Manuscript ID: egusphere-2024-2041

Verdict: Mandatory Major Revision

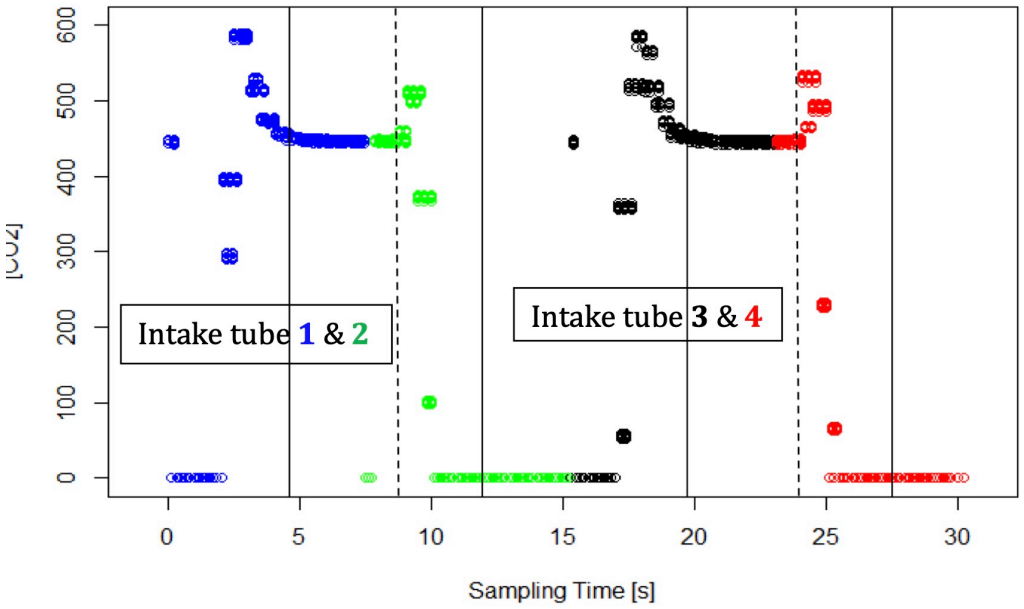
Article Summary and General Remarks: The manuscript entitled "A simplified system to quantify storage of carbon dioxide, water vapor and heat within a maize canopy" highlights a novel set-up to quantify the surface energy energy budget, including storage fluxes, using a vertical array of inlets (2 within the canopy and above the canopy) that measures H2O and CO2 sequentially in a 30 second cycle (or a minute—details about cycling are conflicted in the article (see below)). The aim is to illustrate the utility of the set-up and to highlight the importance of terms often ignored within the energy budget closure at the surface. The introduction lays out the previous work well leading up to the 2023 experiment in Tennessee. The Methodology/Configuration discusses the instrument set-up and measurements conducted reasonably well; however, there are some areas in section 2 that need explanation and the discussion of Figure 2 and how its presented, especially with respect to the vertical line references, is confusing. Section 3 discusses the experimental set-up. In this section there is text dedicated to the Experimental site and the calculation of storage fluxes. The calculation of storage fluxes should be moved to methodology since the authors are talking about a method they are using to calculate storage fluxes. There is no discussion in Section 3 about instances of precipitation, the diurnal wind structure, etc., which would be important for providing context about the weather variability during the two-month period. It highly recommended that the authors describe the weather conditions over this two-month period, and, if possible, subsample days without precipitation to understand impacts on the canopy gradient structure. Section 4 goes over the results and discussion of results. A subsection is dedicated to CO2 storage, H2O storage, and the budget. While the first subsection is written reasonably well, the other two sections fall short of a rigorous analysis of the results with a flawed interpretation (see examples below). Furthermore, if the authors are interested in promoting the novel instrument design, then this paper ought to delve deeper into the measurement capability rather than provide just diurnal plots with limited interpretation of the results (see below). Finally, the conclusions in the final section does not summarize the key findings, shortcomings in the method, or a path forward to integrate the novel set-up as a mainstream measurement option. Below are the key highlights of the study laid out by the authors:

1. A new multiport profile system simplifies examining the CO2 and H2O storage terms
2. Neglecting canopy storage terms leads to inaccuracies in crop energy balance closure
3. Energy closure is improved if canopy storage is considered
4. The multiport system is necessary for eddy covariance-derived fluxes over maize canopy

It is clear that the first two highlights are sufficiently covered, but the second two points are not adequately addressed in the present form of the manuscript. For instance, as a reviewer, I do not have an impression of an improvement. I see results highlighting the vertical structure of CO2 and CO2 storage, but not a clean budget calculation or comparison with and without CO2 storage. Given the data available to the authors, I believe this should be possible. The last point is also a concern. EC is conducted with a fast response wind and temperature/tracer instrument on the order of 10Hz. The storage terms are not calculated as done with EC techniques and instead support the EC calculations of other terms that make up the surface budget.

In light of the comments above, I recommend a mandatory major revision before the manuscript is eligible for publication.

Major Comments:



Caption

Figure 2: In the caption, the authors note that the vertical lines represent stable periods where measurements are suitable for recording. The vertical lines and how they are placed in the figure does not seem to support the text in lines 148-151. The text suggests that feeding in a known amount of CO2 (430 ppm) and looking stability of these measurements define the periods of acceptable data. Based on Figure 1, intake tubes 1 and 2 (3 and 4) are coupled with a smooth transition from one intake tube to the other, while rapid increases and decreases occur as the measurement sequence switches from 2 to 3 and 4 to 1. If useful data is defined by obtaining the ambient CO2 for this test example, then the stability regions in the plot should be represented by time periods between the vertical solid and dashed lines in the annotated Figure 2 plot shared below. If this is not the case, then the authors need to improve the explanation of this plot, because it would seem undesirable to isolate and use the measurements when CO2 is 0 ppm as suggested in Figure 2 with the second and fourth vertical black lines. Lastly, why do measurement heights for tubes 2 and 4 record less data with respect to time compared to tubes 1 and 3 when measuring 430 ppm of CO2? I would think that achieving an approximately equal number of data points for each measurement height is preferred. Please address this.

Lines 179-181: If the authors were able to improve the set-up to measure at 3.2 seconds compared to 4.9 seconds, then the authors should show the plot with improved timing. The authors can show both figures with a panel dedicated to each that shows contrast. This is strongly encouraged.

Equation 2: The form of this equation uses two assumptions defined in Montagnani et. al. 2018: a homogeneous footprint and that the flux divergence can be ignored. While I have more confidence in the first assumption given that the survey region is a large maize field, I have concerns about neglecting the flux divergence term, especially during transitional periods (i.e., sunrise/sunset) where these calculations seem very important (e.g., Figures 5-7). If the goal is to minimize the error in the total surface budget, then how does neglecting the flux

divergence term contribute to the budget error? The authors should comment on this explicitly and note any caveats. It would also be a good idea to acknowledge this shortcoming and work towards evaluating these assumptions used when calculating storage terms.

Height of vertical measurements: The authors note that the height of measurements was adjusted with the growth cycle during the two-month period. The authors should present a table or a stacked bar graph showing height adjustments to measurements as a function of time so that readers know how often the heights were adjusted and the range of heights that were used in this analysis. Furthermore, it is recommended and strongly encouraged that the storage term calculations be conducted for each set of measurement heights to highlight how the storage terms evolved during the growth cycle. The results in their current form mask the growth cycle, which is no doubt an In important contribution to CO2 and H2O storage, photosynthetic response and evapotranspiration. I suspect that changing the height of measurements does not change the result too appreciably given profile linearity. However, I wonder how the slope of profile linearity changed during the growth cycle. These kinds of analyses would strengthen the paper and support the approach and instrument set-up presented in the paper.

Shading in Figures 5-7: There is nothing in the text discussing the shading in these figures. I assume that the shading represents the variability within the two month period, but nothing is discussed about this shading. I do find it unlikely that there was almost no variability in CO2 during the day over the two month period, and the that the results are almost flat (hovering around 350 ppm). The authors need to add a discussion explaining the shading, and it is strongly encouraged to go back into the data to examine variability at each half-hour averaging interval examined during the diurnal cycle.

Lines 288-290: The authors start out talking about morning and evening transitions, but only comment on the morning transition. This leads to a confusing interpretation if the reader views the statement made at the end of the sentence applicable to both morning and evening transitions when in fact it is not. Please comment on both and make the discussion on respective transitions clear.

Lines 295-297; 301-302: The authors claim that the sensible and latent fluxes exhibit similar characteristics. They do not.

Lines 302-303: The statement "…followed by a rapid decrease and negative storage until 2000 UTC" does not seem to support what is seen in Figure 7b. Sure, there was a sharp decline, but what followed was a fair amount of positive (accumulation) and negative (depletion) storage between this time period that does not lend support to this statement. The variability was large. Please revise messaging.

Lines 304: I believe the authors can avoid the statement "presently unexplained" by studying Equation (2) and and examining Figure 6. This is where the slopes in the profile collapse and an inflection is observed (i.e., concentrations begin to decrease). So the time rate of change of concentrations using the discretized form in Equation (2) should help you form a hypothesis, especially since this is during a period where the stable boundary layer forms. It should also be noted that while the peak in Figure 7b stands out during 2000 UTC, it is not considerably larger than some of the peaks and troughs between 900 UTC and 2000 UTC that reach magnitudes close to 2.5 W/m2.

Lines 303-305: Negligible? Visually I see statistically negative values during the night. The statistically negative latent storage (or depletion) follows a drop in water vapor concentration. The mechanisms for evapotranspiration are minimized and the uptake of moisture into the atmosphere near the surface is reversed as temperatures cool and condense at the surface.

The authors sort of allude to this in lines 272-277, but fail to link these results when discussing Figure 7.

Lines 301-313: Discussion in this paragraph is filled with erroneous statements and broad remarks. Dive deeper into the analysis of this figure and make the interpretation clearer and stronger.

Conclusions: The key findings are not summarized nor are caveats and shortcomings of the approach laid out. The conclusions would also benefit from statements related to future work.

Minor Comments:

Line 49: Insert “the” between “improve” and “understanding”

Line 56: can remove “, distributed”

Lines 72-73: Comment on “loss of low- or high-frequency flux components”: What does this mean exactly? Was a filter applied? Resolution limitation? Was it the technique used? Please elaborate and add a reference if possible

Line 73: Remove “etc."

Lines 80-81: Perhaps instead say “Quantifying storage terms is challenging because measurements are required within and above the canopy”. The current sentence is awkwardly phrased.

Lines 107-108: remove “in their series of field experiments”

Line 110: Remove comma between “interest” and “within”. There are a lot of other examples of misplaced commas.

Line 115: replaced “and” with “as” before “well”.

Lines 120-121 and Lines 154-155: In the first set of lines the authors remark on cycling through all heights every minute while the second set of lines indicates every 30 seconds. Which is it?

Line 195: change “variables” to “measurements”

Line 204: Add “in Figure 4” between “map” and “represents”

Lines 206-207: How did the authors account for the slope of terrain on sonic anemometer wind measurements since the plot indicates the circle is in 5-9% grade? Was the sonic anemometer orientated horizontally or was post processing done?

Line 207: How does the soil type factor into interpreting the results in this analysis? Soil type is important for moisture retention, capacity, and subsurface transport.

Line 218: move “be” between “necessarily” and “site-specific” before “necessarily”

Line 226: Be clear about Δz_i. Is it the separation distance between measurements? If so, then the thickness for the bottom measurement would be between the surface and the measurement height (0.11 m).

Line 227: I'm guessing that "N" equals 4, but this not stated explicitly.

Line 237: "assumptions made elsewhere" should be backed up by citations.

Line 241: "with that aloft"? Or "with air aloft" or "with the overlying atmosphere"

Line 245: can replace ", such that" with "as"

Paragraphs in lines 240-246 and 247-257 share a lot of redundant information

Lines 252: Question on the sentence beginning with "Soil respiration". Are you referring to the entire diurnal trend, day portion or night portion? The sentence that preceded this one focused on the pattern at night, and as such, leads to confusion when interpreting the "Soil respiration" sentence.

Line 253: It is remarked that wind speeds decreased at night. Statistically, this may be true, but moderate to strong winds can develop at night which would effect the results and add to the variability observed and not discussed. At a minimum the authors should note that they confirmed the diurnal wind structure. They wouldn't need to show it, just make it clear that this was observed during the two-month survey period.

The authors never discuss the time range that this study was conducted. Please indicate the months that this study took place before introducing Figures 5-7. This should be discussed in Section 3.

Line 265: Replace ", we recorded" with "is"

Line 268: "height increased". The height of what increased?

Line 271: The "profile appears to be *stronger* than" is not an appropriate description of the profile. I think what the authors meant to say is that the profile is steeper or that there is a more pronounced vertical gradient.

Line 271: Remove "cases" after H2O

Line 272: Remove commas between "for H2O"

Figure 6 caption: You can omit "pattern" after "profile"

Line 286: Change "diurnal average" to "average diurnal".

Line 287: Change "higher values" to "larger magnitudes and more variation". Higher values could mean more positive. As such, I would instead go with the suggested change. Can also remove "as" between "nighttime" and "compared".

Line 295: Sensible heat energy storage was not always lower than latent heat storage. Also, references to "7b" and "7c" are incorrectly placed here and elsewhere in this section.

Lines 293-294: Comment on “During nighttime and morning, these processes were revered, leading to CO2 storage”. During the morning transition the CO2 storage becomes negative (depletion) while during the night storage increases (accumulation). Make this clear instead of just saying “leading to CO2 storage”.

Missed opportunity connecting Figures 7a and 7b: The authors should dig into the inflected results between CO2 storage (7a) and latent energy storage (7b) that occur between 0630 UTC and 0900 UTC during the hours following sunrise. Note the importance of both CO2 and water to photosynthesis which activates at the time where the inflected behavior is observed.

Line 298: change “After that, this” to “Afterwards the”

Lines 299-300: “returning nearly to zero”. Its not actually zero

Line 305: Insert “afternoon into” between “the” and “late nighttime”
Figure 7: There is not a comment about how the data is being processed to form the plots. Is the data vertically integrated? I’m guessing not given the units. If so, what height are the authors choosing or is vertical averaging done? Y-axis label in Figure 7b has a parenthesis that shouldn’t be there. Please place the a-c labeling in a corner and not over the plots.

Line 313: Change from “the accurate estimation of flux” to “the accurate estimation of fluxes”

Figure 7a: How do you explain the large variation between negative and positive CO2 at night and virtually no storage/no variability during the day?

Figure quality needs general improvement. Larger fonts, particularly in Figures 5-7 is needed.