

This document contains the responses to the two reviews to the manuscript submitted to EGU sphere titled: “Measurement report: Temporal variability of vertical profiles of CO₂ and CH₄ over urban environment” by Zimnoch et al. The text of the originally submitted article and its public discussion is available at:

<https://egusphere.copernicus.org/preprints/2024/egusphere-2024-1167/#discussion>

Please note that the text available under link above might differ from the one under review due to changes from previous review round.

Response to reviewers

Before addressing specific comments, we would like to offer our sincere thanks to the reviewers for the time and attention given to our manuscript, and for all the suggested corrections, both scientific and language-related. All are highly appreciated. The rebuttal has been prepared by lead authors with contributions from all co-authors.

Please find the original text from the reviewer in black, while the responses from authors are in blue. Text cited from the manuscript is in italics. In this document, we refer to the original manuscript using Lx notation (consistent with the reviewer’s comments), while an R index is added when the revised manuscript is cited. For example, L15 refers to the original manuscript, while RL15 refers to the revised version.

We would also like to mention that we have expanded the list of authors by adding Mr. Michał Kud, who was erased from the authors’ list by omission.

Answers for Reviewer #1 remarks:

Measurement report: Temporal variability of vertical profiles of CO₂ and CH₄ over urban environment

Submitted to ACP

The manuscript presents an overview of a dataset consisting of CO₂, CH₄ concentrations, and meteorological variables collected over an 11-month period in Krakow, Poland. The stated objective of the study is to report these measurements. The authors describe specific vertical profiles of the trace gases and variables collected and conclude that they capture the formation, evolution, and decay of the atmospheric boundary layer, and the influence of local terrain and plumes in the profiles. They also claim that these datasets have the potential for validation of transport models. Indeed, the dataset is potentially valuable and relevant to improving measurement strategies and advancing the understanding of nocturnal boundary-layer processes. However, in its current form, the manuscript requires substantial revision before it can be considered for publication.

First, the introduction lacks sufficient depth. Key statements are not supported by an adequate literature review, and the contextual framing of the study is relatively superficial.

The Introduction has been thoroughly changed. More recent and adequate literature has been added.

The methodology section is not clearly presented and would benefit from significant restructuring. I strongly recommend including schematic diagrams of the measurement setup and methodological workflow to enhance clarity and reproducibility. For a measurement report, detailed descriptions of the measurement procedures, calibration protocols, and data quality assurance are essential. These aspects are insufficiently explained in the current manuscript.

The methodology section has been reconstructed. Schematic diagrams presenting measurement setup for both platforms were added. Description of meteorological sensors has been shortened to enhance the clarity of text. Detailed description of sensors has been moved to Supplementary materials, while in the main text only parts relevant to obtained results are presented. The GHG measurement procedure, calibration procedures and WMO scales has been introduced.

In addition, a more substantive scientific interpretation of the observations is expected, rather than a primarily descriptive presentation. The case studies presented are only superficially addressed, with only one short paragraph describing the case study of methane. While the manuscript includes the use of modelling to support the interpretation of the data, details on these are not sufficiently developed.

The Results section has been significantly extended, including deeper discussion of the results. The methane case study was supplemented with more information regarding possible emission sources and its position, however a deeper analysis requires further research using a more detailed description of emission sources and using more advanced modeling tools, which is beyond the scope of this article. The section describing the use of the WRF-GHG model from the original manuscript has been removed. Instead, a preliminary analysis of CO₂ plume transport using the HySplit model is presented.

Finally, several figures require improvement. Proper labelling, clearer descriptions, and more thorough interpretation are needed to ensure the results are communicated effectively.

All the figures have been revised and modified accordingly. The unification of the terminology was made. Figure 1 now includes more information including the position of main CO₂ and CH₄ emission sources in the area. Two more figures (RFigure2 and Figure S2) presenting measurement setup and the comparison of the altitude determination methods based on GPS signal and barometric formula has been introduced. The figures presenting vertical profiles has been simplified by selecting only representative examples of the profiles. Figures containing all profiles are included in the supplementary material. The estimation of SBL depth was included as well.

Overall, the manuscript presents a dataset with potential scientific value, but major revisions are required to address the outlined issues.

Overall comments:

Abstract

The abstract refers to the nocturnal boundary layer, but the term *nocturnal* appears only there and is not consistently used or defined in the main text. It is unclear whether the authors use *nocturnal boundary layer* interchangeably with *stable boundary layer (SBL)*.

We thank the reviewer for this remark. We went through the text and unified the usage of terminology. To underline characteristics of the urban conditions, we use:

Urban Boundary Layer to describe Atmospheric Boundary Layer in the city, and Stable Boundary Layer (SBL) or Convective Boundary Layer (CBL) to refer to appropriate analysed boundary layer types.

Introduction

The introduction is lacking sufficient citations, and many statements are not supported by the existing literature. A few specific examples include, but are not limited to:

Following the reviewer's suggestions, we have heavily modified the introduction to improve clarity and add necessary citations. Please note that some literature citations were excluded (in fragments that were discarded) or replaced (where more appropriate citations were found). We hope that the reviewer will find the improved introduction satisfactory.

We specifically addressed—but did not limit the changes to—all the statements listed by the reviewer below:

- Lines 33–39

The paragraph describing the relationship between land-use/land-cover (LULC) has been thoroughly modified. Work of Duren and Miller highlighting the challenges of linking dispersed and heterogeneous emission sources to the determination of the emission budget has been referenced. LULC maps in urban areas deliver information regarding the geometry of streets defining the distribution of transport remissions or location of greenery responsible for biospheric sources/sinks in the urban areas. Therefore, the relationship between LULC and the distribution of GHGs in urban areas seems to be justified.

- Lines 53–57

This fragment has been discarded in the modified introduction. Instead, in Methods Section 2.2.3. we have clarified that one of the advantages of our system is the ability to use regularly employed calibration that closely resembles the ICOS atmospheric network procedures described in “ICOS Atmospheric Station specifications Version 2.0” (“Chapter 4: Calibration, standards”). the actual calibration procedure is not followed exactly, as the stations managed by the AGH University of Kraków (Kraków, Białystok, Kasprowy Wierch) are not yet included the ICOS network due to the lack of national ICOS programme in Poland. Efforts are underway to start such a programme. These include activities in AGH, therefore calibration procedures are adjusted to closely resemble those within ICOS to the maximum extent.

The calibration procedures used in the lab follow adhere to the recommendations from GAW, and one of the stations managed by the group, Kasprowy Wierch, management is a GAW Regional site, and its data is reported to the publicly available data collections (NOAA GML ObsPack products for CO₂ and CH₄, <https://gml.noaa.gov/ccgg/obspack/>). Additionally, data from KASLAB at Kasprowy Wierch are frequently used in regional and global atmospheric inversions (e.g. Munassar et al., 2022), where reporting data fulfilling string WMO reproducibility goals is necessary. As we employ the same calibration procedures for the presented dataset as for KASLAB, we are confident that the data is accurately linked to the respective WMO scales.

The full calibration procedure is described in section 2.2.3. We have also added the reference to the ICOS Atmosphere Station Specifications in the text.

References:

ICOS RI, 2020. ICOS Atmosphere Station Specifications V2.0 (editor: O. Laurent). ICOS ERIC. <https://doi.org/10.18160/GK28-2188>

Munassar, S., Rödenbeck, C., Koch, F.-T., Totsche, K. U., Gałkowski, M., Walther, S., and Gerbig, C.: Net ecosystem exchange (NEE) estimates 2006–2019 over Europe from a pre-operational ensemble-inversion system, *Atmos. Chem. Phys.*, 22, 7875–7892, <https://doi.org/10.5194/acp-22-7875-2022>, 2022.

- Line 58 – this is a particularly important statement and must be supported by appropriate references.

We have now added the following references:

IEA: World Energy Outlook, chap. 8, pp. 179–193, International Energy Agency, 2008

Seto, K., Dhakal, S., Bigio, A., Blanco, H., Delgado, G. C., Dewar, D., Huang, L., Inaba, A., Kansal, A., Lwasa, S., McMahon, J., Müller, D. B., Murakami, J., Nagendra, H., and Ramaswami, A.: Human settlements, infrastructure and spatial planning, chap. 12, Cambridge University Press, Cambridge, UK and New York, NY, USA, 2014

- Line 68 – a citation is needed to substantiate the claim regarding “legal limitations.”

The geographical zones defined by Polish Air Navigation Services Agency based on, and in accordance with the regulations of European Commission (Commission Implementing Regulation (EU) 2019/947 of 24 May 2019) are determining the allowed UAV operations performed in Polish airspace. In case of AGH location, it is located within CTR zone of Krakow airport, and this fact limits the maximum altitude of drone operations to 100 m a.g.l.

Although this manuscript is a measurement report, statements made in the introduction must still be grounded in scientific literature. In several places where citations are provided, they are limited to one or two references that do not adequately represent the current state-of-the-art. For example, **line 80** cites only a case from Marseille, which is not even among the longest-running urban monitoring systems, and a two-year measurement study in remote Alpine locations (Schneefernerhaus and the Zugspitze ridge). These examples do not sufficiently demonstrate the broader context of existing observational networks or related research. The introduction needs to be substantially revised to reflect the current state-of-the-art and to clearly demonstrate how the observations presented in this study fill a gap or address an unmet need. At present, the introduction is not adequately supported by literature, which is a critical requirement for both measurement reports and full research articles.

Please find our general response above. In addition (specifically to the comment here), we have expanded our reference list to include studies for which state-of-the-art and long-term efforts towards city-scale observation and modelling of emissions have been published. These include studies from Indianapolis (INFLUX), Paris, Munich, Hamburg and Washington, DC–Baltimore Metropolitan Area. Please find the references to appropriate studies in the revised text (omitted here for brevity). On top of those we also added a reference to a recently published GAW Report No. 314: “Urban Emission Observation and Monitoring Good Research Practice Guidelines”.

We have omitted several other excellent examples, but we hope that the expanded list will be to reviewer’s satisfaction.

Reference:

WMO: Integrated Global Greenhouse Gas Information System: Urban Emission Observation and Monitoring Good Research Practice Guidelines, GAW Report No. 314, <https://doi.org/https://doi.org/10.59327/WMO/GAW/314>, 2025.

Methods

The methodology section requires substantial restructuring. It is difficult to follow and lacks essential procedural detail. This section would greatly benefit from a clear diagram illustrating the instrument setups on each platform, as well as a table listing all sensors used and the conditions under which they were deployed. Information about sensors that were ultimately not used could be moved to the Supplementary Material to improve clarity. Additionally, two “case studies” are mentioned, yet they are not described in the Methods section at all.

The authors should clarify why the selected sites were chosen and how representative they are of the urban boundary layer (UBL). A schematic of the experimental design for both the balloon and the UAV would considerably strengthen the methodological description. There is incomplete information regarding the air inlet in Section 2.2.2 (line 138), which seems only partially addressed later in Section 2.2.3 (line 171). The methodology is also confusing in its description of instrumentation: several sensors are described as being present but not used under certain circumstances (e.g., lines 193–194 for the BME280 and thermocouple; lines 201–204; unused components of the TriSonica anemometer on lines 213–214 and 220–223; and precipitation/radiation sensors on line 230). If the sensors were calibrated using an in-situ meteorological station, a clearer approach would be to justify the selection of each sensor by presenting its performance relative to the stationary reference, rather than listing all instrument capabilities—used or unused.

Following the reviewer’s comments, Section 2.2 (Measurement Techniques) has been revised. A diagram illustrating the instrument setups has been added to the manuscript (RFigure 2). In accordance with the suggestion, Section 2.2.4 has been modified, and part of the information regarding the sensors has been moved to the Supplementary Material. The description of the measurement sites in Section 2.1 has also been updated. In addition, a new subsection (Section 2.4) has been added to the manuscript, describing in detail the methodology used to determine the SBL height applied in this study.

The manuscript does not specify the local time offset from UTC for Kraków, which prevents the reader from interpreting whether the reported times correspond to day or night. There is also no explicit definition of sunrise and sunset times, and the terms “nighttime” and “morning” are used without clear thresholds. This ambiguity makes the temporal context of the measurements difficult to interpret.

We thank the Reviewer for highlighting this important issue. We clarify that all times in the manuscript are reported in Coordinated Universal Time (UTC). For reference, local time in Kraków corresponds to UTC+1 during standard time (typically from late October to late March) and UTC+2 during daylight saving time (from late March to late October). This information has been added to the revised text in Section 2.1.1. (“Flight organisation and schedule”).

To further improve clarity, we have clarified that the sunrise and sunset times have been calculated using the algorithm provided in *Almanac for Computers* (1990, Nautical Almanac Office, United States Naval Observatory). This reference is also added in the same section.

Results

The results section is only superficially developed and lacks clear, traceable interpretation. The authors attempt to describe patterns in the observations, but the text does not sufficiently guide the reader to where these features appear. Figure labelling is poor, and in cases where the authors do reference a specific panel (e.g., Figure 4-d, line 302), the corresponding panel is not labelled within the figure itself. In other instances, figures are cited in an overly general manner, for example, Figure 5 (line 310) is discussed as a whole, yet it contains ten panels that are not individually identified or described. This makes it very difficult for the reader to follow the analysis. It is also unclear why particular cases were chosen for illustration. For example, at line 310, the manuscript does not state whether the selected profile is representative of typical conditions, an exceptional case, or simply a random example. Without this context, the value of the case studies is significantly diminished.

The two case studies presented, one for CO₂ and one for CH₄, are described only superficially, without sufficient depth or supporting evidence. In the introduction, the authors state: “*The obtained results allowed us to analyse in-depth the formation, development and disappearance of the nocturnal boundary layer. In selected profiles, CO₂ and CH₄ plumes located over the inversion layer (150–250 m AGL) were detected during the nighttime and morning hours.*” However, the manuscript does not provide such in-depth analysis. There is not enough detail on even how the boundary-layer height was determined. Overall, the results require substantial expansion, clearer connection between text and figures, explicit justification for case selection, and a more rigorous scientific interpretation consistent with the claims made in the abstract and introduction.

Conclusion

Results and Discussion

Section 3 (Results and Discussion) has been substantially expanded. All figures have been revised, three new figures, a boxplot presenting seasonal variability along with campaign statistics calculated for three altitude ranges: (20–50 m, 80–100 m, and 180–200 m a.g.l.) supplemented with regional background observed at nearby mountain KASLAB station, and two plots showing temporal variability of three above layers during the transition from CBL to SBL and SBL to CBL has been added. The corresponding references in the text have been clarified and made more precise.

Specific comments

Line 16: Use *GHG* for *greenhouse gas* (singular) and *GHGs* for *greenhouse gases* (plural).

Corrected.

Line 106: The location of the Vistula River is not clearly visible in the map. Additionally, the location of the balloon site is unclear. Please improve the map to clearly indicate these features.

Figure 1 has been modified accordingly.

Line 115: “*The direct distance between measurement sites was 2.9 km.*” — Please specify which sites this refers to (presumably the balloon site and AGH). This information should also be added directly to the map.

The sentence has been modified in accordance with the Reviewer’s suggestion.

Lines 152–153: The text mentions a “literature review,” but does not specify which literature was consulted. Please clarify and add appropriate citations supporting the choices made.

The paragraph has been modified. The analyses were based on study by Hedworth et al. (2022) and conclusions from own observations. We agree that the statement “literature review” was not fully justified.

Lines 157–159: The reference to “recent studies” requires specific citations.

The “recent study” in question is the study by Hedworth et al. (2022), who performed CFD simulations of the airflow and scalar fields around the ascending and descending drone. The authors have demonstrated that the measurement error caused by the turbulence generated by propellers is minimal during the ascending fly, at the location above the drone. We have modified the fragment in question to clarify that.

We have modified the text in the manuscript, it now reads:

RL222: “a recent study by Hedworth et al. (2022) indicated that vertical measurements of gaseous pollutants during ascent are characterized by the lowest relative error (on the order of a few %), while during the descent, the relative error of measured mole fractions of gaseous pollutants may be even an order of magnitude higher”

Lines 169–170: Clarify what the intercomparison was performed against (e.g., another instrument, a reference station, a specific calibration standard).

After consideration and consulting with our colleagues, we have decided to remove the mention of intercomparison, as the intercomparison procedures have not been practiced in some years. These have been a regular practice in the past, but after the centralized distribution of the calibrated air mixtures prepared by the Central Analytical Laboratories of ICOS was introduced, the intercomparisons are not needed.

Please also see our response to the comment regarding Lines 53–57 of the unrevised manuscript for more information on the calibration procedures.

Line 180: Please provide the observed water vapor percentages mentioned.

Provided as suggested.

Lines 233–242: The calibration procedure requires much clearer description. The explanation is vague and incomplete.

- How exactly were the linear calibrations (e.g., $y = ax + b$) derived?

Section has been modified. The GHG measurement procedure, calibration procedures, calculations of calibration formulas and WMO scales has been introduced.

- Table 3 is referenced implicitly, but the methodology leading to those coefficients must be explicitly described.

The methodology is described in section 2.2.4

- The anemometer is said to perform “correctly” under calm conditions (0 m s^{-1}), but no information is given about performance at other wind speeds. This is insufficient, as calm conditions are often excluded from both analysis and modelling. A proper assessment of calibration quality across the measurement range is needed.

The sentence “perform “correctly” under calm conditions” was intended to justify the use of a linear equation without an intercept (the calibration line intersects the origin of the coordinate system).

Line 260: It is unclear how the concentrations shown in Figure 2a were obtained. Does the figure show monthly averages, monthly maxima, minima, or another metric? Without this clarification, the following text (lines 261–265) is difficult to interpret.

The caption of Figure 2 (currently Figure 3) has been corrected accordingly. The paragraph describing the data has also been revised to improve clarity and consistency.

Line 295: Figure 4 does **not** show the evolution of the convective boundary layer, but rather the evolution of vertical profiles of trace gases and meteorological variables.

Please:

- Include a visual indication of the boundary-layer height on the profiles (e.g., horizontal lines).

Included as requested.

- Explain how the boundary-layer height was determined (visual inspection? potential temperature gradient? CO_2 gradient? bulk Richardson number?).

We have used visual inspection based primarily on GHG gradient and supported by meteorological variable inspection. After deliberation, to assure reproducibility, we have now adopted a simple algorithm for BL detection that detects midpoint between identified layers using the GHG vertical profiles. Its detailed description is given in section 2.4 of the revised manuscript.

- A standard reference such as Stull (1988) should be consulted for correct methodology.

We thank the reviewer for this suggestion. In fact, we have consulted the excellent book by Stull (1988) as well as other literature, e.g. the review paper by Kotthaus et al. (2023). Indeed, a large part of our measurement efforts were focused on evening, night-time and morning measurements, during boundary layer behaviour is highly variable (also seasonally). However, for many observed profiles the meteorological situations are not typical or not defined. A detailed study of the urban boundary layer based on the collected data is planned in the near future, with particular emphasis on comparisons of various nighttime diagnostics. However, we have determined that due to complexity of the issue, it is out of scope for this study.

As the BL is only used for indicative purposes for this Measurement Report, we have opted to use a simple layer detection algorithm instead. Please also see our answer to the comment immediately above.

Reference:

Kotthaus, S., Bravo-Aranda, J. A., Collaud Coen, M., Guerrero-Rascado, J. L., Costa, M. J., Cimini, D., O'Connor, E. J., Hervo, M., Alados-Arboledas, L., Jiménez-Portaz, M., Mona, L., Ruffieux, D., Illingworth, A., and Haeffelin, M.: Atmospheric boundary layer height from ground-based remote

sensing: a review of capabilities and limitations, *Atmos. Meas. Tech.*, 16, 433–479, <https://doi.org/10.5194/amt-16-433-2023>, 2023.

Line 302: The manuscript refers to “Figure 4-d,” but the panels in Figure 4 are not labeled. Please ensure all figure panels are clearly identified.

The Figure 4 (currently Figure 6) has been modified accordingly.

Line 312: This is the first (and apparently only) instance where temperature is expressed in kelvin. Please use consistent units throughout the manuscript.

The units have been standardized in the manuscript.

Lines 336–340: This is the only mention of modelling results, which are used to support the claimed origin of the CO₂ plume—yet these modelling results are not shown (line 338).

I recommend either:

1. Removing this from the paper and saving it for a dedicated publication, **or** 2. Fully incorporating the modelling analysis into this manuscript, including a description in the methodology.

The paragraph has been modified, the subsection presents the results of the Hysplit back trajectory model calculated using GFS0p25.

Lines 342–347: It is unclear why the CO₂ plume source could be identified but not the CH₄ plume. If source attribution is to be part of this manuscript, please:

- Provide sufficient information for both GHGs,
- Describe the modelling tools and methodology,
- Integrate this material into the methods and results sections.

Otherwise, this discussion should be deferred to a future publication where the interpretation can be properly developed.

The CO₂ plumes could be identified because there are only few strong stack level emission sources and all peaks were detected significantly above SBL. A simple HySplit simulation allowed to identify the source as Skawina PP. In case of methane the picture is much more complicated. There are several possible emission sources, most of them exists at the surface and is not well identified. Detected peaks in most cases exists within SBL, so we conclude, that this part needs more investigation and is out of scope of this article. Contrasting pictures in case of CO₂ and CH₄ plumes also shows a complexity of such tasks.

Lines 4, 66, 74, 101, 114, ..., 350–351: The manuscript uses the word *parameters*, but *variables* is the appropriate term here. Please correct this throughout the text.

Corrected

Figures

Figure 1

- Define all abbreviations at first mention in the caption, including *AGH* and *EC*.
- Clarify whether “m amsl” is a typo and should be “m a.s.l.” (metres above sea level).

The Figure 1 has been corrected.

- Consider adding relevant contextual layers such as major road networks, major industrial facilities (especially high-emitting ones), and landfill locations. Since the text mentions “numerous landfills” (line 100), indicate how many there are and where they are located. This would help readers better understand the spatial distribution of emissions relative to the observation sites.

Thank you for this suggestion. We have added to the map the major road networks from OpenStreetMap database, and major industrial facilities (for both CH₄ and CO₂) from the CoMet Emission Database (CoMet ED v4.01, 2021) - a composite product based on EU-reported E-PRTR and Industrial Emissions Database (2022) that collected information on most important anthropogenic sources of CH₄ and CO₂ over Silesian region and its vicinity (including Kraków). To the best of our knowledge, no major changes between in major sources has occurred between 2018 and 2021, when the study was conducted.

The map colours were slightly altered to accommodate new symbols.

Gałkowski, M., Fiehn, A., Swolkien, J., Stanisavljevic, M., Korben, P., Menoud, M., Necki, J., Roiger, A., Röckmann, T., Gerbig, C., Fix, A., 2021. Emissions of CH₄ and CO₂ over the Upper Silesian Coal Basin (Poland) and its vicinity. <https://doi.org/10.18160/3K6Z-4H73>

- *EC Skawina* and *EC Krakow* are not introduced in Section 2.1 (Study area), where Figure 1 is referenced. These facilities only appear later in the manuscript (lines 331–333). Their first mention should be moved earlier.

Agreed. Please note that we have changed EC (Pol. “elektrociepłownia”, eng. cogenerating power plant), to more widely used PP (power plant) both in Figure 1 and in the text.

Figure 2

- Panels must be labelled (“a”, “b”, “c”, “d”).
- The caption must explain how the monthly concentrations were derived. Are these monthly means, daily maxima, monthly medians, or another metric? Without this clarification, interpretation is unclear.

Figure 2 (currently Figure 3) has been modified. The panels (A-B) and (C-D) present respectively monthly and daily mean of CO₂ and CH₄ mole fraction from AGH site (dark line) at 40 m a.g.l. measured using a Picarro G-2311-f analyser. The Figure was supplemented with measurements from the Kasprowy Wierch high mountain station (blue line) covering the campaign period, from March 2021 to April 2022. The grey shading in panels C–D indicates the diurnal variability at the AGH site, defined as the daily minimum and maximum mole fractions.

Figure 3

- Label each panel (“a”, “b”, “c”, etc.).
- Correct the misspelling (e.g., “repersent”).
- The figure is difficult to read due to overlapping lines and excessive information. I strongly recommend focusing on a smaller number of representative cases (as done in Figure 4). Select profiles that best illustrate seasonal and diurnal variability and move the remaining profiles to supplementary material with appropriate explanation.

The Figure 3 (currently Figure 5) has been modified accordingly.

Figure 4

- Panels are referenced as “a–d” in the caption, but no labels appear on the figure.
- When using abbreviations (e.g., AT for air temperature, WS for wind speed), define them in the caption for clarity.

The Figure 4 (currently Figure 6) has been modified accordingly.

Figure 5

- As with previous figures, label all panels clearly and refer to them specifically in the text (e.g., “Figure 5c”).

The Figure 5 (currently Figure 8) has been modified accordingly.

- Maintain consistency in stylistic conventions, such as using “No.” (line 296 and Table 1) instead of “no” in the caption.

Corrected.

Other editorial comments

- Ensure consistency in the use of *AGL* vs *a.g.l.* The manuscript currently mixes both styles. Since *a.s.l.* is also used, *a.g.l.* would be the more consistent choice.

Corrected.

- Ensure consistency in spelling “nighttime.” Line 314 contains “night-time,” which should be harmonised with the rest of the text.

Corrected.

Answers to remarks from Reviewer #2:

General Comments

The manuscript is a measurement report analyzing vertical profiles of CO₂ and CH₄ as well as of various meteorological properties within the first ~300m of the atmosphere over Krakow, Poland. Measurements were obtained across 11 months, giving a nice sampling of conditions over a wide range of seasons and variation in the sources and sinks of CO₂ and CH₄ for this urban location.

Vertical profiles of CO₂ and CH₄ mole fractions and meteorological parameters were used to investigate transitions between the typically more stable nocturnal boundary layer and the convective boundary layer during the day.

Two case studies are presented, with one discussing a potential source for increased CO₂ concentrations in distinct layers above the nocturnal boundary layer observed on some flights. The source is suspected to be a nearby co-generation plant powered by coal combustion. Another case study examines enhancements in CH₄ in certain vertical layers above the nocturnal boundary layer. The source for these peaks in CH₄ is assumed to be different from those of CO₂ as they are not temporally or vertically coincident. The authors state more work is needed to fully understand the sources of these CH₄ peaks above the nocturnal boundary layer.

The dataset is of high temporal and vertical resolution in the lower atmosphere over Krakow, making this a highly useful dataset for multiple applications, including comparison to model simulations, validation of satellite retrievals, and improved understanding of local dynamics in GHGs in urban locations with complex topography and emission sources/sinks as the authors mention.

I see that the paper has undergone significant revisions, and I find it only needing minor revisions. This is a very valuable dataset for the reasons the authors mention throughout, and the figures/discussion provided show the utility of the data for quantifying the evolution of the boundary layer and GHG mole fractions in the lower atmosphere of an urban location across their diurnal cycles.

[We thank the reviewer for his comment and appreciate the time spent on our manuscript, which helped to significantly improve it. Please find our responses to specific comments below.](#)

Specific comments:

Figure 1: I know you mention in the case studies what EC Skawina and EC Krakow are, but it would be great if you could add that to the caption in Fig. 1. I believe 'balloon' is misspelled in your legend.

[Corrected as suggested.](#)

Line 147: I saw that other reviewers asked about the 'detailed analysis' for determining that altitude calculated from the barometric formula is more precise than that from GPS measurements. It wouldn't hurt to put some result of that analysis in the supplement just to show how you arrived at that conclusion.

Figure presenting comparison of the altitude determined based on GPS signal and atmospheric pressure has been added to the Supplement as requested (Fig S2).

Figure 2: Please write in the caption what the grey shading is in panels B. I think I read in another reviewer's comment that these are the daytime and nighttime values, but it would be great to make this very clear in the figure caption.

The grey shading in panels C–D indicates the diurnal variability at the AGH site, defined as the daily minimum and maximum mole fractions. This information has been added to the figure caption.

Line 268: Missing word 'us'?

Thank you for catching this, the sentence has been corrected.

Line 275: What about with season? Since this discussion is about seasonal and diurnal trends, I think it would sound more complete to comment on both even if just very briefly.

The analysis of seasonal variability of GHGs is described in section 3.1.

Line 279: Is that true for urban locations? I'm not totally sure, but I wonder how heating at night and transportation patterns for example might compare to diurnal variability from the biosphere. Perhaps provide a citation, thanks.

The analysis of diurnal variability of CO₂ in Krakow using stable isotope tracers confirms the dominant influence of the biosphere during the growing season in urban areas. Citation provided.

Line 282: Are you sure that yellow lines are for summer here? I was reading in the Fig 3 legend that JJA is blue?

The Figure 3 has been corrected.

Line 304: Sunrise should be sunrise

The typo was corrected in the text.

Line 318: What do you mean by 'Over the morning, the enhancement in GHG mole fractions develops a well-mixed layer'? I was seeing that mole fractions of CO₂ and CH₄ below the inversion layer decreased as the morning progressed from sunrise (5:13) to later in the morning (9:22 – lowest concentrations of CO₂ and CH₄ in the boundary layer).

The intention was to explain the formation of a well-mixed surface layer limited by a sharp decrease on top by a cap inversion. The text has been modified.

Line 356: I have seen that you wanted to remove the modeling aspect of this paper, but would it be possible to at least show the HYSPLIT trajectory (it can be a very basic run)? Or at least explain why it is not shown in this article (e.g., if a forthcoming paper will evaluate this case study in greater detail).

The analysis of CO₂ event has been supplemented with a simple HySplit simulation confirming the hypothesis regarding the observed plume emission source location.