The below text contains the reviewer comments in black and author answers (AA) in blue

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.

We thank the reviewer for this detailed feedback. All of these comments are addressed through the detailed responses below. We have carefully revised the manuscript to ensure that we do not present the method as "conclusively demonstrated to achieve high accuracy results". Instead, we took care to present the study as a contribution to a development towards improved UAS or drone-based gas flux measurements, emphasizing the need for further work and additional tests. We have also added a more detailed description of the assumptions made in our method as well as regarding the uncertainty estimation. Our attempt was to try method approaches for UAS that were as directly reliant as possible on observations and depend on less assumptions than many other common methods, but obviously all methods do need some assumptions, and we try to clarify this further in the revision.

We agree that method comparisons are important but, as also discussed elsewhere (e.g. Bastviken et al 2022; doi:10.1088/1748-9326/ac8fa9), most methodologies except those for well-defined point sources, are typically not directly comparable because they cover different space-time-domains and all have their own types of errors and biases. There have been numerous attempts to cross-compare most techniques, including e.g. eddy covariance versus large numbers of flux chambers in multiple environments. The

results of such comparisons have varied greatly from reasonable correspondence to differences in various directions, in the end making many comparisons inconclusive (Harper et al. 2011; doi:10.1016/j.anifeedsci.2011.04.013). Hence, beyond flux chamber scales, and for distributed landscape gas exchange, exact verification is at present a challenge, and there is not yet any high-precision flux measurement technique that works well in many environments and can serve as a conclusive reference. Awaiting such a golden standard method, a common approach is to compare ranges with other methods used under as similar conditions and in as similar environments as possible, which is what we try to do in the manuscript.

The tracer release, primarily intended to constrain air motion across the target area and the vertical integration range, represents a strategy to reduce the number of assumptions needed. For shorter distances the tracer release also aimed to enable evaluation of how well point source fluxes in the target area were quantitatively represented by the observations. We believe the tracer strategy helps to at least partially constrain the UAS approaches described, although we fully agree that further improvements and tests are needed in future work.

Major comments

M1. 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.

Thanks for making us aware of using too bold language. We have now revised these statements throughout the manuscript to more appropriate wording hopefully clarifying that we hope to contribute to such long-term goals, albeit being clear that we are aware that additional work is needed to improve and verify the method approaches described. We have also added more information about the gas analyzers as suggested in the abstract, and also the drone and anemometer used.

M2. 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.

We agree and have substantially shortened this part of the Introduction.

M3. 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.

We agree and thank the reviewer for the suggestions. We have now added more detailed method descriptions and formulas in the supplementary information (a new Appendix B) that includes the wind-speed correction procedure, segment-level and total flux integration for the different flight paths, and the corrections for the UAV motion and attitude (roll, pitch, and yaw). Formulas for the drift correction is included. As mentioned above, we have also added a description of the assumptions made (added to the main text).

Specific comments

S1. 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.

Removed “unwanted”.

S2. 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.

We have now made the description more precise.

S3. Line 14: What is “UAS”? Abbreviation should be defined at their first occurrence.

This has now been added when first used in the abstract using the sentence “Uncrewed aerial systems (UAS), comprising drones equipped with lightweight sensor systems, offer a promising platform for mapping greenhouse gas (GHG) emissions”.

S4. Line 14: The terms “all instrumentation onboard” and “fully onboard multi-gas measurements” are repetitive. One of these expressions should be removed.
We have now removed the second term to make it less repetitive.

S5. 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.”
This has now been changed.

S6. 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.
We feel that the abstract would become less easy to read if fully detailing what was done at each location already in the abstract. We therefore clarify that selected flight patterns were used in each environment in the abstract and provide additional details in the main text.

S7. 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.
The sentence has now been changed to: “Our results show the importance of in-flight drift correction or drift control, and that this can reduce baseline-related uncertainty, which is key for detection limits of UAS-based mass balance approaches and estimation of GHG fluxes in a wide range of environments and settings.”.

S8. 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.
We have now clarified this with “reduce baseline-related uncertainty”.

S9, 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.

We have now changed this to “enables distinguishing separate sources in the area of interest”. We believe that Figure 6 (panels A and C) and Figure 9 provides supporting examples, but common practice precludes citing these figures from the Abstract. In those cases, the separation was qualitative (visual or using the tracer plume as border identifier), but the tracer approach also enables other more quantitative ways to distinguish source areas. The end-of-the-abstract sentence in question is intended to highlight the potential of tracer use in general, and we would prefer to not exclude any potential approaches with tracers by making this sentence too specific.

S10. 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.”
Replaced with “intercepted by the sampling walls”.

S11. 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.
We have now added such information in the abstract, while also trying to be clear that this quantitative use of the tracer is not the only value of tracer releases. The end of the abstract now reads “In cases where the tracer release was quantified, the mean recovery was 85% and 94%, respectively, with losses possibly reflecting combinations of wind pattern variability and the dilution of the tracer with distance to below detection limits along the tracer plume periphery. Combining tracer gas release with drone flights also enable in situ-tracking of air movements which enables distinguishing separate sources in the area of interest, verification that targeted emissions are intercepted by the sampling walls and support the determination of the upper integration height needed to capture source-related transport.”

S12. 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.”
Changed as suggested.

S13. 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.
We actually agree and considered doing this. However, as we for space reasons could not provide values from all environments and enough information to put them in context in the abstract, mentioning just a few of them made the abstract imbalanced. Further,

the flux rates from the different environments are snapshots for method studies and do not represent time-representative fluxes. Hence, although we agree that the fluxes observed are interesting, we would prefer to give the methodology full attention in the Abstract while reporting the fluxes in e.g. Table 1.

S14. 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.
This part of the Introduction has now been shortened.

S15. 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.
This discussion has now been shortened.

S16. 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."
This text was removed in response to the comments above.

S17. 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.
The introduction argues that there is a need for additional methods to increase our capacity to measure near-ground GHG fluxes. Given the rapid remote sensing development and frequent questions from non-specialists if not satellites are the answer, we think to is good to briefly clarify that development of near-ground methods are also needed – in parallel with the remote sensing development. This is the reason for this paragraph, and we think it has a value as a part of the brief introductory methods survey. We have however, removed one sentence from this paragraph and tried to connect it better to the surrounding text.

S18. 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.

We have clarified that the ability to evaluate landscape GHG flux management require consideration of local conditions and depend on more flexible methods than presently available.

S19. Line 42: The phrase “natural based solutions” should be corrected to “nature-based solutions.”
Changed as suggested.

S20. 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.
We have now clarified the challenges across sectors, environment, and methodologies in a way that hopefully better illustrates the need to develop supplementary UAS-based methods.

S21. 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.
Removed “non-invasive” and changed to “should not disturb the target area”.

S22. 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.
Brief (non-exclusive) explanations with references to more detailed work have been added.

S23. 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.
We expand on this later in the Introduction and a number (iii) in the list of improvements is dedicated to this point.

S24. 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.
Added “exemplified in the results later”.

S25. 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.
The introduction has been substantially revised. See also the answer to M2.

S26. 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.
This has now been added

S27. 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.
Changed to the more specific statement "Measuring CH_4, CO_2, and N_2O simultaneously for the same target areas…" and we also express the scarcity of available simultaneous multi-gas flux measurements more carefully.

S28. 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.
We have now standardized the terminology to address this comment.

S29. 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."
This has now been fixed.

S30. 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.
We have now added a table in the appendix detailing this information

S31. Line 98: Please change "auxillary" to "auxiliary."
Changed as suggested.

S32. 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.
We have now added quantitative validation results comparing simultaneous wind measurements by the drone system and a weather station for many drone flights in different wind conditions.

S33. 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.
This is phrased as being a plausible reason and not a fact ("…which could be due to…").

S34. 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.
Clarified by changing to "To enable tracer-verified, in-flight drift correction".

S35. 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.
"In-flight drift correction" is a term which is now used to describe relative baseline drift throughout the manuscript as suggested in comment S5 above instead of "in-flight calibration" to be clear it is a drift and not an absolute calibration. The first time we mention "in-flight drift correction" we also write "in-flight drift correction using onboard reference air" and then use "in-flight drift correction" as a shorthand throughout the manuscript.

S36. 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.
We realize this statement is not adequate. It has been replaced with a clarification: "Filling the gas bag with ambient air at the start of each flight made the concentration difference between the air in the bag and the outside air as small as possible. This was advantageous because greater concentrations result in larger absolute uncertainty levels which influence the overall drift-correction uncertainty levels. If the reference and air-sample concentrations are close, they will on the other hand exhibit similar absolute measurement uncertainty which reduces the drift-correction uncertainty.".

S37. 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.
We have now added delay corrections used for each analyzer and inlet. We have also added that this is checked each field campaign.

S38. 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.
We have revised section 2.2 to clarify its content in relation to the results. We have also clarified the headings throughout the manuscript.

S39. 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.”
This has now been changed to “determining the vertical dispersion of emitted gas”.

S40. 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.
Removed the word “highly” to make the statement less strong and less broad and references have been added. We also now highlight in the following sentence that we here tried to use observations of air movement via the tracer to clarify that we did not model air movement but inferred the vertical dispersion from tracer observations.

S41. Line 163: “C2H6”
Please report the purity of the C_2H_6 gas and the uncertainty of the regulator output during each experiment.
We have now added the C_2H_6 purity in the methods section and the uncertainty of the regulator output.

S42. 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.
The sentence has now been clarified: “strong wind shear enhanced mechanical turbulence due to complex ground structures”

S43. 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.
We have now added info on the flight path selection parameters (box dimension, wall orientation, altitude spacing, maximum altitude)

S44. 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.
We used a large distance (80 m) between transects which included enough margin to be sure that the downwash did not affect neighboring transects. When flying in fog during mornings at the same site and altitudes, the volume of air disturbed could be observed and was estimated to a radius of 3-5 m around the drone when flying at these low altitudes. Also, it is evident when flying over grass that the radius of influence from the drone is not influencing air 80 m away.
A short clarification of the transect selection has now been added to the manuscript.

S45. 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.

This sentence was removed and we have in general changed the language to clarify that this example takes advantage of a nocturnal inversion in a landscape depression.

S46. 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.
See answer to question M3

S47. 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.
See answer to question M3

S48. 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.
See answer to question M3

S49. 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.
The walls typically contain altitudes from a few meters up to a maximum of 120 m. The speed of the drone is typically 3-5 m/s, which is enough that the measured wind speeds are not affected significantly by the downwash, this can be seen by comparing wind speeds at different altitudes (say 2m, 4m, 6m) which show similar wind speeds and directions. See also the answer to question S32

We have now included the uncertainty associated with wind-speed correction into the final flux uncertainty (using the size of the wind speed corrections at different altitude segments in the Monte-Carlo calculations).

S50. Lines 368–369: "while the rewetted peatland itself is a net CO2 sink, it is a source of CH_4 and N_2O 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.
We have now added the reference to Table 1 and also changed the section to be more clear when we talk about concentrations and fluxes:
"In this example, a rewetted peatland was targeted using a box flight up to 30 m altitude (Fig. 6). The concentration patterns across the walls are shown in panels A–D. The flux estimates in Table 1 indicate that the rewetted peatland itself was a net CO_2 sink and a source of CH_4 during the measurement period. The estimated N_2O flux was positive, but its uncertainty interval crossed zero, preventing a definitive source–sink classification. The spatial distribution of the major CH_4 concentration features, accounting for the wind directions at low altitudes, correspond well with streams and the wettest areas, consistent with these areas contributing CH_4 to the measured air (panel A). For example, the C_2H_6 tracer-release, close to the upwind wall (marked in Fig. 6C) near a small waterfall, yielded elevated concentrations (panel C; northwest wall) that were co-located with elevated CH_4 concentrations (panel A). Trees 3-4 m tall were present in the upper left part of Fig. 6 and may have channeled near-ground airflow along the edges of that area. Figure A1 shows the in-flight instrument drift correction for the rewetted peatland area, and Fig. A5 shows the interpolated wind speeds perpendicular to the walls."

S51. 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.
Due to turbulence and dispersion, there will not be a clear limit for the upper integration height to use based on the GHGs, that can also be contributed from areas longer upwind of the target area and thereby be detected at higher altitudes. Instead, it is based on including most of the tracer gas (with some margin), which is now better explained in the manuscript. The Monte-Carlo uncertainty analysis already includes different upper integration heights as part of its uncertainty estimate.

S52. 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.
Changed sentence to “This shows the altitude reached by gas released upwind at a given distance…”

S53. 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.
See the answer to question S32

S54. 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.
We have now included a table comparing our measurements with literature reviews (Table A1 in the Appendix).

S55. Line 489: The phrase “having corresponding flux rates those of the present study” is grammatically incorrect and should be revised.
Changed as suggested.

S56. 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.
Thanks. We forgot to change this heading to “General remarks”. (The whole section 3 is Results and discussion that is distributed across the different subsections as correctly observed.)

S57. 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.
Changed “accuracy” to “reduced spatiotemporal mismatch”.

S58. 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.

We have revised the sentence to “Future light-weight optical gas analyzers that have less drift would improve the method, but unless drift can be prevented, the ability to assess lower-level GHG-exchange in natural landscapes will depend on successful drift correction. “

We agree that multiple factors contribute to the flux uncertainty. However, as illustrated by our results, without baseline drift correction, the Strato gas analyzer concentration drifts are by far dominating over the other factors. The drift was simply much larger than the concentration changes between up- and downwind of target areas in the environments with low level natural landscape fluxes (the rewetted peatland and the littoral zone). Hence, without such drift correction, no reliable flux estimation would have been possible (which is beyond the uncertainty contribution of the other factors). After implementing drift correction, future work can focus on constraining the uncertainty by the other factors further.