Dear Editor,

This manuscript addresses an important and practical challenge in hectare-scale greenhouse-gas flux monitoring by proposing a UAV-based system that integrates fully onboard, simultaneous multi-gas measurements, in-flight reference-gas drift correction, and ethane tracer release. The study covers four flight patterns and multiple environmental settings, and the figures are intuitive and informative. The instrument integration and field implementation have clear engineering value. In particular, the simultaneous acquisition of CH_4, N_2O, CO_2, wind-field data, and tracer-gas measurements on a single platform, together with the attempt to use tracer plumes to determine the vertical integration limit for mass-balance calculations, represents potentially impactful work.

However, the principal quantitative conclusions of the current manuscript have not yet been validated with sufficient rigor. The drift correction, wind-field correction, and uncertainty propagation all rely on relatively strong assumptions. The method has not been cross-validated against an independent high-precision flux measurement technique or through a systematic series of controlled-release experiments. I therefore recommend major revision. Until greater methodological description, additional validation experiments, and more comprehensive uncertainty analyses are provided, the method should not be presented as having been conclusively demonstrated to achieve high accuracy result.

Major comments

The abstract currently remains largely qualitative and does not provide sufficient numerical evidence to support its principal claims. In particular, statements regarding substantially improved sensitivity, reduced uncertainty, source separation, complete capture of emissions, and determination of the appropriate integration height should be supported by key quantitative results. In addition, I believe it is necessary to briefly specify in the abstract which instruments were used for the flux measurements.

The Introduction provides a broad motivation for improving field-scale greenhouse-gas flux measurements and identifies several practical limitations of current UAV-based approaches, including instrument drift, uncertain source-area attribution, wind-field variability, and the lack of simultaneous measurements of multiple greenhouse gases. However, the section is currently overextended and somewhat unfocused. A substantial portion is devoted to examples of underestimated emissions across oil and gas systems, wastewater treatment, agricultural ponds, and peatlands, whereas the methodological gap addressed by this study is introduced relatively late. The Introduction should be shortened and reorganized around a clearer progression: the need for hectare-scale flux measurements, the strengths and limitations of existing UAV approaches, the specific unresolved problems addressed here, and the explicit objectives and hypotheses of the study.

Section 2 contains the core methodological information of the study, but it lacks the equations used to calculate fluxes under the different flight configurations. Although this subsection is intended to describe the methodology, I found only one equation in the entire subsection, and that equation represents only the flux density at an individual point. Additional equations should therefore be provided to describe how point-level fluxes are integrated to obtain segment-level and total fluxes for the box, two-wall, and long-wall configurations. In addition, the manuscript does not provide a sufficiently detailed description of the wind-speed correction procedure, which is fundamental to the flux calculations. The authors should provide the equations used to correct for UAV motion and attitude. This is a critical component of the methodology and should not be overlooked. The uncertainty analysis should also account for the uncertainty introduced by the wind-speed correction. The Methods section is the most important part of the manuscript. The authors should therefore revise it carefully and thoroughly.

Specific comments

Line 8: The expression "unwanted gas emissions" is overly broad and value-laden. Since the manuscript focuses on greenhouse-gas fluxes, the wording should be made more specific.

Line 14: The phrase "mapping many different GHGs" is imprecise. The system measures three target greenhouse gases—CH_4, CO_2, and N_2O—together with C_2H_6 as a tracer. This distinction should be made explicitly.

Line 14: What is "UAS"? Abbreviation should be defined at their first occurrence.

Line 14: The terms "all instrumentation onboard" and "fully onboard multi-gas measurements" are repetitive. One of these expressions should be removed.

Lines 15–16: The term "in-flight calibration" may be misleading because the onboard ambient-air bag appears to be used for relative baseline-drift correction rather than traceable absolute calibration. Please consider using "in-flight drift correction using onboard reference air."

Lines 17–19: The field sites are presented together, but the abstract does not distinguish which cases were used for methodological testing, tracer validation, or environmental demonstration. Please clarify the role of each type of application.

Line 20: The statement that drift correction can "substantially increase sensitivity" requires a quantitative comparison and a clearly defined reference condition. Please specify whether this refers to improvement relative to uncorrected data, a previous UAV system, or another measurement approach.

Line 20: The claim that drift correction "reduce uncertainty" should specify the type of uncertainty involved. Drift correction most directly reduces baseline-related concentration uncertainty, whereas total flux uncertainty also includes wind, positioning, interpolation, and atmospheric non-stationarity.

Lines 22: The statement that tracer release enables "separation of sources" should be supported by a specific result or example. The abstract should explain whether the separation was qualitative or quantitatively validated.

Lines 21–23: The claim that the method verifies that targeted emissions are "fully captured" is too strong. A tracer released from a selected location may constrain sampling coverage but cannot necessarily represent all source locations, release heights, or temporal variations. Consider replacing "fully captured" with "adequately captured" or "intercepted by the sampling walls."

Lines 21–23: Because the tracer approach is presented as a central methodological contribution, the abstract should report the measured recovery of the known C_2H_6 release, or at least the agreement between the known and drone-derived tracer flux.

Lines 22–24: The phrase "determination of the altitude range required for mass balance calculations" should be made more precise. Consider using "determination of the upper integration height needed to capture source-related transport."

Lines 19–24: The abstract contains no representative flux values from the peatland, lake littoral zone, manure wells, or dairy barn. At least one or two quantitative field results should be included to demonstrate the environmental relevance of the method.

Lines 25–30: The opening discussion of international climate agreements and national greenhouse-gas inventories is broad and somewhat distant from the specific methodological focus of the paper. Consider shortening this paragraph and moving more rapidly to the need for reliable measurements at facility and landscape scales.

Lines 30–55: The Introduction includes many examples of inventory underestimation from unrelated sectors. Although these examples demonstrate a general monitoring gap, the discussion is lengthy and dilutes the central methodological argument. Consider retaining only the most relevant examples and summarizing the remainder in one sentence.

Lines 34–36: The phrase "1.5 to 2-fold greater" should be standardized, for example as "1.5–2.0 times higher," while avoiding ambiguity between "greater than" and "times as large as."

Lines 35–43: The examples from low-production oil and gas sites, ultra-emitters, and the IEA Global Methane Tracker are relevant to methane monitoring but only indirectly related to the multi-gas, hectare-scale UAV method proposed here. Please clarify why these examples are particularly relevant to the spatial scale and detection limits targeted in this study.

Lines 42–47: The discussion of peatland rewetting and the erosion of the distinction between natural and anthropogenic emissions is interesting but only loosely connected to the specific method-development objectives. Please explain more directly why these mixed-source landscapes require the proposed tracer-assisted UAV approach.

Line 42: The phrase “natural based solutions” should be corrected to “nature-based solutions.”

Lines 49–57: The manuscript repeatedly states that monitoring capacity is limited, but the specific spatial and temporal measurement gap should be defined more precisely. Please explain why the hectare scale is particularly difficult and what performance requirements a suitable method should meet.

Lines 54–55: The authors state that a “non-invasive (i.e., does not disturb the measured fluxes)” measurement method is needed. However, I believe that UAV-based measurements inevitably introduce some disturbance to the airflow and potentially to the measured fluxes, particularly because of rotor downwash. I therefore suggest that the authors reconsider this wording and use a more cautious and precise description.

Lines 56–57: The statement that chamber and eddy-covariance methods motivate the need for alternatives is too general. Please briefly explain their specific limitations at the target scale.

Line 66: The transition from satellites and infrared imaging to drone-based systems is logical, but the Introduction should more clearly explain what unique measurement capability is provided by co-located gas and wind measurements onboard a UAV.

Lines 75–77: “Drifts in gas concentrations of analyzers and sensors on a UAV can be considerable and nonlinear, and stabilization can take several hours if at all reached within reasonable time.” Please provide the relevant references.

Lines 79–91: The list of three methodological limitations is useful and should become the central organizing structure of the Introduction. Consider shortening the earlier background and expanding this section with a concise review of how previous studies have addressed each limitation.

Lines 86–91: This paragraph describes the specific work conducted by the authors. Please indicate the subsection(s) in which each of these tasks is presented or evaluated.

Line 83: The statement that simultaneous measurement of “all three major GHGs” has apparently not been performed should be treated cautiously. Please define the gases intended.

Line 83: Throughout the Introduction: Please standardize the terminology “UAV,” “UAS,” “drone-borne system,” and “iUAS-FGHG.” Too many overlapping terms make the methodological narrative unnecessarily difficult to follow.

Line 92: Section heading following the Introduction: The heading “2 Introduction” appears to be incorrect and should presumably be “2 Methods” or “2 Materials and methods.”

Lines 93–120: Please provide a table summarizing all instruments, including manufacturer and model, measured variables, sampling frequency, precision and weight. The current information is distributed across the text and is insufficient for a clear assessment of system performance.

Line 98: Please change “auxillary” to “auxiliary.”

Lines 96–98: The anemometer was positioned 94 cm, or 1.54 rotor diameters, above the propellers, and the authors state that this was sufficient to minimize disturbance. Please provide quantitative validation results rather than relying only on a literature citation and the statement that the setup was verified against stationary anemometers.

Lines 122–124: The authors state that lightweight instruments generally exhibit greater drift than heavier instruments because temperature-stable cells are more difficult to design. This is plausible but currently presented as a general conclusion without supporting references.

Lines 127–128: The procedure is described as “in-situ verified, in-flight drift correction,” but it is unclear what is meant by “verified.” Please state what independent measurement was used to verify the correction.

Line 130: “the in-flight drift correction”
Because the bag contains unassigned ambient air rather than a traceable standard gas, the procedure appears to correct relative baseline drift rather than perform an absolute in-flight calibration. Please revise the terminology consistently throughout the manuscript.

Line 134: The statement “Our best results were found” is not sufficiently quantitative. Please define the criterion used to identify the “best” reference-gas composition and report the corresponding performance metrics.

Lines 136–138: “The response times of changes in gas concentrations, for both gas analyzers, with regards to both inlets (at the anemometer and the reference gas bag, having different tubing length) were routinely checked and are considered in the calculations (the time delay between gas intake and change in measured gas concentrations).”
The manuscript states that response times were “routinely checked” and considered in

the calculations. Please provide the actual delay corrections applied separately to each analyzer and inlet, as well as the frequency and results of these checks.

Lines 145–148: When I reached this point, Section 2.2 appeared more like a description of the experimental procedure than a sufficiently supported methodological evaluation. For example, the authors state, "Hence, we also performed tests on how different reference gas measurement frequencies influenced baseline correction." However, the subsequent text only explains how the tests were conducted and does not report the corresponding results. Please either provide the key quantitative outcomes here or clearly direct readers to the subsection, figure, or table in which these results are presented.

Line 150 "vertical movement of emitted gas"
I understand the controlled-release experiment as an attempt to simulate atmospheric dispersion. However, the flux derived using the box model and mass-balance approach appears to account only for fluxes through the lateral boundaries, without explicitly including the vertical flux through the top boundary. I suggest deleting the phrase "vertical movement."

Lines 157–159: "Air movement models for this are highly sensitive to the in-situ variability in topography, surface roughness, wind shadows, air heating, etc., or are computationally very expensive and not adapted for all landscapes."
The statement that air-movement models are either highly sensitive, computationally expensive, or unsuitable for all landscapes is too broad. Please provide appropriate references and describe more specifically which modelling alternatives were considered.

Line 163: "C2H6"
Please report the purity of the C_2H_6 gas and the uncertainty of the regulator output during each experiment.

Lines 174–175: "lot of turbulence due to complicated ground structures"
Why would complex terrain necessarily be associated with strong turbulence? Turbulence can generally be divided into thermal and mechanical components. Complex terrain can indeed generate strong wind shear and thereby enhance mechanical turbulence, but the current wording is overly broad and insufficiently precise. Please rewrite this sentence to clarify the physical mechanism involved.

Lines 195–204: For box flights, please define how the box dimensions, flight-wall orientation, altitude spacing, and maximum altitude were selected for each site.

Lines 239–241: "By flying at very low altitude, in our case 2-5 m, and at a drone speed of 3-5 m/s it was possible to cover a large area using many transects with enough space between the transects not to affect each other from propeller downwash."

The assertion that transects were sufficiently separated to avoid downwash interference requires quantitative support. Please report the transect spacing and the basis for selecting it.

Line 242: “As the wind speed is negligible and vertical transport is low in stable conditions”
The statement that stable conditions necessarily produce low vertical transport should be qualified. Stable stratification suppresses turbulence but does not eliminate drainage flows, intermittent mixing, or advection.

Lines 256–257: “This includes correcting measured wind data for the drone speed, direction, and orientation relative to north.”
I am particularly concerned about how the wind-speed correction was performed. Could the authors add the relevant equations to the Methods section, including how corrections were made for UAV velocity and the pitch, roll, and yaw angles? In addition, was the corrected wind speed validated against independent reference measurements? This is critically important for the flux calculations.

Lines 290–291: “The net flux is then the difference between the outgoing and incoming flux densities for each segment.”
Please provide the corresponding equation.

Lines 291–293: “The total flux from the area between the walls is calculated by adding net fluxes for all segments up to an altitude limit that is high enough that all potential sources and sinks within the enclosed area can be measured at the outgoing wall.”
Please provide the corresponding equation.

Lines 296–304: “The flux uncertainty for …”
I believe this uncertainty analysis primarily accounts for instrumental measurement errors. However, the uncertainty introduced by the wind-speed correction could be substantial and should also be included. In particular, the authors state above that the minimum flight altitude was 2–5 m. At such low altitudes, UAV downwash may have a pronounced influence on the wind-speed measurements.
Additional uncertainty also arises from the use of the mass-balance model, because its application depends on several underlying assumptions. I therefore recommend that the authors reconsider and expand the uncertainty assessment for the UAV-based measurements. In particular, the uncertainty associated with wind-speed correction must be explicitly quantified and propagated into the final flux uncertainty.

Lines 368–369: “while the rewetted peatland itself is a net CO2 sink, it is a source of CH4 and N2O at the time of measurements.”
The manuscript states that the rewetted peatland was a net CO_2 sink and a source of CH_4 and N_2O, but this paragraph mainly presents concentration-wall patterns rather than flux results. Please distinguish concentration patterns from flux conclusions and

link source–sink statements directly to the fluxes and uncertainties in Table 1.

Lines 423–427: “The altitude profiles for this flight…”
Slightly elevated fluxes were still observed between 10 and 20 m, yet the integration height was set to 10 m. Please explain why the 10–20 m contribution was excluded and provide a sensitivity analysis using alternative upper integration heights.

Line 433: The sentence “This shows how fast the gas was rising” is not accurate because vertical gas velocity was not measured directly. The result only indicates the plume-height distribution at a given downwind distance. Please revise the wording.

Figure 10 and Lines 470–474: The flight altitude was approximately 3 m, where ground effects and UAV downwash may be important. Please explain whether the low-altitude concentration and wind measurements were affected by rotor-induced flow and provide validation.

Line 485: The manuscript states that the values were “comparable” with literature reviews but does not provide the actual comparison ranges. Please include the numerical ranges in the text or supplementary table.

Line 489: The phrase “having corresponding flux rates those of the present study” is grammatically incorrect and should be revised.

Lines 503–535: The method discussion should be linked more directly to the actual validation results of this study. At present, much of Section 3.3 is a general literature overview rather than a critical discussion of the performance demonstrated here.

Line 517: “accuracy”
The authors state that synchronous gas and wind measurements improve accuracy, but the study does not provide a direct comparison with an independent reference-flux method. Please replace “accuracy” with a more cautious statement such as “reduces spatial mismatch,” unless independent validation is provided.

Line 537: “high accuracy will depend on successful drift correction”
The statement that high accuracy depends on successful drift correction overemphasizes concentration drift and underemphasizes wind correction, rotor downwash, non-steady conditions, top-boundary transport, lateral losses, and interpolation error. These factors may also dominate the total flux uncertainty.