Response to

https://doi.org/10.5194/egusphere-2026-144

RC1, Referee #4

Citation: https://doi.org/10.5194/egusphere-2026-144-RC1

This study presents a fast-response, low-cost carbon dioxide sensor for flux measurements. It is based on approximately five months of field observations and includes comparisons with a standard eddy covariance system. The sensor's performance is thoroughly evaluated, and it appears to offer a cost-effective alternative for field CO2 flux measurements. However, a few major issues should be addressed before the manuscript can be considered for publication in AMT.

We are grateful for useful comments and recommendation. Our responses to comments and corrections are found below, along with changes to the manuscript.

Major Comments:

1. Clarification of technical details: As this is primarily a technical paper intended for users of CO_2 flux measurements, I think some methodological aspects need to be explained more clearly in the manuscript itself. In particular, it is not sufficiently clear what improvements were introduced with the software update and how these changes affected the sensor performance. A more explicit description would be very helpful. Similarly, the autocalibration procedure should be described in more detail. At present, it is difficult to understand how this procedure is implemented in practice and how it affects the long-term behaviour of the sensor. Related to this, it would also be useful if the authors could comment on the temporal evolution of sensor drift. For example, does the drift increase approximately linearly with time, or does it follow a different pattern?

   The prototype development involved series of minor software updates trying to improve the instrument's performance and characteristics. However, the software update on 21 July 2022 improved the sensor performance significantly, eliminating the signal jumps after each autocalibration.

   Prior to this software upgrade, the auto-calibration was performed according to the following algorithm. The signals were measured with 5 Hz until temperature of the cell changed certain threshold (around 0.2º C). Then autocalibration was applied by measurement a reference signal at 1 Hz for 1 sec; after such calibration the CO_2 reading was adjusted to the reference signal. Then measurements continued again with 5 Hz frequency.

The drift was primarily a function of temperature and time. However, we could not establish well-behaving functional relationship on temperature and therefore performed linear correction between the calibrations in the earlier phases of the prototype development.

With the software update on 21 July 2022, the signal jumps occurring with reference autocalibrations disappeared. The instruments software was modified to perform step-wise corrections for the drift between the calibrations against the reference signal using internal signals from instruments components. Continuous corrections to the signal based on such drift predictions virtually removed discontinuities in signal after each autocalibration.

Due to built-in autocalibration against the reference signal the long-term signal drifts were avoided. At the end of the field experiments the prototype's calibration was tested in Vasiala's lab and no drifting was detected.

We have added additional explanation to Sect. 2.2.

2. Response time and wind conditions: The approach used to estimate the sensor response time would benefit from some further clarification. As I understand it, the estimate is derived from data collected under unstable atmospheric conditions and within a relatively narrow wind speed range of 2.5–3.5 m s^{-1}.This raises the question of whether the derived response time is partly influenced by the prevailing wind or turbulence conditions. If so, the authors should discuss to what extent the estimated response time may depend on wind speed or atmospheric state more generally. Would the same response time be obtained under calmer conditions, or under stronger turbulence? Some discussion of the robustness of this estimate would strengthen the paper.

   Low-pass filtering of the system is mainly due to frequency response of the analyser (+tube damping in case of H_2O), and thus the property of the instrument and not turbulence as such. The unstable conditions were chosen to use the measurements with higher flux magnitude in order to obtain better results. This is a common practice in response time estimation (e.g. Aubinet et al. 2008). In addition, the limited wind speed interval was used to use observations from similar conditions as the spectra are usually localized with non-dimensional frequency (the non-dimensional frequency is proportional to height and inversely proportional to wind speed, i.e. spectra are shifted in frequency space in proportion to wind speed). In summary, the conditions were selected to form an ensemble of similar observations.

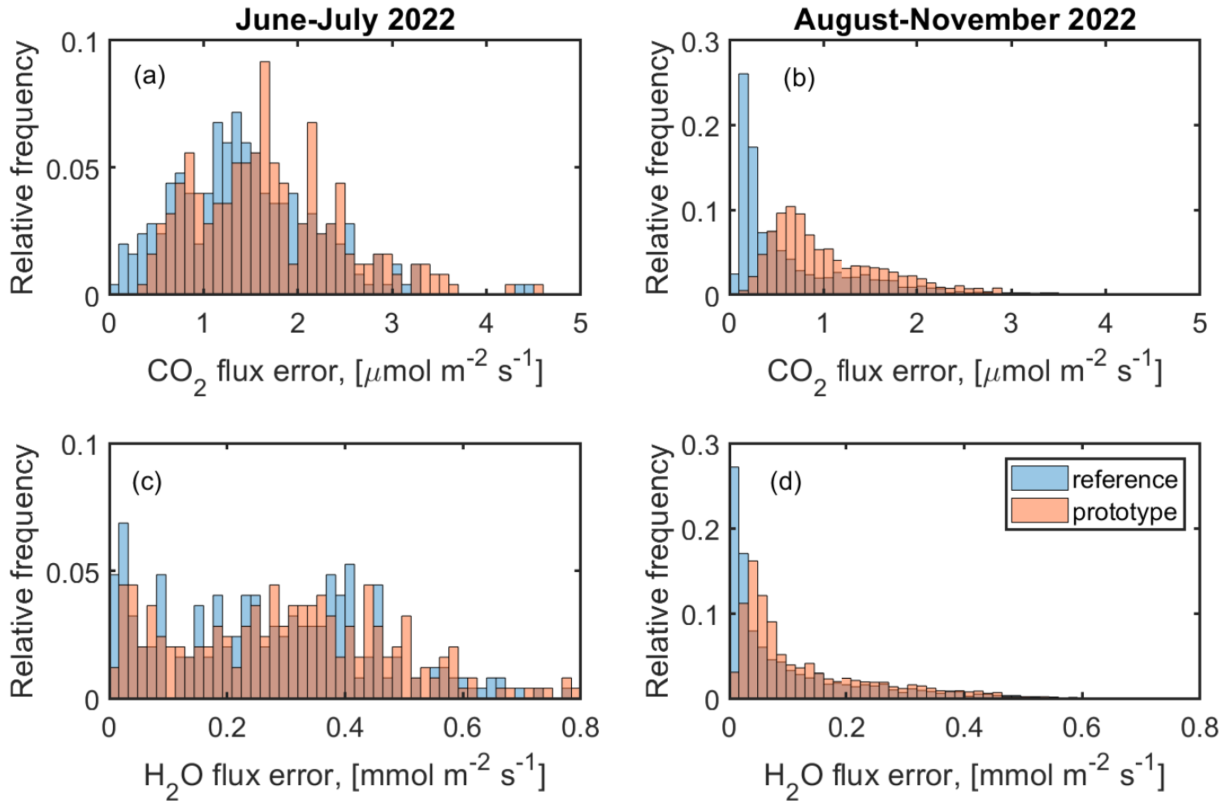
   We added additional explanations in Sect. 3.1.

3. Uncertainty in weak flux measurements: I also feel that the manuscript should discuss more carefully the uncertainty associated with very weak CO_2 fluxes. For example, in Figure 6 the H_2O fluxes show a relatively smooth decline from July to September. The August H_2O fluxes are weaker than in July, but they are still clearly visible and remain higher than those in September. In contrast, the CO_2 fluxes over the same period remain close to zero. This makes me wonder how reliably the

sensor can quantify very small CO_2 fluxes. A similar concern arises from Figure 7. If one focuses on the August data only, excluding the period from 21 July 2022 to the end of July 2022, the agreement between prototype and reference appears rather weak for small CO_2 fluxes, especially in the range of about –5 to 5 µmol m^{-2} s^{-1}. This suggests that the uncertainty may be substantial in the near-zero range. I would therefore encourage the authors to discuss more explicitly the reliability and uncertainty of the sensor under weak-flux conditions.

The random noise of the instrument was provided in Fig. 5 and how it translates to flux uncertainty in Sect. 3.1. However, total flux random uncertainty is larger than the uncertainty originating from the instrumental noise; it is contributed also by stochastic nature of turbulence. In general, the random flux uncertainty is proportional to flux magnitude. However, in near-zero exchange conditions the relative flux uncertainty (i.e. the random error divided by the flux magnitude) becomes large. We calculated the flux random uncertainties of both systems, see Fig. below. The histograms of the total random flux uncertainty for June-July period, when higher absolute flux values prevailed, did not differ essentially for the prototype and the reference systems. However, during August-November period, the distributions for the reference system peaked at smaller values. This effect can be attributed to the impact of the higher signal noise of the prototype system. We have estimated the random uncertainty of the CO_2 flux approx. 0.24 µmol m^{-2} s^{-1}. This corresponded to the noise std of 1 ppm. However, during earlier phases of the field testing the noise level was roughly higher by a factor of 2. We agree with the reviewer that the noise level at initial phases of the field measurements introduced significant flux random uncertainty that limits accurate detection of small fluxes. However, the noise level obtained towards the end of the field experiments (after the software update on 20 September 2022) is a good compromise in our opinion. Also, the random uncertainty does not impact accurate measurement of averages over longer periods.

We included the discussion of the random uncertainties of fluxes in more detail in the revised MS, Sect 3.1 and the figure in SI Fig. S7



Other Comments:

In Figure 4, sensible heat flux is used as an indicator of atmospheric stability. I wonder whether momentum flux or friction velocity (u*) might be more appropriate for this purpose. At least, the choice of sensible heat flux should be justified more clearly.

In general, intention was to select unstable conditions as under these conditions co-spectra are known not to depend on stability parameter in the ASL. The criterion for heat flux > 25 W m^{-2} was chosen to avoid near-zero sensible heat fluxes because the co-spectral estimates become uncertain under low flux conditions and transfer function is obtained by dividing the CO_2 co-spectrum with the temperature co-spectrum. Thus, the threshold was chosen to avoid division with near-zero values, which would lead to higher uncertainty in calculated results.

We have covered this topic in the response to the comment 2 above

It would be useful to include some practical recommendations for long-term operation and maintenance. Based on the results shown, the system appears to require relatively frequent updates, and some guidance for future users would add value to the paper.

The latest software updates improved the analyzer performance and after development completion no frequent updates would have been needed. The intention was to complete development with fully production-ready product which would satisfy the criteria set for eddy covariance measurements, including robustness of maintenance.

The prototype sensor operates at 5 Hz, whereas the reference eddy-covariance system typically operates at 10 Hz. The manuscript should discuss how this difference in sampling frequency may affect the flux calculation and the comparison between the two systems. In particular, could the lower sampling rate lead to a bias or loss of information?

Lower sampling rate is less important than the frequency response of the analyzer. The frequency of signal sampling as such is related to the impact of disjunct sampling on calculated fluxes. If the measurements are instant values of the true signal, but sampling is done in intervals determined by sampling frequency, then calculated fluxes are not affected by systematic bias.

It is shown that sampling rate has no influence on the expected mean value of an eddy-covariance flux estimate. The random uncertainty in the flux estimate, however, increases with decreasing sampling rate. This sampling induced uncertainty is comparable with the natural random uncertainty in the flux estimate due to the stochastic nature of turbulence. It is shown that the sampling induced uncertainty only becomes important when the sampling interval is larger than the integral time scale of the flux time series (see, Bosveld and Beljaars, 2001; also Rinne et. al., 2000; Turnispeed et al., 2009). In terms of sampling interval influence on calculated fluxes, the reduction of sampling rate from 10Hz to 5Hz has essentially no impact. Therefore, we concentrated on the analyses of the sensor frequency response. The frequency response of the analyser was determined to be 0.2 seconds

We added relevant discussion into Sect. 4.

Technical Corrections:

Line 16: I would suggest avoiding the word “promising”, which sounds slightly informal in this context.

Thank you for suggestion, the sentence was removed.

Line 137: Please clarify which lag time was used for the CO_2 flux calculation.

We used fixed lag time of 0.6 and 0.3 sec for the prototype and reference systems, respectively, added to the MS.

Line 147: Delete the extra word “for”.

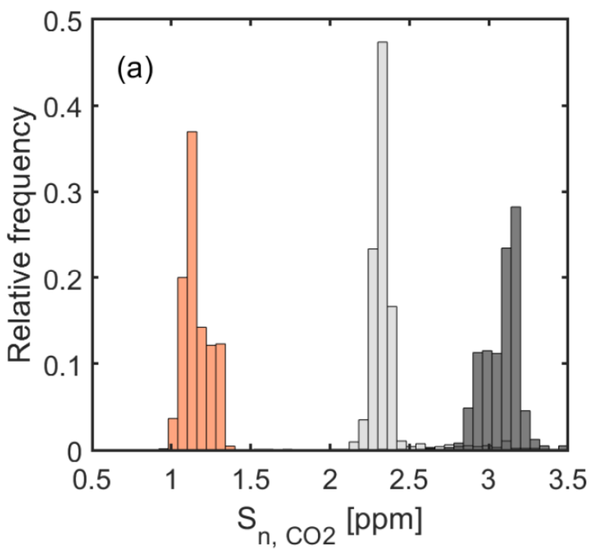
Deleted in the revised MS, thank you.

Line 213: Please clarify whether this refers to r or R^2, and use the appropriate notation consistently.

Fig. 7 shows the correlation coefficients i.e. R. We have used mistakenly in text the R values and saying those were R^2. We changed the figure to include R^2 values and used the numbers consistently in the revised MS.

Figure 5: It would help readability to use slightly more distinct colours or symbols for the different periods.

Thank you for the suggestion. We denote different periods with different colors in the updated figure of the revised MS. The figure below shows observations from the beginning of measurements till the software update on 21 July 2022 with light grey, from then on till 20 September with dark grey and from then till the end of campaign with orange.



Referencies

Aubinet, M., Vesala, T., Papale, D., 2008, A Practical Guide to Measurement and Data Analysis. Publisher: Springer.

Bosveld, F.C. , Beljaars, A.C.M. , 2001. The impact of sampling rate on eddy-covariance flux estimates, *Agricultural and Forest Meteorology*. https://doi.org/10.1016/S0168-1923(01)00257-X.

Rinne, H.J.I., Delany, A.C., Greenberg, J.P., and Guenther, A.B., 2000. A true eddy accumulation system for trace gas fluxes using disjunct eddy sampling method. *J. Geophys. Res., 105, NO. D20.*

Turnipseed, A.A., Pressley, S.N, Karl, T, Lamb, B, Nemitz, E., Allwine, E., Cooper, W.A., Shertz, S., and Guenther, A.B., 2009. The use of disjunct eddy sampling methods for the determination of ecosystem level fluxes of trace gases. *Atmos. Chem. Phys.*, https://doi.org/10.5194/acp-9-981-2009.

RC2, Referee #2

Citation: https://doi.org/10.5194/egusphere-2026-144-RC2

This manuscript describes the development and evaluation of a low-cost CO_2/H_2O sensor for eddy covariance applications, which is particularly relevant given the growing demand for scalable carbon flux measurements. The results of this sensor are in agreement with the chosen reference system. Before publication, some clarification should be required as follows:

We are grateful for useful comments and recommendation. Our responses to your comments are found below, along with changes to the manuscript.

1. Impact of software updates on results: The paper describes some updates during the field campaign, particularly a major change on July 21. More clarification should be given to the software update and autocalibration and how that affected performance. Would this be a reliable method for a more long term use or how does drift affect over long periods of time?

We have explained the major software update and autocalibration topics in the response to the referee comments #1 and repeat it here.

The prototype development involved series of minor software updates trying to improve the instrument's performance and characteristics. However, the software update on 21 July 2022 improved the sensor performance significantly, eliminating the signal jumps after each autocalibration.

Prior to this software upgrade, the auto-calibration was performed according to the following algorithm. The signals were measured with 5 Hz until temperature of the cell changed certain threshold (around 0.2^{o} C). Then autocalibration was applied by measurement a reference signal at 1 Hz for 1 sec; after such calibration the CO_2 reading was adjusted to the reference signal. Then measurements continued again with 5 Hz frequency.

The drift was primarily a function of temperature and time. However, we could not establish well-behaving functional relationship on temperature and therefore performed linear correction between the calibrations in the earlier phases of the prototype development.

With the software update on 21 July 2022, the signal jumps occurring with reference autocalibrations disappeared. The instruments software was modified to perform step-wise corrections for the drift between the calibrations against the reference signal using internal signals from instruments components. Continuous corrections to the signal based on such drift predictions virtually removed discontinuities in signal after each autocalibration.

Due to built-in autocalibration against the reference signal the long-term signal drifts were avoided. At the end of the field experiments the prototype's calibration was tested in Vasiala's lab and no drifting was detected.

It is also important to note that in EC application the CO_2 analyzer does not need to measure absolute concentrations correctly but rather concentration changes (fluctuations) up to a few tens of minutes (the averaging time of the flux).

We have added additional explanation to Sect. 2.2.

2. Differences in the prototype and reference system: As stated in the paper, the prototype and reference systems are installed at different heights and separated horizontally. Additionally, the prototype operates at 5 Hz, compared to 10 Hz for the reference system. Clarification should be given in the manuscript of how these changes impact the data between the prototype and the reference. For example, how does the separation affect flux differences?

The effect of sampling rate was discussed in the response to RC1 and also in Sect. 4 of the revised MS.

In general, the observation height has impact in terms of turbulence properties and also on the field of view (the footprint) of the measurements. Turbulence spectra scale with measurement height and wind speed; in general, the lower observation level implies shift of spectra to higher frequencies. While reviewing the questions and the manuscript, we discovered a mistaken statement that the systems were installed at different heights. Both systems were installed at the same height of about 2.9 m. The prototype analyzer was located at a horizontal boom at 2.15 m. We will correct this information in the revised MS. The results will not be affected.

Thus, both systems measured the fluxes from similarly-sized footprints covered with same surface vegetation. The horizontal separation has an effect that the systems do not sample exactly the same turbulence realization (turbulence records at some horizontal distance are not fully correlated) and therefore the results can differ due to statistical uncertainty. This is also explained in the Sect 3.2 of the revised MS in relation to discussion of Fig. 7.

3. Line 16 says, "The results at the end of the paper are very promising..." and then proceeds to give the results. This sentence is not needed. The results should prove the promise, it does not need to be stated.

Thank you for the comment, we removed the sentence.

4. Line 151: "indicated the presence of noise at high frequencies", consider specifying the frequency range

The noise in CO_2 signal can be observed as the upward sloping power spectral densities (multiplied with frequency) starting from normalized frequency around 0.5. Will added this to the MS.

5. The figures are in a blue/green color palette. Consider switching to a blue/red palette instead for readability.

We changed the palette to blue/orange + adding light and dark grey, when necessary. Blue/orange/black should be the colors easiest to separate for people with color vision deficiencies.

RC3, Referee #1

Citation: https://doi.org/10.5194/egusphere-2026-144-RC3

This work designed a low-cost CO2 sensor to measure CO2 flux. The system has advantages in fast response and low noise. The good performance of the low-cost sensor was validated by simultaneous long-term field measurement of CO2 flux using a scientific reference eddy covariance setup. This study fits well within the scope of AMT and have importance implications in greenhouse gas measurements and the evaluation of their climate effects. The following concerns should be addressed before it can be accepted for publication.

We are grateful for useful comments and recommendation. Our responses to your comments are found below, along with changes to the manuscript.

Specific comments:

Lines 83-85: The fund information should go to the section “Acknowledgements”.

Thank you, the fund was already mentioned in Acknowledgements, we removed mentioning of the fund in this section.

Lines 111-115: The software updates have a significant impact on the performance of the sensor. The authors should elaborate on what the key update is.

The prototype development involved series of minor software updates trying to improve the instrument’s performance and characteristics. However, the software update on 21 July 2022 improved the sensor performance significantly, eliminating the signal jumps after each autocalibration.

Prior to this software upgrade, the auto-calibration was performed according to the following algorithm. The signals were measured with 5 Hz until temperature of the cell changed certain threshold (around 0.2^{o} C). Then autocalibration was applied by measurement a reference signal at 1 Hz for 1 sec; after such calibration the CO_2 reading was adjusted to the reference signal. Then measurements continued again with 5 Hz frequency.

The drift was primarily a function of temperature and time. However, we could not establish well-behaving functional relationship on temperature and therefore performed linear correction between the calibrations in the earlier phases of the prototype development.

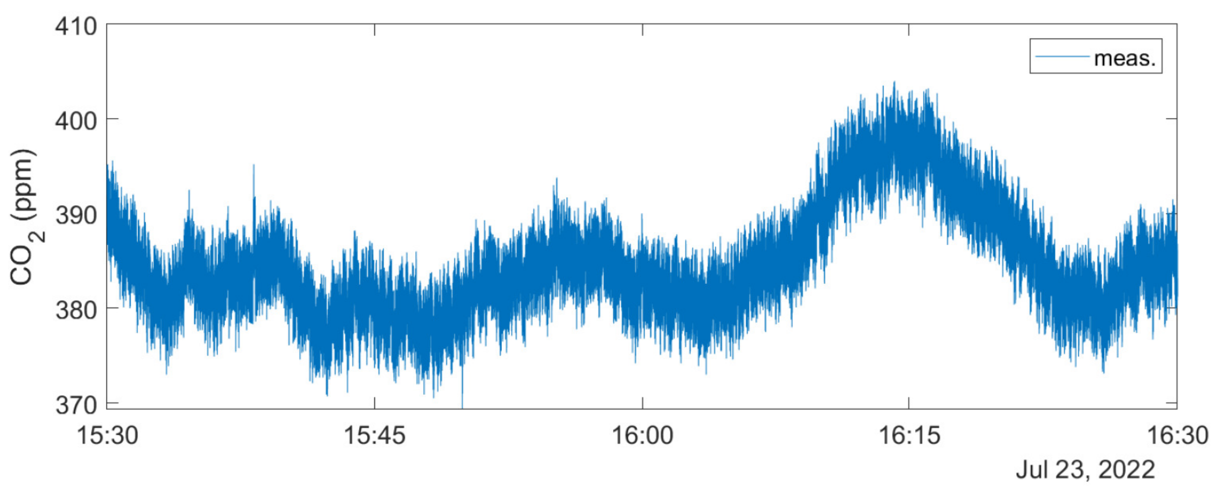
With the software update on 21 July 2022, the signal jumps occurring with reference autocalibrations disappeared. The instruments software was modified to perform step-wise corrections for the drift between the calibrations against the reference signal using internal signals from instruments components. Continuous corrections to the signal based on such drift predictions virtually removed discontinuities in signal after each autocalibration.

Due to built-in autocalibration against the reference signal the long-term signal drifts were avoided. At the end of the field experiments the prototype's calibration was tested in Vasiala's lab and no drifting was detected.

We have added additional explanation to Sect. 2.2.

Lines 120-124: The CO2 mixing ratios before and after 21 July should be given to have a better understanding of how the software update shapes the measurement of CO2.

The "jumping behavior" of CO_2 signal was shown on Fig. 2 of the MS. Below we show two examples (on 23 July and 27 July 15:30 till 16:30) of the measured CO_2 mixing ratios after the software update on 21 July 2022. Discontinuous jumps of the signal have disappeared. We added these plots to the SI (Fig. S2) of the revise MS.



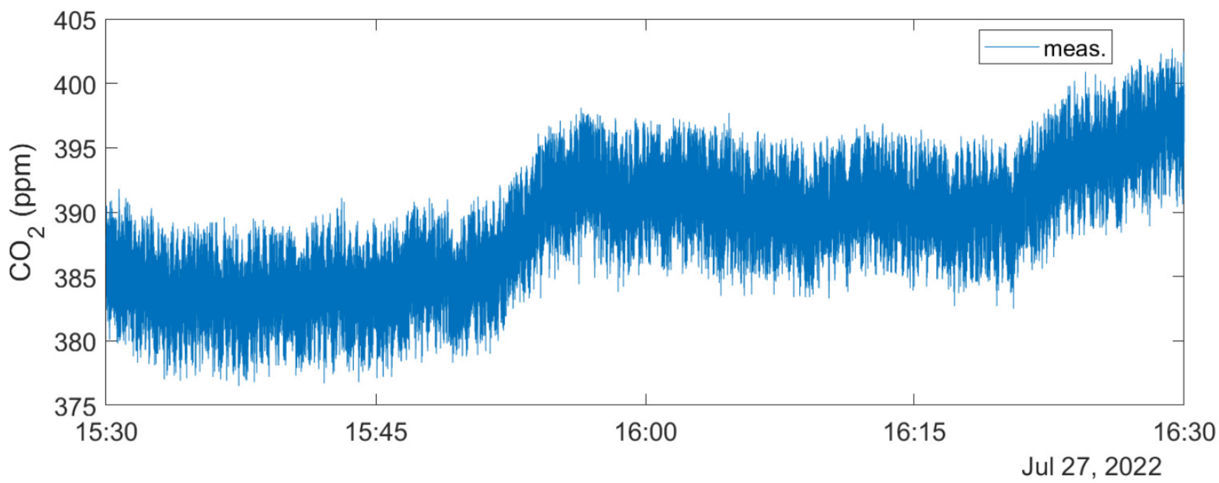


Figure 6: The current x-axis is difficult to read. I would suggest changing the x-axis to day 1 to 31.

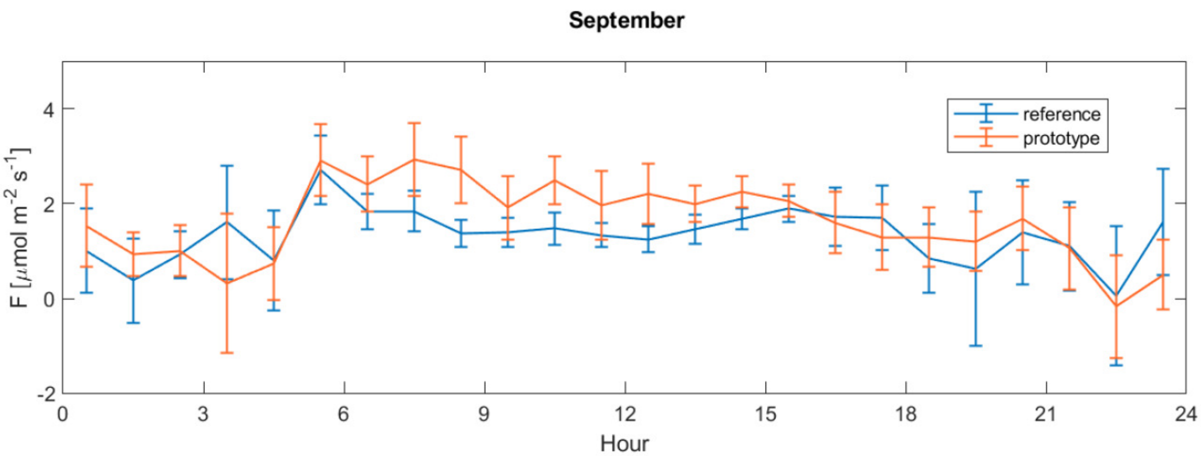
We modified the x-axis in Fig. 6 in the revised MS.

Figure 8: The difference in CO2 flux in September is also relatively large. What is the reason?

In September the average flux magnitudes are relatively small. At 30 min averaging level, however, relatively large variation of fluxes occurs, which can be observed in the results of both systems (Fig S3 of the revised MS).

Below we present closer look at the September comparison. Instead of SE (standard error) representing the error bars on Fig. 8, we use here 2 x SE (of the average for each hourly bin). Twice the standard error would represent the confidence intervals at 95% probability. We observe that two curves differ more than the error bars only for a few hours in September, thus making it difficult to decide if they are different. However, within time interval from 7 am till about 3 pm the difference tends to be systematic. As a possible reason for this could be density fluctuations effect, if not properly accounted for. However, similar pattern is not observed in previous and following months (see August and October in Fig. 8). We have applied the same correction routines throughout the period. Thus, currently we cannot confirm if the difference in September between the two system results is real and significant. Also, we are not aware of system malfunctioning during this specific period.

We have included the plot below in SI as Fig. S8 and included relevant discussion in Sect. 3.2.


September
F [μmol m^{-2} s^{-1}]
Hour
reference
prototype
4
2
0
-2
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21
24

RC4, Referee #3

Citation: https://doi.org/10.5194/egusphere-2026-144-RC4

The manuscript by Rannik et al. studies the performance of a fast-response prototype CO_2 and H_2O vapor sensor that was implemented in a low-cost eddy covariance (EC) system. Its performance was then compared against simultaneous observations from a commercial high-performance EC system in an agricultural field in 2022. Correlations (R^2) greater than 0.92 were reported between the prototype and reference setup's CO_2 and H_2O fluxes. The CO_2 sensor also had a noise level of ~1 ppm and around ~0.1 ppth for H_2O at a 5 Hz sampling rate.

I think this paper would be of interest to readers of AMT but only after adequate attention to the following comments:

We are grateful for useful comments and recommendations. Our responses to your comments and revision suggestions are found below, along with changes to the manuscript.

MAJOR REVISIONS

- Authors are obligated to declare any competing interests at the end of the manuscript to clarify their relationship to Vaisala and if any of the authors are being compensated by the company (whether financial or other support).

Pirkko Väkimies and Hilkka Heiskari-Tuohiniemi were affiliated and compensated by Vaisala Oyj. Mika Korkiakoski was funded by the Finnish Research Impact Foundation (project “Breathing of biosphere – Industrialization of easy-to-use carbon dioxide exchange measurements”) during the project in 2022. We added this information.

- Line 85: "would be cheaper than the currently commercially available ones": Cheaper by how much? Do not cite exact prices, but is it cheaper by 25%, 50%, etc. when taking into account all the parts? The title of the paper has 'low cost' in its title, so there should be some indication of the prototype's low cost in case someone wanted to build it themselves or replicate this study.

Target was to achieve approximately 50-65% lower price than the commercially available ones. We added this to the revised MS (beginning of Sect. 2).

- Section 2.1: I assume that the blueprints of the prototype sensor are likely intellectual property of Vaisala, but the current description of the low cost sensor is gravely lacking. For example, what is the operating principle (e.g., NDIR, etc.)? What spectral lines are being used? How can it achieve a fast time response (5 Hz) at a fraction of the cost? As a reviewer, I need to assess how novel this sensor is compared to other CO_2 sensors already available, but I have no way of doing so given the lack of technical information about the sensor itself.

The prototype utilized a thermopile detector and had internal temperature and pressure sensors capable of compensating for temperature, pressure and water vapour dilution. This was stated in the MS (line 90). Thermopile detector with optimal design of other hardware components enabled the price to reduce significantly while achieving performance characteristics suitable for EC measurements. We have shown in the manuscript that the first order response time of the sensor was around 0.2 sec.

- Lines 93-94: "The main feature of the instrument was its built-in autocalibration capability": Authors need to describe this capability/functionality in greater detail since it impacts the sensor's performance in the study. The authors even admit in Lines 122-123 that "well-designed autocalibration functionality appeared to be a critical feature of the prototype with the thermopile detector," so it's important to describe the technical aspects of this autocalibration feature for the reader.

We have explained the major software update and autocalibration topics in the response to the referee comments #1 and repeat it here.

The prototype development involved series of minor software updates trying to improve the instrument's performance and characteristics. However, the software update on 21 July 2022 improved the sensor performance significantly, eliminating the signal jumps after each autocalibration.

Prior to this software upgrade, the auto-calibration was performed according to the following algorithm. The signals were measured with 5 Hz until temperature of the cell changed certain threshold (around 0.2^o C). Then autocalibration was applied by measurement a reference signal at 1 Hz for 1 sec; after such calibration the CO_2 reading was adjusted to the reference signal. Then measurements continued again with 5 Hz frequency.

The drift was primarily a function of temperature and time. However, we could not establish well-behaving functional relationship on temperature and therefore performed linear correction between the calibrations in the earlier phases of the prototype development.

With the software update on 21 July 2022, the signal jumps occurring with reference autocalibrations disappeared. The instruments software was modified to perform step-wise corrections for the drift between the calibrations against the reference signal using internal signals from instruments components. Continuous corrections to the signal based on such drift predictions virtually removed discontinuities in signal after each autocalibration.

Due to built-in autocalibration against the reference signal the long-term signal drifts were avoided. At the end of the field experiments the prototype's calibration was tested in Vasiala's lab and no drifting was detected.

We have added additional explanation to Sect. 2.2.

- Line 110 (and elsewhere): Enigmatic software upgrades and maintenance are referred to throughout the manuscript. What exactly was being changed in these software upgrades or maintenance periods that fixed sensor drift and generally improved the sensor's performance? Such knowledge would be helpful for readers to be aware of if they are trying to build their own setup.

The prototype as well as its internal software were under development during the test measurements. Therefore, the software updates were aimed to improve instruments performance; most of the upgrades were dealing with minor improvements that did not significantly impact the results. The main upgrade is explained in the earlier response above.

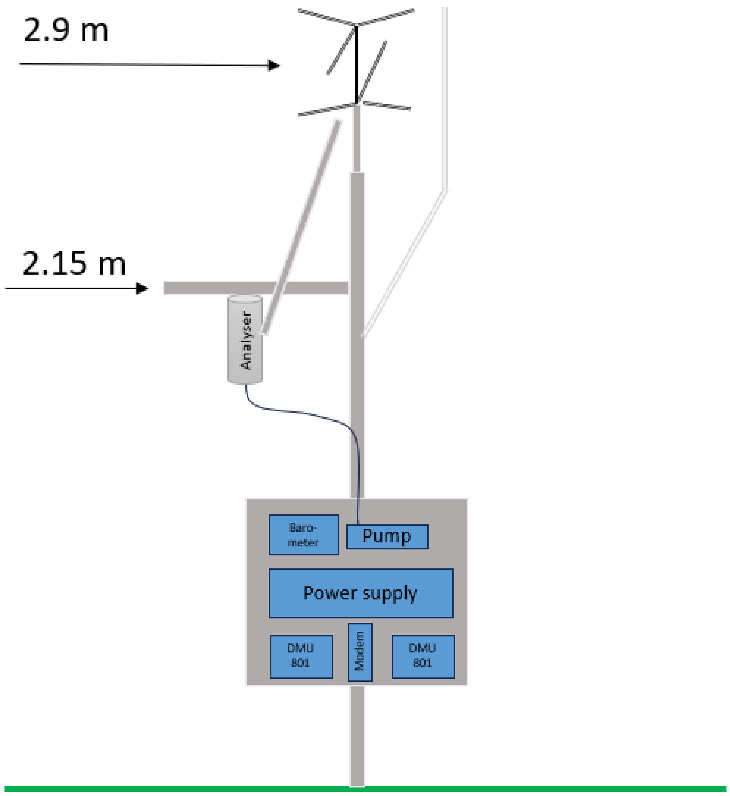
- Lines 283-286: Remove paragraph concerning Vaisala's market analysis and investment strategy since it does not belong in a scientific manuscript. However, the instrument's lack of availability does raise concerns about reproducibility if the prototype instrument cannot be obtained by anyone other than the authors. What technical aspects can be shared with the community about the prototype so that others may independently reproduce the results of this paper?

We have shared that the cost optimality of the prototype was achieved mostly by using a thermopile detector with optimal design of other hardware components. We have described also the general idea of the autocalibration procedure, however, specific implementation and algorithms are proprietary and cannot be disclosed.

MINOR REVISIONS

- Figure 1: I appreciate the included photos, but a detailed schematic/diagram of the prototype EC setup would be of greater benefit to the reader.

We have included a simple schematic diagram in SI (Fig. S1) and include it below. The rod rising on the level of the anemometer is a 4G antenna. The prototype gas analyzer is located at a horizontal bar at 2.15 m from the ground. A heated sampling line of 70 cm length was used to suck air from close the anemometer to the prototype analyzer. Instrumental box contained the pump, Vaisala digital barometer, power supply unit with fuses, Vaisala DMU801 data loggers and an internet modem.



- Title: Authors may want to consider adding 'fast-response' to the manuscript's title since there are many CO_2 sensors available on the market (e.g., some using NDIR) but most are inadequate for EC applications.

Thank you, we have added “fast-response” to the title.

- Lines 22-24: "There has been discussion... For example, in agriculture...": Please cite studies that back up this claim.

We have added citation as follows:

There has been discussion of forming carbon (C) markets where countries and companies can trade C credits depending on their GHG emission needs (e.g., Wetterberg et al., 2025; Bin et al., 2025). For example, in agriculture, interest in carbon farming has increased, where the aim is to store C in soil (Pang et al., 2025; Baumber et al., 2024).

Baumber, A., Cross, R., Ampt, P., Waters, C., Ringbauer, J., Bowdler, I., Scott, A., Gordon, L., Sutton, A., Metternicht, G., Soil-based carbon farming: Opportunities for collaboration, Journal of Rural Studies, https://doi.org/10.1016/j.jrurstud.2024.103268, 2024.

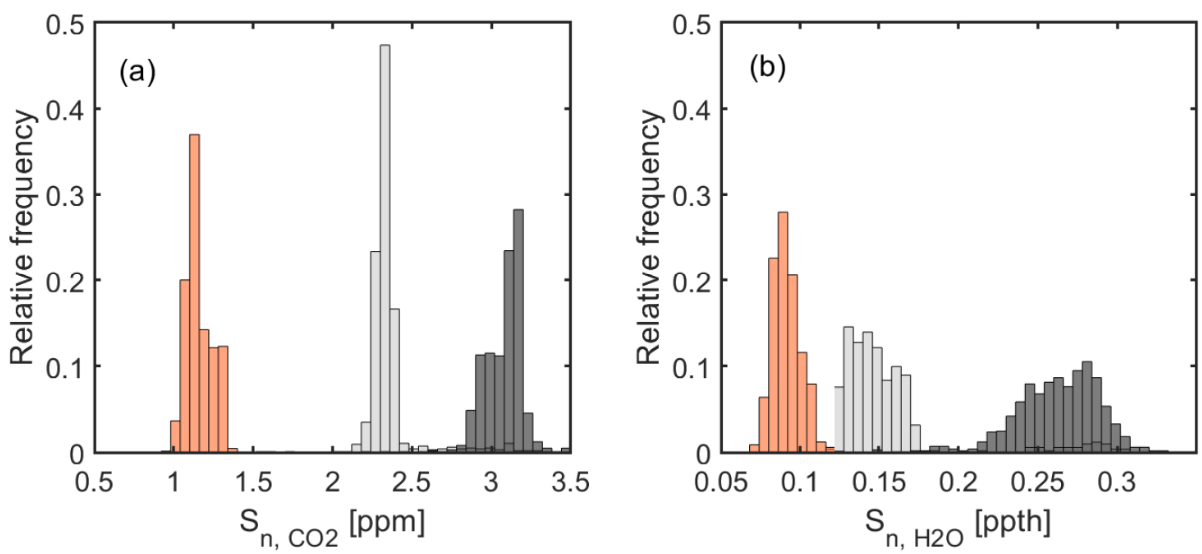
Bin, X., Weifang, J., Research on the impact of carbon trading on enterprises' green technology innovation, Energy Policy, https://doi.org/10.1016/j.enpol.2024.114436, 2025.

Pang, Y., Zhang, M., Zhong, H., Cevin, T., Sun, C., Zhang, S., Li, X., Dai, J., Liu, C., Zhang, C., Current Progress and Future Trends in Carbon Sources and Sinks in Farmland Ecosystems: A Bibliometric Analysis (2002-2023), Biology,14(4), 365, https://doi.org/10.3390/biology14040365, 2025.

Wetterberg, K., Lanzi, E., and Gómez, N., Exploring governments' efforts to shape carbon credit markets: Possible actions to enhance integrity, OECD Environment Working Papers No. 263, https://dx.doi.org/10.1787/0bafe9af-en, 2025.

- Fig 5: Suggest giving each sub-period its own color so that it is easier to differentiate between the time periods in the figure. It also helps denote that the instrument was operating differently during those periods.

Thank you for the suggestion. We denote different periods with different colors in the updated figure of the revised MS. The figure below shows observations from the beginning of measurements till the software update on 21 July 2022 with light grey, from then on till 20 September with dark grey and from then till the end of campaign with orange.



- Lines 181-182: "An increase in signal noise... for H_2O." What caused this to happen on 21 July? Is it from the software update? If so, what was changed in the update that caused the noise to get worse?

Yes, the signal noise was elevated after the software upgrade on 21 July 2022. At that stage the main challenge for the prototype was the signal jumps after autocalibrations. With the software upgrade the algorithm was improved (see answer to the autocalibration related question under major revision) at the cost of incorporating more noise from different hardware components. Thus, the signal stability, i.e. eliminating virtually the signal jumps after autocalibrations, was achieved at the cost of higher noise in signal. With the later improvements (software upgrade on 20 September 2022) the signal noise achieved lower levels again.

We comment this in Sect 3.1 of the revised MS.

- Figure 8 caption: Authors need to explicitly comment in the caption for Fig 8(b, g, l) that the magnitude for the reference and prototype fluxes is lower from 21 July since the fluxes for the latter half of July are lower than the first half. It is presumably not due to the software update from 21 July onwards.

Yes, the reason for lower fluxes during second half of July are decreasing flux magnitudes during that period. We have added a sentence into the caption of Fig. 8:

“The magnitude of the fluxes after 21 July is significantly lower for CO_2 (b) due to decline in carbon assimilation towards the end of the month.”

TECHNICAL REVISIONS

- Lines 12-13: Recommend removing phrase "present a case study... industrial and academic competencies" from the sentence since affiliations are already listed for the authors. The emphasis should always be on the prototype setup and not on the company that produced it. As mentioned before, put any competing interests or conflict of interest in a separate section.

Thank you, the phrase was removed.

- Lines 147-148: There seems to be a missing or extra word in the sentence as it is currently written.

Excess “for” was deleted.