between 120 and 280 μmol CO₂ cap⁻¹ s⁻¹ according to the normalized diurnal variation in population density. This scaling represents an assumed proxy for metabolic variation, rather than independently observed activity intensity. We have clarified the population treatment and terminology in the opening paragraph of Sect. S3 (**Supplement: Sect. S3, Page 3, Lines 57–60**).

We have also revised the final paragraph of Sect. S3 to clarify the assumed nature of this variation and explicitly state that indoor CO₂ storage and ventilation-related release delays were not represented (**Supplement: Sect. S3, Page 4, Lines 86–89**).

Finally, we have corrected “Agrid” in the text immediately below Eq. (S2), formatting “grid” as a subscript consistently with the equation (**Supplement: Sect. S3, Page 4, Line 74**).

Section S4: The air temperature variable ‘Ta’ also is not formatted with subscripts like the other variables in the text.

Response:

Thank you for noting this formatting inconsistency. We agree that the air-temperature notation should be consistent between the text and equations. We have corrected both textual occurrences of the air-temperature variable in Sect. S4, immediately before and below Eq. (S6), by formatting “a” as a subscript consistently with the equations (**Supplement: Sect. S4, Page 5, Lines 117 and 121**).

Section S5: How were emissions in the inventory reported and what sectors were included? How were they converted to flux units? If the inventory is, for example, annual, how did the authors convert it to a monthly inventory? Or is the inventory already reported on a monthly basis? Lastly, did the authors use the 2023 inventory for comparison with observations from both 2023 and 2024? More details are needed regarding this aspect of the analysis.

Response:

We thank the reviewer for identifying the need for a more complete description of the inventory comparison. **We have expanded the inventory subsection of Sect. S5 to specify the emission sources included, original reporting units, temporal resolution, unit-conversion procedures, and correspondence between inventory years and observation years.** We have also revised the inventory versions and numerical ranges in the first paragraph of Sect. 3.4.3, updated Fig. 10 and its caption, and added the sources of all three inventories to the Code and data availability section.

First, the revised Sect. S5 specifies the source coverage of each inventory. ODIAC includes fossil fuel combustion, cement production, and gas flaring; its 1 km product used here excludes international aviation and marine bunker emissions. For EDGAR, we used the fossil CO₂ component, IEA-EDGAR CO₂, covering power generation, industrial combustion, buildings, transport, fuel exploitation, industrial processes, and fossil CO₂ contributions associated with agriculture and waste. The separately reported EDGAR CO₂bio component was excluded. MEIC-global-CO₂ includes fossil fuel combustion and cement-process emissions, with 52 categories aggregated into six sectors: electricity and heat production, industries and construction, transport, residential and commercial, international aviation, and international navigation. Biofuel combustion emissions are excluded (**Supplement: Sect. S5, Page 8, Lines 190–199**).

Second, all three inventories were obtained as monthly gridded products, so no additional annual-to-monthly disaggregation was performed in this study (**Supplement: Sect. S5, Page 8, Lines 184–189**). Their native spatial resolutions are 1 km × 1 km for ODIAC and 0.1° × 0.1° for EDGAR and MEIC. Conversion to $\mu\text{mol CO}_2 \text{ m}^{-2} \text{ s}^{-1}$ followed the original units of each product. ODIAC monthly grid-cell totals in tonnes C were divided by the native-grid cell area and the actual number of seconds in the corresponding month, and multiplied by $10^{12}/12.01$. MEIC monthly grid-cell totals in tonnes CO₂ were similarly divided by grid-cell area and monthly duration, and multiplied by $10^{12}/44.01$. In contrast, the EDGAR files already provide monthly mean mass fluxes in $\text{kg CO}_2 \text{ m}^{-2} \text{ s}^{-1}$ and were multiplied directly by $10^9/44.01$, without further division by area or time. The converted fields were subsequently mapped to the

footprint grid and weighted using the monthly footprint climatologies, as described by Eq. (S12) (**Supplement: Sect. S5, Pages 8–9, Lines 200–214**).

Third, we acknowledge that the original comparison used the 2023 monthly inventory fields for observations from both 2023 and 2024. We have now updated the comparison to use monthly inventory fields for 2023–2024, matching the inventory year and month to those of the observations. The updated products are ODIAC2025, EDGAR_2025_GHG, and MEIC-global-CO₂ updated through 2024. The original manuscript reported approximate footprint-weighted monthly inventory ranges of 9–11 $\mu\text{mol m}^{-2} \text{s}^{-1}$ for EDGAR, 13–15 $\mu\text{mol m}^{-2} \text{s}^{-1}$ for MEIC, and 16–23 $\mu\text{mol m}^{-2} \text{s}^{-1}$ for ODIAC. The updated footprint-weighted inventory estimates, averaged across 2023–2024 for each calendar month, range from 8.68 to 10.51 $\mu\text{mol m}^{-2} \text{s}^{-1}$ for EDGAR, 12.98 to 15.01 $\mu\text{mol m}^{-2} \text{s}^{-1}$ for MEIC, and 16.64 to 25.15 $\mu\text{mol m}^{-2} \text{s}^{-1}$ for ODIAC. These ranges have been updated in Sect. 3.4.3 (**Page 38, Lines 1024–1025**), and the corresponding inventory estimates and temporal coverage are reflected in the revised Fig. 10 and its caption (**Page 39, Line 1041, Lines 1046–1047**).

Alongside the inventory update, we have corrected the PY F_{ff} range reported in Sect. 3.4.3 from 2.59–4.29 to 2.49–4.23 $\mu\text{mol m}^{-2} \text{s}^{-1}$ to match the monthly means shown in Fig. 10 (**Page 38, Line 1026**). This correction concerns only the range reported in the text; the PY F_{ff} data and their presentation in Fig. 10 remain unchanged. The previously reported range corresponds to the monthly medians of daily aggregated fluxes presented in Sect. 3.3.2 and Fig. 6. Sect. 3.4.3 now explicitly identifies the corrected range as monthly means, whereas Sect. 3.3.2 and Fig. 6 retain the monthly medians of daily aggregated fluxes.

To quantify the effect of this update, we compared the original and updated estimates using the same footprint weighting and temporal aggregation. For each inventory, the mean flux was calculated across the 12 calendar-month estimates shown in Fig. 10. The table below reports these means, their relative changes, and the range of relative changes across the 12 calendar-month estimates. Flux values are in $\mu\text{mol CO}_2 \text{m}^{-2} \text{s}^{-1}$:

Inventory	Original mean flux	Updated mean flux	Relative change	Range of monthly relative changes
EDGAR	9.6584	9.4634	−2.02 %	−3.60 % to −0.94 %
MEIC	14.1115	14.054	−0.41 %	−4.37 % to +2.86 %
ODIAC	19.285	20.4895	+6.25 %	+1.12 % to +10.25 %

Separately, we calculated the inventory-to- F_{ff} ratio for each calendar month and then averaged the 12 ratios to assess whether the update affected the several-fold relationship between inventory estimates and observation-inferred fluxes:

Inventory	Original mean ratio	Updated mean ratio	Relative change in mean ratio
EDGAR	3.116	3.054	−1.99 %
MEIC	4.566	4.547	−0.42 %
ODIAC	6.241	6.618	+6.04 %

The changes in both mean inventory fluxes and mean inventory-to- F_{ff} ratios are modest relative to the several-fold inventory- F_{ff} differences. The updated inventory estimates remain higher than the observation-inferred F_{ff} throughout the monthly comparison, with mean ratios of approximately 3.1, 4.5, and 6.6 for EDGAR, MEIC, and ODIAC, respectively. Updating the inventory years therefore improves temporal correspondence without changing the several-fold differences between inventory estimates and observation-inferred fluxes or the main interpretation of this comparison.

Finally, links to the ODIAC, EDGAR, and MEIC data sources have been added to the Code and data availability section to make the inventory products used in this comparison explicitly traceable (**Page 51, Lines 1368–1371**).

Section S8: Since the road congestion data are from May 2026 and the EC data are from 2023–2024, why do the authors state that they ‘do not fully overlap’ when actually they do not overlap at all?

Response:

Thank you for this comment. We agree with the reviewer. The road-congestion data collected during 6–24 May 2026 have no temporal overlap with the EC data analyzed for 2023–2024. The phrase “do not fully overlap” was inaccurate and has been replaced with “have no temporal overlap” in Sect. S8 (**Supplement: Sect. S8, Page 14, Line 332**). We have also revised the corresponding sentence in Sect. 3.4.2 to make this

temporal separation explicit (**Page 36, Lines 965–966**).

Figure S9: There is no reference to this figure that I could find in the main text, so it would be great if the authors could provide some text to explain the significance of the traces in panels a and b overlapping so much. In panel b especially, it is almost impossible to distinguish between the three curves that are apparently shown.

Response:

Thank you for this comment. Figure S9 was referenced and discussed in Sect. 3.5.3 of the original manuscript, including explicit references to Fig. S9a and Fig. S9b–c. We have now expanded the discussion in Sect. 3.5.3 (**Page 46, Lines 1219–1223, Lines 1231–1235**) and the Fig. S9 caption (**Supplement, Page 27, Lines 448–453**) to explain the overlapping curves more directly.

In panel (a), the close overlap of the baseline, Scheme B, and weak daytime-respiration-inhibition curves indicates limited sensitivity to the tested temperature-group boundaries and respiration-inhibition strength. The pure saturating and saturating + VPD inhibition curves also nearly overlap because the fitted VPD inhibition coefficient is close to zero. However, these two structural alternatives yield lower F_{ff} than the baseline, demonstrating that GPP model structure still affects the inferred flux magnitude.

In panel (b), changing human respiration by $\pm 50\%$ changes the mean F_{ff} by only approximately $\pm 0.01 \mu\text{mol m}^{-2} \text{s}^{-1}$, with a maximum hourly effect of approximately $\pm 0.026 \mu\text{mol m}^{-2} \text{s}^{-1}$. Consequently, the three curves are difficult to distinguish at the full flux scale. Panel (c) shows these small, oppositely signed differences relative to the baseline separately. The revised text and caption now explicitly direct readers to this difference panel.