Author Reply to RC2, 10 Feb 2026

RC2: 'Comment on egusphere-2025-4542', Anonymous Referee #2, 18 Dec 2025

Review of: "Optimizing Airborne Emission Rate Retrievals with Sub-Hectometre Resolution Numerical Modelling", by S. Fathi, M. Gordon & J. Hao.

By: Anonymous Reviewer

General comment:

The manuscript presents a detailed model-based study aimed at providing insights into recommended strategies for flight planning when employing mass-balance methods. It builds upon earlier works (both modelling and measurements) and is focused on various source types (dispersed area sources and tall stacks) around the Canadian Athabasca oil sands, where heavy oil industry is responsible for releases of large amounts of various atmospheric pollutants. The primary tool used in the investigation is the WRF model run at very high spatial resolution (50 m horizontal). Based on the setup evaluated in previous studies, the model delivers highly resolved spatial concentration fields based on the assumed source distributions, which form the basis of the analysis. The authors evaluate the ability to retrieve true source emissions from the measurements performed using hypothetical airborne platforms (either aircraft or UAVs), represented by extracting model-predicted fields at locations and times mimicking a way airborne platforms would be normally flown. The authors evaluate accuracy of the estimated emissions varying factors like flight strategy, measurement distance from the source and the role of vertical data density on the said accuracy, including data density. They interpret this data in order to search recommended ways to sample similar emission sources in real-world conditions.

I find the paper extremely interesting, well written, and generally well structured, with minor editing notes listed at the end of this review. I find the quality of the modelling work very high and the interpretation of data shows very good understanding and the topic. However, I also identify several major flaws that need to be addressed before the paper can be considered for publication.

PS – I ask authors not to be discouraged by multiple remarks. These are meant to be constructive and I want to underline that I think the work is of high quality and will make a valuable scientific contribution when the following concerns are addressed.

Specific major comments:

1. I believe the use of statistics needs to be reworked. The authors incorrectly describe uncertainties, using S.E. = $1.96\ \sigma / \sqrt{n}$, when S.E. should be defined without the 1.96 factor. The intent is right, but according to metrology standards this quantity is correctly "expanded standard uncertainty of the mean", or "expanded standard error" (although this is discouraged). See JCGM 2008 for details. Here also k (coverage factor) is chosen inappropriately – 1.96 is the correct value when the effective degrees of freedom are extremely large. In their study the authors have only 10 repetitions, in which k value will be higher than 2 (if the results were uncorrelated – see below).

   * This will be corrected to account for the effective number of observations discussed in point 2a below.

2. Following to the above, but much more important, is that the authors incorrectly assume that the observations within their subsets are independent, and ignore the existence of correlation. In fact it was demonstrated in the past (Gerbig et al., 2003) and in more recent works (Fuentes-Andrade et al. 2024, Galkowski et al. 2025) that the atmospheric signals of atmospheric pollutants are correlated at spatial and temporal scales large enough to be of significance to the measurements like those investigated in this work. In Galkowski et al., CO_2 emissions from elevated stacks were found to be auto-correlated down to a distance of 4 kilometers with persistent spatial (mostly horizontal) structures affecting plumes down to distances of even 20 kilometers. Although here the correlated structures are likely to be shorter (smaller PBLH, lower emission altitudes), the authors cannot ignore correlation in the signals in their analysis. Formally evaluating and including impact of correlation are likely affect the results in two major ways:

   a) the uncertainty ranges calculated for the emission rates are expected to increase, as the effective degrees of freedom (number of independent measurements) will be reduced for each distance, for which emissions were evaluated. For methods on evaluating degrees of freedom, see e.g. works cited above.

   * We have calculated the autocorrelation of the horizontal advective flux for each flight set and used this to calculate the effective number of observations (n_{eff}), following Zieba (2010, as referenced in the manuscripts listed above). Although we are still working on incorporating this analysis into the revised manuscript, preliminary results give an average n_{eff} of 8.0 for the stack sources, 7.9 for the small area sources, and 7.5 for the large area sources. This suggests that the reviewer's concerns are warranted and we will account for this in the revised manuscript.

   If we can assume that the variability within a flight set (σ) is representative of the real variability a flight would encounter under similar conditions, then we can use that value to estimate the uncertainty in a single flight estimate of E_H. Using the value of $n_{eff} = 7$ as a conservative example, we are 95% certain that a single estimate is within 2.45σ of the mean E_H value (Table G2 in JCGM for 6 degrees of freedom and a 95% confidence interval). If, for example, two flights can be flown (far enough apart in time to assume they are independent measurements), then this uncertainty in the mean is reduced by $\sqrt{2}$ to 1.73σ. We will add text to the revised manuscript along these lines.

   b) close to the emission source, due to impact of turbulence, persistent turbulent structures form that cause the cross-section mass in the plume to generate peak-to-trough structures that are advected downwind from the source (Galkowski et al.). As a thought experiment - if the speed of these structures in the studied cases was (unluckily) the same as advection speed of those structures, it might be that also the sampling of the plume at different distances was not independent, leading to potential biases in the estimations (worst-case: if the extreme peak (through) was always sampled – one would observe consistently positive (negative) bias in evaluated emission, respectively. The authors need to evaluate whether this synchronization of sampling and plume structure is responsible for the observed biases.

This is effectively describing the storage term, since the peak-to-trough structures shown in Galkowski et al. are due to build-up and release of emissions, cause by large-scale turbulent fluctuations in the advection speed. We hope that the additional explanation around storage (see response to comment 3 below) will help make this clearer. Storage is most likely related to the bias seen in Figures 3, 6, and 7 and it is discussed extensively in the manuscript.

3. If my understanding is correct, what authors call “storage” is actually a momentary turbulent flux (positive or negative) – but it is resolved in the model, so not considered by author’s definition of turbulent flux as described in Appendix B of Fathi et al. 2023. If my assumption is correct, then a more appropriate term here would be “large eddy turbulent flux”. I would like to suggest adding discussion on relationship between “storage”, turbulence and advective fluxes, as well as the effect of their interplay, somewhere in the study.

   The following discussion is added in Section 2.3. *“A significant part of the storage term can be due to eddies and circulation at scales comparable to the control volume or flight time. As horizontal winds decrease (or increase), the total concentration within the volume will increase giving $S > 0$ (or decrease giving $S < 0$). Very generally, the horizontal turbulence term (E_{HT}) estimates flux due to boundary-layer turbulence, while the storage term (S) estimates flux due to mesoscale turbulence. However, as discussed in Fathi et al. (2021), storage can also include the effects of any non-steady-state conditions. For example, changes in atmospheric stability can modify the plume’s buoyancy, moving the plume to different heights and resulting in changes in the horizontal advection speed of the plume.”*

4. The description and analysis of the role of wind speed should be expanded. Only very rudimentary information about how the effective wind speed and direction was calculated is given (L182). How the wind was calculated for each screen (or group flight) is crucial, as the results are very sensitive to biases of U. Especially in Fig 7c and 7d, I have a strong suspicion that the wind speed and direction cause the sign shift in the bias, as the overall plume structure visible in Fig. 1 turns progressively to more southerly directions.. It might be that more accurate evaluation of wind direction could help reducing that bias – it stands to reason that in those areas far downwind the wind direction (and speed) is highly variable within the screen and assumption of a single-average wind is simply wrong. Authors might either test if local wind speed information can be interpolated (UAVs or aircraft usually carry wind sensors), another approach could be to detect the central plume path (see Kuhlmann et al 2020).

   The following text is added in the 3rd paragraph of Section 2.3 (below Eq 3). “$U_\perp$ is the wind speed perpendicular to the screen at *each screen location (s, z)… Both C and $U_\perp$ are typically measured simultaneously (or close to it) during the flight, which accounts for variation in the wind pattern across the area of the screen.”*.

   While we understand that other techniques (such as satellite emissions analysis) typically use an average estimated wind speed, flight-based mass-balance analysis always uses winds measured along the flight path. Most publications of aircraft mass-balance approaches do not explicitly state this, but if this makes it clearer for a wider audience, we are happy to add this explanation.

5. Finally, I would like to point out that the results from the modelling of four simulations covering two afternoons, even after so detailed an analysis, is not sufficient to generalize the results. Statements that could be interpreted as general recommendations should be therefore avoided, e.g.: "*alone, a screen at a downwind distance of 4 km or more provides the same level of accuracy for the three types of sources investigated here (i.e. elevated stacks, small surface area sources, or a large surface area source)*". There is simply not enough proof to extrapolate these results to all cases, with local conditions (meteorological and otherwise) playing such a major role in the atmospheric transport in turbulent conditions. I therefore suggest to soften all such statements. The paper will not lose its (high) value, but transparency will be increased. I have marked some of such statements below.

   We have modified all the text outlined in the comments below (as well as other instances) to emphasize that the results are for these specific atmospheric conditions and for these specific cases. We have tried to avoid generally extrapolating from these results.

Other comments:

L59: "*… and requires induvial plumes to be well defined and separate (e.g. Baray et al., 2018).*" – This makes sense if information on individual sources is required. If information on the cluster / group of sources is sufficient, there is no such need.

This was the point of the sentence – if there is a need for individual source information, footprint models can do that, but mass balance can only do that with sufficient separation of sources. To make this clearer, we modified the text as "*Estimating separate emission rates for each source* is more difficult to do with the mass-balance method and requires induvial plumes to be well defined and separate".

L80: "*This study aims to optimize…*" – here the authors indirectly imply that the results could also be extrapolated to dust particles – or at least this is how I understand it. While it might be true, it needs to clearly be stated in the study (also in the abstract, and in conclusions) that the tracers emitted in WRF are considered gaseous sources, and that typical dust processes like deposition etc. are not considered.

In the abstract we modified the text to "were investigated to determine emission rate retrieval accuracy *for emissions of a trace gas…*"

In Section 2.2 (end of 3rd paragraph), we add "*Emissions are all treated as trace gas. These results could be extrapolated to particulate emissions (which would be expected from an area source such as an open pit mine); however, dust processes such as gravitational settling and deposition are not considered here.*"

In the conclusions (near the end of the 2nd last paragraph) we add "*Although gravitational settling of particles or deposition (of gas or particles) to the surface could modify the concentration profiles,* *especially near the surface,* these results *generally* emphasize the need to constrain aircraft measurements...".

L95: *Case Studies and Locations* – perhaps "location"? The study is concentrated around Athabasca Oil Sands and facilities there.

Corrected.

L96: "*The model is run*" -> "The model is run in an LES mode..."

This part of the sentence was deleted (see point below).

L96: "dz ≈ 12 m -- I assume this is the height of the lowest layer - please state it clearly. Also, this information is given in sec. 2.2. again (with 11.2 m stated), please see my comment there.

We removed the resolution information and moved the sentence to the second paragraph of the model description.

L108-L117: I find this paragraph hard to read, consider revising. Possibly also moving to another section, since here the focus is on the extraction of data from the model, which doesn't fit the section title. Some suggestions follow:

We have removed this paragraph in its entirety. The overview is presented elsewhere (e.g. last paragraph of Section 1) and we agree that it doesn't fit in this section.

L108: "*In this study, we …* " – erase this sentence and fragment of the next until "To achieve this" – This is said again below, with higher information content.

Paragraph deleted.

L110: "*along flight paths similar to those conducted during*" – I think it's ok to use "same" or "matching" here.

Paragraph deleted.

L110: "*The super-resolution of our model-generated atmospheric fields allow us to sample data at temporal and spatial scales of airborne measurements without the need for interpolation of model generated fields.*" – some details important for study reproducibility are missing. How was the model sampled in horizontal and vertical? Was it simply using nearest-neighbour sampling? Or interpolation was used? If yes – were absolute heights used, or pressure, for vertical coordinate?

We added this information in Section 2.3 (5th paragraph). There, the text is added as "*The data (wind and concentration) along the flight path (x, y, z, t) within the model are sampled from the model values at the nearest grid location. No interpolation is done within the grid-cell or time-step. The sampling locations are then mapped to screen locations (s, z), and* interpolation of the 2D screens is done with the kriging method...".

L121: a) neither T, p or c symbols are used later in the paper, consider dropping; b) please be specific, which moisture variable is archived? Relative humidity? Specific humidity?

a) Symbols are removed.

b) Changed to "*…water vapour mixing ratio*".

L124: "*~ 31 km*" – Please give 31.25 km exactly, this makes sense with 1:5 nesting ratio for WRF, approximation raises an eyebrow.

Changed.

L125: Was the vertical resolution forced to 11.2 m for all 40 grid levels? This is not a typical WRF configuration with hybrid model levels, so please state it clearly here. For comparisons against other modelling setups, it would also help to state how many vertical layers are present in the lowest 3km, please add this information here.

The grid spacing in the vertical is both refined and nested for the two finest domains of the model. Here we quote from the Fathi et al. (2023) Geosci. Model Dev. Paper that "*… Δz = 11.62 m for the first 40 full grid levels near the surface.*" For technical details of the model, it is better that the reader refers to that paper.

L127: Please state the spatial resolution for NARR data as well.

"*31.25 km resolution*" added.

L131: Please limit the description to sources and tracers relevant to this analysis.

Discussion of other sources not used in the analysis has been removed. The paragraph now begins with "*We use 7 modeled emission locations in this analysis, which are described in Fathi et al. (2023). The locations are shown in Figure 1. These are comprised of 4* elevated (stack) sources, two small area surface sources (surface mines), *and* a large area source (tailings pond).".

L136: 1. "in height" repeated 2. Please give exact heights of all four stacks 3. Please state their respective emissions – do they differ? Consider a table if they do. This is relevant for the analysis later.

1. Heights are added as "The existing stacks (CNRL1-4) *have respective heights of 114, 54, 30, and 54 m*".

2. We add "*Each of the 4 stacks emits at the same rate and the area sources all emit at the same rate per unit area.*".

L137: "*Each source emits a known amount*" -- 1. Is it meant that the emissions are known in real world, or prescribed in the model? Please make clear. Consider "Each source in the model emits a known amount" or "The emissions prescribed in the model E_s can be compared…"

Replaced with the former option.

L139: *"Here we evaluate three emissions scenarios: stacks"* -- "Scenarios" does not make sense in this context. "Emissions from group of emitters" are evaluated, consider this or similar.

We change this to "Here we *group the different emission source types together:* stacks (the sum of CNRL1, 2, 3, and 4), small area sources (the sum of MINE1 and MINE2), and the large area source (POND)*, and we investigate each of the three groups separately*."

L146: “*more than enough*” –> sufficient

Changed.

Figure 1, caption: “*All stacks are combined…*” -> It’s the emissions that are combined. Consider: “Emissions from all stacks are followed using a single tracer in the model. Small dispersed area sources are grouped similarly”. Also: degree symbol missing in coordinates.

Modified as suggested to “*Emissions from all stacks are followed using a single tracer in the model. Emissions from two small area sources are grouped similarly*.”

Degree symbols added throughout text and in figure labels.

L165-184: I have my doubts about whether the full algorithm in this context, as most of the components except for the horizontal advective flux are immediately discarded. See my major comment 3.

See response to comment 3 above. A discussion is added around large eddy turbulent flux and the storage term.

L182: More details need to be given on how the wind was calculated. See major comment 4.

See the response to comment 4 above.

L186: “*The terms… must be ignored*” -- Wording. Actually they must not be ignored -- because that would mean we accept a presence of potentially large bias, as the mass escapes the volume. More precisely, it is reasonable to >assume they are negligible< - provided that there is no indication of mass on the higher levels of the flight, and no deep convection was observed –half a sentence that none of this was observed is worth adding.

The text “*must be ignored*” is modified to “*can be assumed negligible (provided there is no indication of mass on the higher levels of the flight, and no deep convection is observed).*”

L191: “*a 3-dimensional prism*” – please add “or a cylinder”

Added.

L201-202: When read first, it feels like contradicting L151. I suggest removing part of sentence “*representing a well-mixed concentration in the boundary-layer*” entirely.

We have deleted this part of the sentence.

L215: This discussion of the storage is very relevant to biases demonstrated later, but not highlighted in the discussion. If it’s possible to evaluate the storage component in the previous study numerically, why not use the same method to “correct” the emission estimates here for individual cases? See also my major comment 3 and comment to L581.

* This work is underway, but it requires significant re-coding and modification of programs to run on a different platform. We hope that it will be possible to do this, but more time will be required.

L243: Please use another symbol. T is used for temperature or period of oscillation, both could theoretically be used in this study (e.g. period of circling around the source, where circular paths is discussed). In fact, temperature is also denoted as T in sec. 2.5.. To avoid confusion (especially in discussion), I strongly suggest simply Dh here (or similar).

We choose the variable ΔZ and have made replacements throughout. The delta demonstrates the differential spacing, which is distinct from the absolute distance measured by D, and the Z makes it clear that it is vertical.

L243: If T is set to 100m, then what's the point of optimizing it? Is that the base value? Please make clear

Modified to: "…*an initial value* of the vertical transect spacing… *is set to* $T = 100$ m,"

L245: Is 1 minute for turn a realistic time based on actual data from measurement campaigns?

Text added: "*(based on flight paths from Gordon et al., 2015 and Liggio et al., 2016)*".

L249: See major comment 1.

* This will be reworded in response to new stats discussion discussed above.

L249: "*Based on our estimation…*" – sigma is simply a single measurement uncertainty estimate. Please erase or simplify.

* This will be reworded in response to new stats discussion discussed above.

L250: "*When comparing…*" – Is this relevant? Please clarify or erase.

This misinterpretation of the dependence of uncertainty on the number of samples stemmed from a misunderstanding of the convergence factor and the degrees of freedom. The sentence is erased as it is no longer needed.

L255: In real world that would mean we have 10 instruments available. Perhaps add clarification whether this is meant to represent real-world situation where someone is flying 10 drones (unlikely for various reasons), or is just a method to estimate uncertainty. Related to major comment 2.

In the previous paragraph (where we first introduce that there are 10 flights in each set), we add the following text, "*For each set of 10 flights, each subsequent flight starts 1 minute later than the start of the previous flight. This offset is added to investigate the uncertainty in the estimated emission rate due to turbulent fluctuations with time scale on the order of 1 to 10 mins. Through the statistical analysis of multiple flights, we can also assess how effective repeated flights (or multiple sampling with 2 or 3 UAVs or aircraft) are in reducing the measurement uncertainty in the emission rate estimate.*".

L256: "*horizontal aircraft speed is randomly offset…*" Is this number according to real data? Based on my knowledge the variability of speed in UAVs in automatic mode is usually within 0.1 m/s at an altitudes up to 200 meters. When flown "manually" this value increases somehow (0.5 m/s – data from actual measurements) but having 3 m/s variability is unlikely, as these sort of conditions are not flight-permitting. For small aircraft change of wind speed by 3 m/s at higher altitudes is perhaps more likely, but then the momentum preservation law will prevents that to be >completely< random. And for larger aircraft this is simply impossible. Finally, the accumulation of the error is an entirely wrong assumption as either the automatic guidance systems, or the pilots will prevent "drifts" of the desired speeds and altitudes. This needs to be addressed, either by recalculating the procedure entirely, or by demonstrating that this does not lead to major biases in estimation.

After the "3 m/s" sentence, we add the text "*These random offsets, although potentially exaggerated compared to the variability of real flight speed or position, were found to produce visually similar flight paths compared to paths shown in Gordon et al. (2015). Given that this is a very subjective comparison, we investigate the effect of reduced offsets in Section 3.1.2 below. Although the analysis demonstrates that the effect of the randomized offset is small (<7% change in the average horizontal advective flux),* the temporal and spatial offsets ensures that each of the 10 flights (for each D and ΔZ value) is distinct but generally sampling the same meteorological and emission conditions."

We have added a new Section with a new figure (which will be renumbered in the revised manuscript).

"3.1.2 Sensitivity to Random Offsets

As discussed in Section 2.4, at each 1-s timestep of the flight, the horizontal aircraft speed is randomly offset by a Gaussian random number with a standard deviation of 3 m/s, and the vertical position is offset by a Gaussian random number with a standard deviation of 1 m. To assess the sensitivity of the results to the scale of the offsets, we rerun the analysis for the set of flights on Aug 20 flight (at 16:20) at $D = 6$ km with both horizontal speed and vertical position offsets simultaneously modified by a factor of 0 (i.e. no offset), 1/3, and 2/3. The resulting changes in estimated E_H/E_S and the variability (σ) are shown in Figure X (in addition to the 3 m/s and 1 m offsets used throughout the study). Using an evenly spaced, elevation-following grid with no offsets give a value of $E_H/E_S = 1.10$ with $\sigma =$28%. Adding a small amount of random offset (a factor of 1/3) to the grid increases E_H/E_S to 1.15. The offsets higher than that (factors of 2/3 and 1) both give $E_H/E_S = 1.17$. The difference in variability between flights within the flight set is < 1%. Hence, although there is a slight difference between no random offsets (even grid spacing) and the inclusion of random offsets (E_H/E_S of 1.10 versus 1.15), the results are not sensitive to the size of the offset over the range of values investigated here."

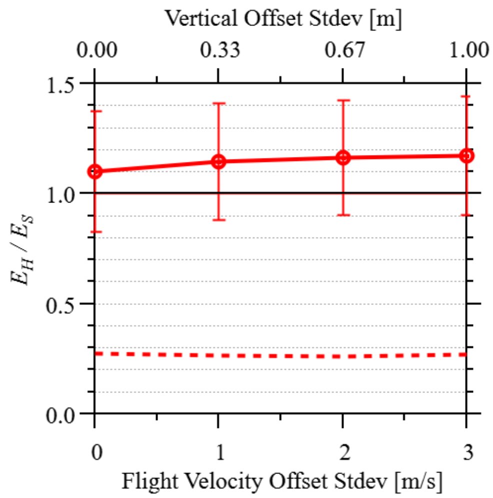


Figure X. The variation in the ratio of horizontal advection flux (E_H) to the known emission rate (E_S) with change in the random offsets along the flight path for the Aug 20 flight set starting at 16:20 (labelled following Figure 3).

L268: "*These screens are flown…*" - I think "flown" is confusing in this context - if the full screen are output at a single time, then perhaps it's better to use "sampled" here.

Changed.

L270: *We refer to these flights and the calculated emission rate values as "instantaneous".* ◊Linked to above; I suggest: "... to these calculated emission rates as instantaneous".

Changed.

L274: Here and throughout the text, I feel it would be beneficial to differentiate between a "single flight" and "10 subsequent flights". Consider "formation flight", "group flight" or even "echelon flight".

We prefer the term "flight set" or "set of 10 flights". This is added in Section 2.4 where the use of 10 flights for statistical investigation is first discussed, and the language throughout the manuscript is changed to refer to the "flight sets".

L281: "*turbulence and the and stability*" –repeated "and"

Corrected.

L283: "*model runs (using a criteria of 0.25 > Ri > –0.25 for neutral conditions)"* -- This is not a typical interpretation: please add a reference for range given if available. I've never encountered values below zero to be interpreted as neutral. Usually flows with Ri_b < 0.25 are treated as turbulent. See Stull, "Introduction to Boundary Layer Meteorology", Sec, 5.6.3. Fig 5.19., for example.

This was a mistaken interpretation. We have removed the text in the brackets and added the qualifier "...demonstrates that the conditions are *always turbulent and likely* unstable...".

L284: *"Temperature rises consistently during both afternoons, rising approximately*" – "rises, rising" - replace second with "by"

Changed.

L288: -- way >to< sample

Fixed.

L291: *"eliminated"* – should be "eliminating"

Fixed.

L305: "*calculation of .... means ...*" -> "calculating the screen length... results in a screen length that is..."

Modified.

L340: I think the critical point here is the temporal scale of the changes - these occur on high time frequencies, high enough to cause variability in estimated emissions between flights separated by 1 minute. The "storage" term here is a manifestation of the turbulent eddies transferring mass through the screen at highly variable rates. See my major comment 3.

We agree. We hope that the added discussion in response to Comment 3 has addressed this point.

Figure 3. a. This figure is only for stacks - and it should be noted in the caption. b. Red symbols are not mentioned - please add where appropriate. c. Please add Panel A/B/C/D references next to appropriate dates.

a) Added "for the stack sources". b) The red symbols were mentioned in the 3rd line, but we started a new sentence with "The red circles..." to make that more clear. c) Panel identifiers are added next to the times in the figure caption.

L376: "The extrapolated concentrations..." - The way this is written it suggests ("average of...") that more than 1 sampling was compared, but the text above says it was only the "first single instantaneous flight" was sampled and compared against the original screen. Please clarify if only one instance was compared, or the comparison was done to 10 flights).

This has been removed due to responses to the first reviewer's comments about kriging analysis.

L378-380: The authors correctly spotted this effect for vertical motion but didn't consider it for horizontal – see major comment 2.

As outlined in the response to Comment 2, we consider horizontal advection fluctuations in the storage term.

L393: "*Generally, flying...*" – I'm quite certain this is due to source being below 150 m and extrapolating without "seeing" most of the mass. It's quite clear from Fig 4, where at 2km the tracer concentration extrapolation < 150m underestimates concentrations in 14/16 cases (most of those quite clearly). Would require to look in detail on the model output over a longer period (if the output is available for several hours, then would be a good addition) to confirm without any doubt, but it's quite logical - 2 km is not enough distance and time for the model to assure updrafts move the mass above 150m.

We agree with the reviewer, but it is not clear if any modification to the text is needed here.

L408: "*This transition from overestimation at small spacing to underestimation at larger spacing could be due to vertical movement of the plume opposite to the sampling direction, resulting in transects missing the plume centre at larger spacing.*" – Again, authors think only of vertical, but not of horizontal. See major comment 2.

As outlined in the response to Comment 2, we consider horizontal advection fluctuations in the storage term.

L413: sometime -> sometimes

Changed.

L435: "For the small area sources (Figs. 6a-d), the instantaneous flight horizontal…" – see comment for L393, same effect.

As above, it is unclear if modifications are required or if this just a general comment.

L445: "The relatively good agreement between instantaneous and non-instantaneous estimates implies that vertical motion of the plume does not result in over- or under-sampling." It is also partially because for large source area the effective distance from the source is much larger – what is given is calculated to the >edge< of a large source, so that the emission-centre point is much further upwind (Fig 1), and the effective signal is from areas well-mixed (far away) and not well-mixed (close to measurement). This deserves some expanded discussion as well, with "effective distance" or "distance to centerpoint" rather than distance to edge used for x if the comparison is to be fair.

The following text is added to Section 3.2.1 (at the end of the 2nd paragraph) "*Additionally, since D is defined as distance from the edge of the area source (as is necessary to sample the entire source area), emissions from the upwind side of the area source will have had more time to mix relative to the emissions from the downwind side of the are source. Hence, it would be expected that large area sources have smaller uncertainties for similar D values relative to small area sources.*".

L458: "*large area source would show substantially less uncertainty relative to a single flight sampling small area sources.*" – delete "would", no need to hypothesise.

Changed.

L459: "*we expect*" – as above, "we estimate"

Changed.

L467: "*instantaneous area source flights*" -- instantaneous sampling maybe? See comment to L270.

Changed.

L477: "*however, this is…*" - Clearly something else negates this effect. Bias in wind speed or direction could be explained, especially since the model clearly predicts a large-scale change of wind direction, shifting to more southerly winds as the plume goes northwards. See major comment 4.

See response to Comment 4.

L484: "*For this source…*" – More precisely it should start with "for this source and these atmospheric conditions". See my major comment 5.

Text added.

L508: "*Scaling*" – this section doesn't have a corresponding entry in Methods. Reorganize, with expanded description of the method (and motivation for its use) moved to Section 2.

Text from Section 3.3 is moved to a new Section 2.7 in the Methods and it is rewritten to expand the description of the method and the motivation for its use.

L511: "*wind speed*" – horizontal, or also using W component?

Text "horizontal" added.

L511: "boundary layer heights are taken as…" –What was the method for PBLH evaluation here? State it clearly. Also, authors assume that PBLH did not change significantly – see comment below.

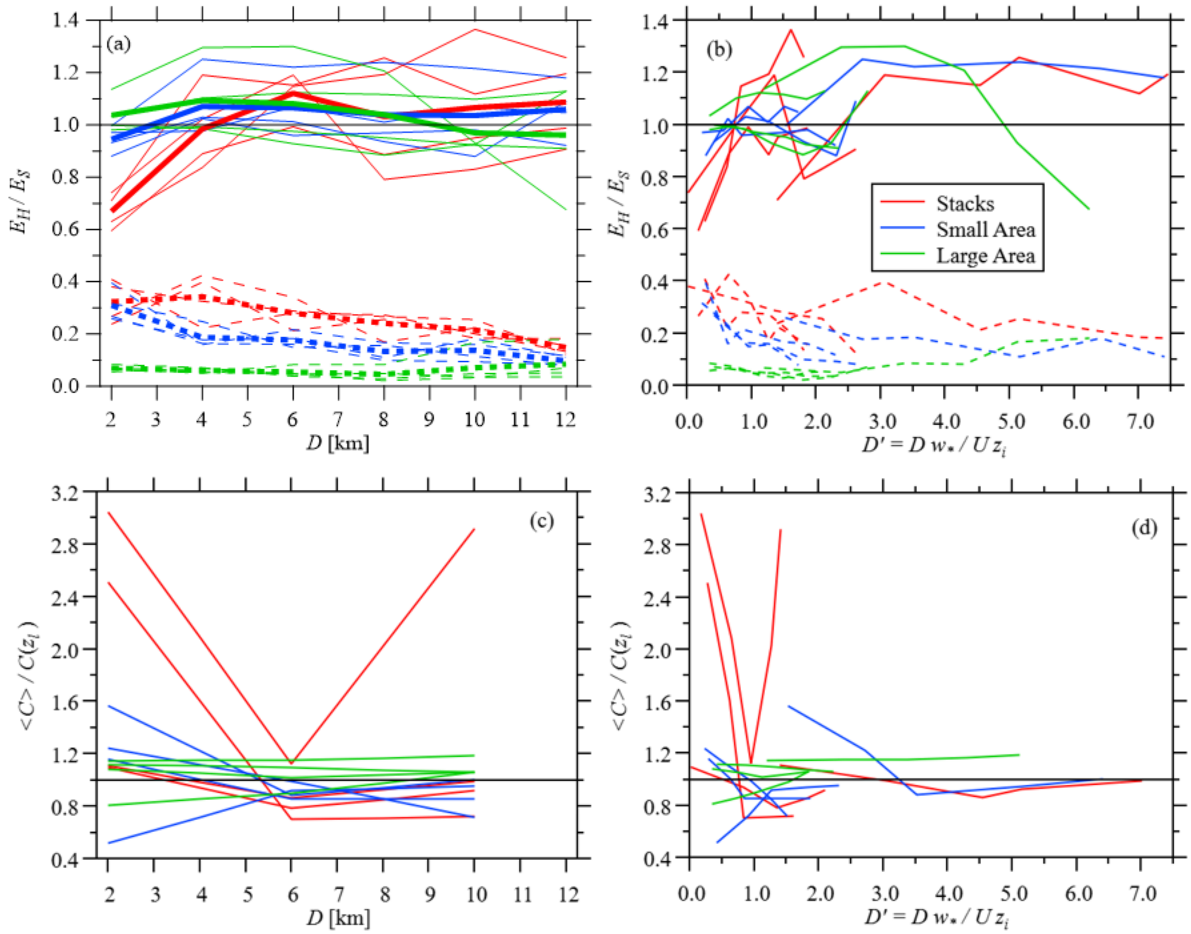
Following the comment below, PBLH is extracted form the model and this evaluation method is removed.

L516: "*The results are not collapsed…*" - Would it be better if actual PBLH was taken into the account, I wonder? High variability is possible - 16 UTC and 17UTC corresponds to approximately 9 and 10 local time in Alberta - PBLH development can be quite dynamic at this time, changes of 200 m per hour are typical for mid-latitudes in summer, so if the longest analysed time periods are 30 mins, then change of 100m is over 20% if the zi. I assume the PBLH field from the model is available, please give numbers here and discuss. Consider also the plume extent – single point values might not be representative.

We have extracted PBLH from the model. We add the following text to the new Section 2.7 (Scaling), "*The boundary-layer heights (z_i) are output from the model at the source locations. An average value of z_i is determined for each flight set and the effect of boundary-layer growth is discussed below.*"

In Section 3.3, we add the following "*Here we use an average z_i value for each flight set but note that this value can vary significantly during the flights. For all Aug 16:20 flight set, the model value of z_i is constant (824 m). It then grows linearly during the 17:20 flight sets, from 867 m to 1315 m (for the 40-min duration of longest flights at $D = 12$ km). During the Sep 2 flight sets, z_i increases from 528 m at 16:20 to 1078 m at 17:00 and then decreases from 1078 m at 17:10 to 952 m at 17:50. Hence, this normalization should be interpreted with some degree of caution. Although the variation is relatively small in most cases (more than 50% of the flight sets show less than 5% change in z_i during the flight durations), in some cases the increase in z_i can be up to 100%.*"

The modified Figure 9 (b,d) are shown below. Although this analysis has changed the figure slightly and introduces some uncertainty into the results, the interpretation of the results does not change significantly and very little of the following discussion is modified.



We also note that MDT is 6 hours behind UTC, so 16 UTC corresponds to 10 local time, although this doesn't change the reviewer's point.

L527: "*for the Aug 20 17:20 stack flights at D= 10 km (see Fig. 4b), and it is unclear what would happen at further downwind distances for that flight.*" -- Large-scale change of wind direction is probably at play here and this breaks the method assumptions – see major comment 4.

Please see the response to comment 4. There is no assumed wind direction.

L545: "*Hence, based on the average estimate of E_H/E_S alone…*" – This reads as a general comment. I disagree that the evidence presented support this. See my major comment 5.

We have added the qualifying text "*for the cases studied here*".

L549: "*variability is seen instantaneous results*" – "seen in"

Corrected.

L555: "*Hence, 3 flights can be flown at $\mathcal{D}$= 4 km in the same time it takes to fly one flight at $\mathcal{D}$ = 12 km. Taking the average of these 3 flights, reduces the uncertainty by a factor of 0.58 (1/√3). Hence,* …" – numbers flawed as based on wrong assumptions of statistics. See major comment 2. Also: "hence" is used twice.

* We have corrected the double "hence" and will modify the statistics to account for n_{eff} as discussed above.

L561-567: Again, results are based on four eddy realizations. I find the sample size too small to derive such conclusions. See major comment 5.

We make the following edits here:

"…the results show that, for these cases, reducing the transect spacing…"

"For *the* area sources we investigate here, the variability…"

"For *the* small area sources *we investigate here*, increasing…"

"for the large area source *we investigate here*, increasing…"

L579: "*However, the results do demonstrate the potential to improve emission rate retrieval by accompanying any flight campaign with a strong modelling effort.*" - While I agree this statement is true in general, I do need to point out that this is not demonstrated in this study, as the results were not compared to actual measurement data here -- emission estimates were not "improved". Consider erasing. See also comment below (L581).

This is modified to "… the results *suggest that* emission rate retrieval *could potentially be improved* by accompanying any flight campaign with a strong modelling effort*, at least to help with understanding of the plume dynamics and behaviour*."

L581: "*Reanalysis data combined with tracer release can be used to mimic flight actual patterns and estimate storage and release during actual flight time, thus reducing the most substantial uncertainty in the emission rate estimation.*" – If I understand the authors' thought here, the model would require that to simulate exactly the same plumes, same eddies, as in reality. Do authors believe this is possible? The eddies are stochastic, and while can simulate realistic conditions, it's unlikely that we will reproduce exactly the same eddy pattern. And if we can't, then can the model help us correct estimations if we only have a single, or maybe two flights (as we often do?). Or does it only allow us to estimate uncertainty more realistically? Please comment.

This sentence is deleted.