

## **Reviewer #1**

*We thank reviewer #1 for their contributions to our manuscript.*

**Reviewer #2** – The authors addressed the comments and presented arguments that failed to convince this reviewer to support the use of eddy covariance flux systems in extremely heterogenous urban settings.

As mentioned in the previous set of comments, this reviewer acknowledges the effort required to run this set of flux monitoring sites, as well as the time required to postprocess and analyze the collected data. However, he cannot endorse the installation of flux systems in urban sites that do not meet the working assumptions of the method.

It would be irresponsible to recommend the installation of urban flux systems, taking the Indianapolis Flux Experiment (INFLUX) as example. Horne et al. challenge the consensus reached by researchers with experience running urban flux systems during the elaboration of the IG3IS Urban Greenhouse Gas Emission Observation and Monitoring Good Research Practice Guidelines (GAW Report No. 275), which was coordinated by the World Meteorological Organization.

*We understand the reviewers' concerns regarding the data presented at the three mixed urban sites and, to the best of our abilities, have attempted to articulate both their methodological philosophies and the philosophy taken by the authors here. We also note that the authors of the GAW report did not reach consensus (coauthor and project PI Davis was a coauthor of the original text of the GAW report whose input was perhaps ignored in revision), that the GAW report is not a peer-reviewed publication, and that disagreement in the scientific literature is, in fact, necessary for rigorous scientific progress on difficult topics.*

The manuscript indicates that the urban flux systems were mounted in existing communication towers that had been previously used to measure mole fractions for atmospheric inversions as part of the same project (L408-410). This suggests that the monitoring sites were not chosen based on the eddy covariance method requirements. Both methods have different measuring requirements; a site used for one method may not meet the requirements for the other.

The revised manuscript defends the validity of INFLUX urban sites, recognizing that the flux measurements fell short of meeting the method's basic assumptions, but arguing that this is irrelevant and that such an issue can be addressed when the data is interpreted.

Statements such as “Finally, the flux data are flagged based on a threshold of N standard deviations from the mean, where is a site-specific number chosen to keep flux magnitudes within geophysical limits” (L294-296) suggest that the postprocessed fluxes may have been artificially manipulated to yield plausible readings.

*The data processing is documented and transparent. All fluxes are reported. Use of flagged data is enabled. Simple flags, such as standard deviation flags, can be easily recreated and manipulated by the end user based on individual research needs. We have added a sentence (lines 314-317) based on Reviewer 3’s suggestion, explicitly stating the limitations of the quality control processes shown here.*

The comments that challenged the location of the urban flux systems were not properly addressed. To defend the urban flux measurements, the authors made reference to three recent papers that they had published. However, the publication of a paper does not guarantee that the results are valid. Furthermore, they merged the figures showing the footprints of each individual site in the original manuscript (Figures 7,8,9) into a single figure with four panels using colors that no longer clearly depict the sites’ features (Figure 7).

*Our primary argument for these data is not prior publication. Our argument is that urban systems are heterogeneous by nature. Rather than insisting that we collect measurements only in places that adhere to our idealized theories about how the world should look, we advocate making measurements in the real world and learning how to interpret those data.*

*The previous publications yielded important and interpretable results thus empirically show the value of the measurements.*

*The new figure (Figure 7) was created to show both the larger spatial extent of the EC footprints at each of the mixed urban towers and a zoomed-in image of the nearby landscape. We believe this figure does a better job of both visualizing the large spatial extent of these flux towers and displaying the landscape variability surrounding each tower.*

Also, the authors argue that the incomplete instrumentation in some eddy covariance systems was due to a lack of resources, which is hard to believe for such an extended and ambitious study (L503-513). They only started measuring the four components of the solar radiation budget at two sites near the end of the eleven-year measurement period. Similarly, the urban sites did not include measurements of basic meteorological parameters needed to postprocess and correct the measured fluxes. According to the

authors, the postprocessing software includes a routine that convert the virtual temperature obtained from sonic anemometers to real temperature.

*We are sorry that the reviewer doesn't believe what we have written. Choices must be made in any experiment. We chose to measure what we felt would be the most valuable contributions to our understanding of the urban system given limited resources.*

*Regardless of our disagreements we thank reviewer #2's input and perspectives. We hope that we have documented reviewer #2's concerns sufficiently for any investigator reading this paper or using our data to be aware of these concerns.*

**Reviewer #3** – The manuscript is well written and informative, providing a detailed overview of all flux measurements available in INFLUX. I think the authors have addressed the uncertainties concerning how to analyze and interpret urban EC measurements well and have responded to the critical concerns of previous reviewers in detail. The openly available and well-documented urban EC measurements are highly relevant for the growing urban land surface modeling community. Thus, even if there remain some methodological uncertainties, having more urban EC measurements from different urban environments will help the modeling community further advance their capabilities and understanding of the urban ecosystem. I have only minor comments.

*Thank you for these comments.*

L79: It would be worth mentioning human respiration here (or later) as one of the components measured with urban EC and how it impacts the interpretation of CO<sub>2</sub> measurements.

*Thank you for the helpful comment. We have added a note to line 101 that respiration includes both heterotrophic and autotrophic processes. We have also added an estimate of the CO<sub>2</sub> flux from human respiration (line 103).*

L101: Is the abbreviation GHG explained? It is quite obvious, but it should still be defined once in the text.

*Thank you for catching this. We have clarified at the point of its first mention in line 128.*

L109–111: Is it relevant to point out values only for methane emissions in the introduction, given that your setup does not include these? Why not also provide ranges for CO<sub>2</sub> fluxes from other studies?

*This is a good point, but while we are not directly measuring the covariance flux of methane, the INFLUX project also has a substantial component devoted to city-scale methane emission, but quantified via alternative methods (typically inversion modeling). Thus, we thought it was appropriate to mention methane, given its critical importance to INFLUX and to anthropogenic GHG emissions as a whole. We have removed the reported values for methane (line 141).*

*Suggested text. We agree that those two values were overly specific, given the paragraph's purpose. We deleted those two numerical values for methane fluxes from the introduction.*

L119: Make sure that all CO<sub>2</sub> notations are written with the proper subscript.

*Thank you for catching this. Hopefully, we have now addressed all of these mistakes throughout the document.*

Table 1: The Site Description column could be systematized for a cleaner appearance. In particular, articles such as “the” or “an” could be removed from the beginning of the sentences.

*Thank you for the helpful comment. We have attempted to consolidate the site description column for greater concision.*

Check the  $\mu\text{mol m}^{-2} \text{s}^{-1}$  units in each figure. Sometimes you have the correct “ $\mu$ ,” and sometimes only “u.”

*That is a good catch. We have fixed all of the units so they are correctly marked as  $\mu$ .*

Figure 5: It is a little misleading that the colorbar ranges change between sinks and sources. Consider adding a note to the figure caption to alert readers to the varying colorbar ranges.

*Thank you for the comment. We do not wish any part of the plot to be misleading and have tried to make the color bar more straightforward.*

Figure 6: Could you enlarge the legend font? The x-axis label “Hour (LST)” is also not fully visible. The y-label on the right-hand side currently reads “H/LE Flux ( $\text{W m}^{-2}$ )”, which seems intended to indicate that these are the ranges for H and LE. However, since the Bowen ratio (H/LE) is not shown, this label could be misleading. I suggest changing it to something like “H and LE ( $\text{W m}^{-2}$ )” for clarity.

*This is a great point! Thank you for pointing that out. We have changed the y-axis label to clarify the energy fluxes (rather than the Bowen ratio) and enlarged the legend labels.*

**Reviewer #4** – This paper introduces important work on monitoring eddy-covariance fluxes across a large number of sites within the urban region of Indianapolis. The authors have made a commendable effort to explicitly describe the measurement techniques, site characteristics, and all data-processing steps—including filtering and quality-control procedures.

As noted in the manuscript, establishing and operating urban EC sites is inherently challenging, yet essential. Scientists working in urban environments are investigating highly heterogeneous landscapes with diverse sources and sinks.

While there are some drawbacks reported—such as complications arising from roughness elements or flow distortions typical of urban settings—the authors already acknowledge these issues. These examples can serve as practical lessons for researchers working on real-world urban projects, helping them to anticipate challenges, calibrate models, and interpret fluxes in their own local contexts.

*Thank you for these comments.*

#### General Comments:

I would like to raise a concern regarding the title “Urban Eddy Covariance – The INFLUX Network.” While I understand the authors’ intention to frame the dataset and this paper within an urban context, several of the included sites are agricultural. Although these sites are geographically located within the broader Indianapolis region, their land use characteristics do not align with the concept of an urban eddy-covariance network, in my consideration. To avoid potential misinterpretation, I recommend reconsidering or refining the title so that it more accurately reflects the diversity of site types within the INFLUX network.

*We agree with the reviewers' comments on the title and have changed it to “The INFLUX Network – Eddy Covariance in and Around an Urban Environment,” which we believe accurately conveys that measurements are taken in both the city and the surrounding area.*

Lines 41–79: Furthermore, the entire section is not well-written and with weak structural coherence. While the underlying topics the authors aim to develop are clear, the narrative lacks consistency, and the flow of ideas is difficult to follow. I strongly recommend rewriting these paragraphs. Additionally, this section could be strengthened by acknowledging the valuable work carried out within major flux-monitoring networks such

as FLUXNET and ICOS, including the growing body of urban eddy-covariance measurements from ICOS sites. Projects such as ICOS-Cities are also highly relevant and would provide useful context for situating this study within current efforts in urban greenhouse-gas monitoring and analysis.

*Thank you for the comments. We have mentioned large-scale efforts like FLUXNET and ICOS-cities, as well as worked to improve the structural coherence of these sections (lines 53-56), and hope our edits have enhanced the overall narrative. We have also moved a later introductory paragraph to the beginning of the section to frame some of the current efforts ongoing, which have made valuable contributions and advancements in our understanding of urban surface atmosphere interactions.*

Line 289: The description of the flagging criterion raises an important question. If the two-week moving window happens to contain generally low signal strength values or limited data coverage, the mean of that window may itself be unrepresentative or systematically low. This could result in questionable or inconsistent flagging outcomes. The current sentence leaves room for uncertainty about how the method behaves under such conditions. I recommend clarifying how the algorithm handles periods with sparse data, persistently low signal strength, or gaps within the two-week window, and whether any safeguards or minimum data thresholds are applied.

*We agree with the review's comment and have made an effort to articulate the overall simplicity of flags, such as the signal-strength and standard-deviation flags (lines 317-320). It is without question that the flagging procedure is imperfect and can potentially flag realistic flux measurements. However, this is potentially true of any quality control procedure, which is why we have left it to the end user to decide which data they would like to remove.*

#### Specific Comments:

Lines 14–15: The revised text still does not adequately address the concern raised by Reviewer 1. The current wording continues to give the impression that all eleven flux systems contributed equally and concurrently. The distinction between long-running sites and short-term deployments remains unclear.

*We have added the phrase “for varying deployment periods” (line 14) to clarify this. We would like to point out Figure 2, which clearly shows that these sites were not all deployed concurrently.*

Line 22, I would suggest that the word “instrumented” is unnecessary here and can be removed. Additionally, “Indianapolis” is already mentioned later in the same sentence.

*We have removed the specified word to improve the document's overall flow.*

Line 29: Since eddy-covariance measurements are, by design, conducted within the surface layer it is unclear what distinction the authors intend to make here. Are you referring to surface–atmosphere exchange in general, or to measurements taken at typical EC tower heights (i.e., above the roughness sublayer)?

*We have removed the phrase “Near-surface” to avoid confusion.*

Line 41: The term “challenging” is ambiguous in this context. It is not clear in what sense the micrometeorological methods are “challenged.” If the authors mean that certain underlying EC assumptions (e.g., stationarity, horizontal homogeneity, or planar flow) are violated at times—as they discuss later—this should be stated.

*We have reworded much of this and the following paragraphs to hopefully ameliorate the potential confusion.*

Line 42: The reference to “homogeneous systems” requires further elaboration. As written, it is unclear what types of systems the authors are contrasting with urban environments and how this comparison relates to the eddy-covariance methodology. If the intention is to refer to ecosystems such as forests or grasslands—where surface properties and source–sink distributions are relatively uniform—this should be stated explicitly or include one or two references.

*We have followed the statement “horizontally homogeneous” with the mathematical definition typically associated with it (line 60-62).*

Line 90: Please elaborate on what AmeriFlux stands for, as some readers may not be familiar with the network. A brief explanation of its purpose and scope would improve clarity for the audience.

*We have included a statement “in the Americas flux tower network (Ameriflux, Novick et al., 2018)” (line 116-117) in the sentence to elaborate on what Ameriflux exactly is. We believe this reference expands on the purpose of Ameriflux.*

Line 102: This sentence could be further elaborated. For example, you may wish to mention relevant initiatives such as NetZeroCities in Europe or the Covenant of Mayors, which are actively engaged in similar mitigation and adaptation efforts.

*We thank the reviewer for the suggestion and have added the mentioned initiatives and relevant citations (lines 129-131).*

Line 107: You may also wish to reference the work of Stagakis et al. (Atmospheric Environment, 2019; DOI: 10.1016/j.atmosenv.2019.01.009), which deals with urban eddy-covariance measurements and further partitioning of source–sink contributions.

*Thank you for the comment. We have included a sentence from the recommended manuscript (lines 137-140).*

Line 119: You may consider adding a reference to the Oxford Stringency Index to strengthen this point. The Nicolini et al. paper provides a citation.

*We have included the OSI as an example in the statement “stringency index (e.g., the Oxford Stringency Index)” line 45.*

Lines 115-124: To further support the arguments made in this paragraph, you may also consider referencing the paper with Jongen et al., 2022, DOI: 10.1029/2021GL096069, which provides a relevant implementation of urban eddy-covariance towers with Evapotranspiration measurements.

*We appreciate the reviewer's suggestion and have added a sentence mentioning the recommended manuscript (lines 47-48).*

Line 131: Even though the authors responded to the earlier Reviewer 1 concern regarding the use of the term “tall towers” and adjusted the wording elsewhere, the term still appears. I agree with Reviewer 1 that this terminology is problematic, particularly in urban environments where flux towers typically exceed the mean building height only by a factor of 2–5. They may consider benchmarking their terminology against examples such as the 144 m urban flux tower described in Matthews et al., 2022 (DOI: 10.1016/j.atmosenv.2022.118941).

*Thank you for catching this mistake. We have removed the adjective “tall” when describing the KUOM tower.*

Table 1: The site description section contains substantial detail, but much of it is embedded in the text and not easily comparable across sites. I recommend adding an additional column—e.g., specifying the type of grassland, or providing a quantitative proportion of adjacent land-cover classes within a defined radius (e.g., 100–250 m), like table 4.

*We thank the reviewer for their comment, but have chosen to leave Table 1 as it currently appears, given the substantial amount of information already displayed and the requested information being available in other tables (e.g. Table 4), figures and throughout the text.*

Line 212 & 282: These sentences would benefit from rewriting for greater clarity.

*We have rewritten lines 232 and 303 as suggested by the reviewer to improve overall clarity.*

Line 354: There is an incorrect spacing at the beginning of this paragraph.

*Thank you for catching this error. We have corrected the incorrect spacing.*

Lines 356–357: The statement that the towers were placed “close to, but not directly within, the actively managed crop fields” is unclear in purpose. Could the authors please elaborate on why this distinction is important? From a flux-footprint perspective, the tower location determines the source area influencing the measurements, and fluxes from the surrounding fields will still be captured if they fall within the footprint. As written, the sentence does not make its relevance or implication for the measurement strategy sufficiently clear.

*This sentence is included to state that, given that the field was under active management when working with landowners, we placed the flux towers in narrow pathways immediately adjacent to the crop fields. In some cases (e.g., a linear path along the crop boundary), if the wind is oriented in a particular direction, the fraction of the flux footprint representing crops can drop significantly. This was taken into account, as the flux footprint fraction flags used for the agricultural flux towers were. We have attempted to improve the clarity of this sentence to communicate this point (lines 379–381).*

Lines 395–396: The authors mention that these towers are also being used in ongoing analyses of sensible and latent heat fluxes in NWP models. Since this appears to be a

recent addition to the manuscript, I recommend elaborating slightly on how the flux data are being used in this context.

*We agree with the reviewer that more information on the ongoing investigation is merited. We have chosen to remove this sentence as the research is ongoing or currently in peer review.*

Figure 6: The legend font size is too small and should be increased for readability. Additionally, the hour (LST) index at the bottom appears misaligned and is partially overwritten by the Figure 6 caption text in the manuscript.

*Thank you for the comment. We have corrected the figure and enlarged the overall text.*

Line 468: The suggestion to “interpret these sectors independently, potentially thought of as two distinct flux towers” is ambiguous. In eddy-covariance analyses, a single site is not conceptualized as two separate towers. Instead, standard practice involves source–sink partitioning or distinguishing flux contributions based on wind-direction sectors and footprint characteristics. While the intended meaning is understandable, this sentence should be rephrased to reflect established EC terminology and to clarify that the authors are recommending sector-based analysis.

*We have decided to remove this sentence because we agree with the reviewer that it is potentially confusing and does not align with the existing EC terminology.*